Cavendish

The Experimental Life

Revised Second Edition

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The Experimental Life

Revised Second Edition

Christa Jungnickel and Russell McCormmach

Studies 7

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Editorial Team: Lindy Divarci, Georg Pflanz, Bendix Düker, Caroline Frank, Beatrice Hermann, Beatrice Hilke

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Scientific Board

for
Robert Deltete
and
Marvin Sparks

Nothing is more fantastic, ultimately, than precision.
– Robbe-Grillet on Kafka

Contents

List of Illustrations

Preface and Acknowledgements

Fifteen years have passed since my wife Christa Jungnickel and I published *Cavendish, the Experimental Life.*[1] In the meantime, I have published two books about Cavendish. The first, *Speculative Truth,* is about Cavendish's work in theoretical physics.[2] He is known primarily as an experimentalist but he was no less accomplished as a theorist, and this book helps correct a partial view of his work. Cavendish exhibited some of the most baffling behaviors in the history of science, which are taken up in the second book, *The Personality of Henry Cavendish.*[3] We only touched on this subject in our biography, and to that extent it was incomplete.

Cavendish was a "great man with extraordinary singularities," his colleague Humphry Davy observed. The new edition of the biography brings a fuller understanding to what was "great" about Cavendish, and as well to what was "extraordinary" about his personality, and by clarifying the connections between the two, it more fully integrates his personality and his work. The new materials and perspectives complete the biography of Cavendish. As with any revision, this one also makes improvements of the usual kind: it corrects flaws in the original, sharpens discussions, introduces new materials, and improves the writing throughout.

We express our gratitude to persons who read part or all of the original book in manuscript and to others who in one form or another have given encouragement, advice, and help: Mark Bonthrone, William H. Brock, I. Bernard Cohen, Arthur L. Donovan, Mordechai Feingold, John Gascoigne, Charles C. Gillispie, Jan Golinski, Sean Goodlett, Peter Harman, Patrick Henry, Ingrid Hofmaster, Sean Kissane, Carmen Mayer-Robin, David Philip Miller, Betty Mohr, Joseph F. Mulligan, Rosemarie Ostler, Jean Luc Robin, Richard Sorrenson, and Mary Lou Sumberg. We have been aided in our study of Cavendish by many archivists. Here we give special thanks to the archivists in charge of the Devonshire Collection at Chatsworth, which contains Henry Cavendish's scientific manuscripts: Peter Day, Charles Nobel, Michael Pearman, Andrew Peppitt James Towe, and Thomas Wragg.

Russell McCormmach
March 25, 2015

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[1] Jungnickel und McCormmach (1999).

[2] McCormmach (2004).

[3] McCormmach (2014).

Introduction: The Problem of Cavendish

Henry Cavendish, 1731–1810, has been described in superlatives, which are often of praise or wonder. On matters of intellect and fortune, he has been called "the wisest of the rich and the richest of the wise."[1] In his dedication to science, he has been compared with "the most austere anchorites," who were "not more faithful to their vows."[2] Concerning his ability, Humphry Davy called him the greatest English scientist since Newton.[3] Superlatives of another kind have been used as well. Cavendish was a man of a "most reserved disposition," of a "degree bordering on disease."[4] Cavendish was, to be sure, one of the best scientists, one of the richest men of the realm, a scion of one of the most powerful aristocratic families, a man of strange behaviors, and a scientific fanatic.

Until we looked closely at the life of his father, Lord Charles Cavendish, 1704–83, we did not have a firm understanding of Henry's life. Coming from a family of politicians, Lord Charles predictably entered public life as a politician. While he was active in politics, he also pursued science as a side interest, at a certain point leaving politics and becoming more involved with science. His direction was continued by his son Henry, who made a complete life within science. The scientific calling of Charles and Henry Cavendish found a congenial home in the Royal Society of London.

By the time Henry joined his father in the Royal Society, it had been in existence for a century. A legacy of the Scientific Revolution, it retained a measure of its revolutionary potential in English society, as shown by the lives of Charles and Henry Cavendish. Charles found support in the Royal Society for his move from a traditional aristocratic career in politics to the uncommon life of an aristocrat *seriously* engaged in science; his son Henry began where his father left off, on a course of scientific experiment, observation, and theory in close association with the Royal Society. In its membership, the Royal Society was selective, but in its understanding of science, it offered an acceptable path of *public* service, which was taken by our branch of the Cavendish family. Owing to the Society, the lives of Charles and Henry Cavendish were, in part, public careers in science.

Charles Cavendish's attention to the affairs of the Royal Society was extraordinary by any standard: with the exception of the officers, no member of the Society gave more of his time than he did. Having made no major discovery, he has entered the history of science as, at most, a footnote, but in a biography of the discoverer Henry Cavendish, he holds an important place. Lionel Trilling's stricture that "every man's biography is to be understood in relation to his father"[5] may not be a practical guide for all biographers, but for biographers of Henry Cavendish, it is indispensable. We have written this book as a biography of father and son.

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[1] J.B. Biot (1813, 272–273, on 273).

[2] Georges Cuvier (1961, 227–238, on 236).

[3] Humphry Davy, quoted in John Davy (1836, 222).

[4] Henry Brougham (1845, 444). Thomas Thomson (1830–1831, 1:337).

[5] Lionel Trilling (1949, 15).

Historians of science know of Cavendishes earlier than Charles. Richard Cavendish, one of the Cavendishes of Suffolk from whom the Devonshires descended, was an Elizabethan politician and scholar—for twenty-eight years he was a student at Cambridge and Oxford—who translated Euclid into English and wrote poems including (and in spirit foreshadowing our Henry Cavendish) *No Joy Comparable to a Quiet Minde*, which begins, "In loathsome race pursued by slippery life [...]."[6] The namesake of one of our Cavendishes, Charles Cavendish, a seventeenth-century politician, solved mathematical problems, performed experiments, improved telescopes, and corresponded with inventors of world systems. This Charles was "small and deformed," but he had a beautiful mind. In a time of violent controversy, he advocated cooperation as the way to truth, subscribing to Descartes' maxim, "to strive to vanquish myself rather than fortune and to change my desires rather than the order of the world."[7] This Charles and his older brother William, duke of Newcastle, who had a scientific laboratory, were friends of Thomas Hobbes, the philosopher who envisioned a state of war of each against all, and who also wrote the most original scientific philosophy in England. Hobbes tutored and influenced three generations of the other main branch of the Cavendishes, the earls, not yet dukes, of Devonshire. He moved in the great houses of the Cavendishes, Chatsworth and Hardwick Hall, and in the Cavendish library he found the true university that he had not found in Oxford.[8]

By Charles Cavendish's time, science was not exclusively a male preserve: Margaret Cavendish, duchess of Newcastle, wrote a number of good popular books on the microscope and other scientific subjects. She demanded to be admitted as a visitor to the Royal Society, and in general she behaved in such an original and independent manner that she, the first scientific lady in England, was known as "Mad Madge."[9] In Henry Cavendish's time, Margaret Cavendish Bentinct, duchess of Portland, also of the Newcastle branch of the family, was a correspondent of Rousseau and a passionate collector; at her death, the sale of her natural history collection took thirty-eight days.[10] As if handing on the torch, in the year Henry Cavendish was born, Charles Boyle, earl of Orrery died. Nephew of the first duke of Devonshire, this earl was related to the great seventeenth century chemist Robert Boyle. The same earl gave his name to George Graham's machine to show the motions of the heavenly bodies, the "orrery," the embodiment of the scientific worldview of our Cavendishes.[11] Other early scientifically inclined Cavendishes include three notable fellows of the Royal Society: the third earl of Devonshire; the first duke of Devonshire, who was tutored by the secretary of the Royal Society Henry Oldenburg; and the youngest son of the first duke, Lord James Cavendish.[12] English aristocrats who actively pursued science were few, and if a titled family was destined to distinguish itself in the eighteenth century, Cavendish had a claim to be that family.

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[6] Henry Cavendish's forebear also wrote, "The enemies of Grace, do lurke under the prayse of Nature." "Cavendish, Richard," *Dictionary of National Biography*, ed. L. Stephen and S. Lee, 22 vols. (New York, Macmillan 1909) 3:1266–67. Hereafter *DNB*, 1st ed. The second edition, in 60 vols., edited by H.C.G. Matthew and B. Harrison, published by Oxford Universiy Press in 2004 is denoted by *DNB*. This work being cited throughout the book, the full reference is not repeated in each chapter.

[7] Jacquot (1952, 13, 187, 191).

[8] "Hobbes, Thomas," *DNB* 1st ed. 6:444–51, on 444–45.

[9] Meyer (1955, 14).

[10] Allen (1976, 29).

[11] "Boyle, Charles, Fourth Earl of Orrery," *DNB* 1st ed. 2:1017.

[12] A. Rupert Hall (1974, 10:200).

Our Cavendishes descended from two revolutions, one political and the other scientific. The Cavendish who became the first duke of Devonshire took a leading part in the revolution of 1688–89, which deposed one king, James, and replaced him with another, William. Referred to as the "Glorious Revolution," this change may not seem all that revolutionary when compared with subsequent political upheavals,[13] but to the British of the eighteenth century, it was the epitome of a major change in human affairs. Joseph Priestley, a scientific colleague of Henry Cavendish's and a friend of revolutions, said that before the French and American revolutions, the "revolution under King William [...] had perhaps no parallel in the history of the world." For support he cited the philosopher David Hume's opinion that this revolution "cut off all pretensions to power founded on hereditary right; when a prince was chosen who received the crown on express conditions, and found his authority established on the same bottom with the privileges of the people."[14] For his part in the revolution, Devonshire was honored by the victorious court. In return, he and his descendants, who included Charles, recognized a duty to uphold the revolutionary settlement and to give desirable shape to its aftermath.

Science, which had been an occasional interest of various earlier Cavendishes, became for Charles an alternative to politics. Having served a respectable number of years in Parliament, he redirected his public activities without changing their essential nature and motivation. The Royal Society offered him a worthy setting in which he could continue to exercise his highly developed sense of duty. The evidence of continuity in his life is as undramatic as it is indisputable: he moved his committee work from the House of Commons to the Royal Society. If committees are more often associated with endurance then with high endeavor, they are nevertheless the level of organization in scientific and learned institutions in which necessary tasks get done, and where colleagues get to know one another well and decide who has good judgment and who takes responsibility.[15] Owing in part to Charles' conscientious work as a committeeman and councillor of the Royal Society, he was one of the most important men of science in London. When he turned from assisting in the governing of the nation to assisting in the governing of the national scientific society, he was in middle age. By the time Henry came of age, the alternative lives of politics and science open to a Cavendish were clear, and he could choose between them at the outset.

By the middle of the eighteenth century, the new political notion of revolution as a radical change rather than a cyclic return was applied to science, and with specific reference to Isaac Newton's *Mathematical Principles of Natural Philosophy*, or *Principia*.[16] Almost to the year, the political Revolution of 1688–89 coincided with the publication of that book, an event which has often been singled out as a culmination of the Scientific Revolution. The *Principia* was the single most important book of science for Henry Cavendish on several levels. It was a treatise on mechanics, a compendium of useful theorems developed from

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[13] If the revolution is not viewed as "glorious" in the "Whig" sense of the term, as the "harbinger of Liberal England," its significance may be seen to have an "even greater global magnitude." D. Hoak and M. Feingold (1996, vii–viii).

[14] On this point, see Joseph Priestley's *Lectures on History and General Policy* (1826). Quoted and discussed in I.B. Cohen (1976, 263–264).

[15] Lewis Thomas, a redoubtable committeeman of science, has remarked in various places on the indispensability and value of committees and on the inescapable disruptiveness of human individuality in the work of committees. For example, in *The Youngest Science: Notes of a Medicine-Watcher* (1983, 171); *The Medusa and the Snai* (1979, 94–98). Although Cavendish served on committees throughout his sixteen years in the House of Commons, we note that his committee work fell off with time.

[16] Cohen (1976, 264).

fundamental laws of matter, force, and motion. It contained the derivation of the law of gravitation, the model for future investigations of other forces of nature. It was a model of another kind, too: how to present scientific work. Most important, it demonstrated that mathematics is as important as experiment in natural philosophy. In classifying papers in the *Philosophical Transactions of the Royal Society of London*, the abridgment of this series placed Cavendish's paper on a mathematical electrical theory together with papers on electrical experiments and instruments under "Electricity, Magnetism, Thermometry," and this under "Mechanical Philosophy" (an alternative to "Natural Philosophy"); Cavendish's paper was not placed under "Mathematics."[17] Cavendish treated many subjects in natural philosophy mathematically, and when he did, he was applying the "mathematical methods of natural philosophy"; at his writing desk as in his laboratory, he worked *in* natural philosophy.

Having made Newton's *Principia* a prominent marker in this introduction, we can envision the brickbats flying. For forty years or longer, historians of science have reacted against the idolatry of Newton, arguing that the eighteenth century should be regarded as a time of originating scientific energies of its own.[18] We concede the point; nevertheless, in following the tracks of Henry Cavendish, we repeatedly encounter Newton. He was educated at Cambridge at the time when Newton's *Principia* dominated the curriculum, and although his greatest contributions to science were experimental, he was also a theorist who grasped the new experimental fields in Newton's "mathematical way."[19] New instruments, apparatus, and experimental techniques were invented in the eighteenth century, but not everything about science had to be invented. In Cavendish's electrical researches, we see that for him the *Principia* was still, after a century, the example of science at its best. For the record, we do not subscribe to the view that science in the eighteenth century consisted of filling in the blanks left by Newton's incomplete natural philosophy.

Today when we speak of *the* Scientific Revolution, we recognize it as a long and complex historical process, one which did not consist solely of a preparation for the mathematical principles of mechanics and the gravitational system of the world as laid down in the *Principia*. Human understanding of the vastly more complicated operations of chemistry and of life underwent major reinterpretations as well, and the subtle art of experiment was enriched by advances in techniques and instruments. That ingenious master of experimental apparatus Robert Hooke was hardly less important than Newton in preparing the way for Charles and Henry Cavendish. The same can be said of that eminent model of experimental persistence and perspicacity, Robert Boyle (who, as an aristocrat working in experimental science and shaping the Royal Society, was a model for Charles and Henry Cavendish in another sense). Newton himself was a great experimental as well as mathematical investigator. Together, the scientific examples of Boyle, Hooke, and Newton and the political settlement of the revolution of 1688–89 go far to make intelligible the remarkable lives of Charles and Henry Cavendish.

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[17] "Contents," *PT*, abr. 13 (1770–76), i–vii, on iv–v. The classification did not use the category "mixed mathematics," a common term then for subjects treated mathematically as opposed to pure mathematics. Like any classification, this one had a rationale, but there is no reason to think that Cavendish considered his researches to belong to different categories of science, only to different methods of natural philosophy.

[18] This by now historiographic commonplace was once fresh, serving as an important corrective; for example, R.W. Home (1979).

[19] Newton's expression, quoted and discussed in Henry Guerlac (1965, 323).

Charles and Henry Cavendish present their biographers with a problem. The practical concerns, and perhaps the private reserve, of the Cavendish family ensured that every scrap of paper having to do with Charles and Henry Cavendish's property was saved, but little that could be regarded as personal. We have Charles Cavendish's business correspondence but not his and his family's private letters, which were in Henry's possession when he died. Henry Cavendish's business correspondence is preserved too, but in his case, we suspect that there may not have been many personal letters. Virginia Woolf approached her biography of Roger Fry with the question, "How can one make a life of six cardboard boxes full of tailors' bills, love letters, and old picture postcards?"[20] The answer is, as she went on to show, that it is possible. Henry Cavendish, whose cardboard boxes contain nothing so personal as even tailors' bills, let alone love letters, presents his biographers with an even harder task. How can they make a life from a record of observations of thermometers and magnetic needles? Once again, we intend to show that it is possible. Cavendish's scientific papers are, in their way, as revealing of his nature as personal letters are of a lover's.

Cavendish's public life was carried out in the Royal Society and other settings where scientific men gathered. His private life was carried out mainly in his laboratory and study, and what he said about it he said primarily in writing, not in speech. Writing can be as impermanent as speech if it is not published, but Cavendish kept what he wrote for fifty years, clearly valuing what he put on paper. Each written report of a scientific observation of his is a record of experience, and as such it is potentially the material of biography. Because Cavendish's life was about science, the trove of scientific manuscripts he left behind is its faithful record, and his life accordingly is one of the best documented lives of the eighteenth century.

When Cavendish died, his unpublished scientific papers passed to his principal heir, Lord George Cavendish. They evidently remained with Lord George's family until his grandson became the seventh duke of Devonshire in 1858, when they were removed to the ancestral house of the Devonshire's, Chatsworth, where they remain.[21] The papers, which consist of experimental and observational memoranda, calculations, and studies in various stages of writing, are substantial, and to Cavendish's biographers an embarrassment of riches, posing hazards of their own. We have tried to heed Henry Adams' advice to biographers, "proportion is everything,"[22] while at the same time we have accepted that Cavendish's life was disproportionate by the usual standards. The distinction between biography and history of science can be fine, and Cavendish's biography calls for a balancing act. We could not have written this book without Cavendish's unpublished scientific papers, and we have relied extensively on them, but at the same time we have tried not to lose a sense of proportion, and with it the man.

A selection of Cavendish's manuscripts has been published, though only one group of them, the electrical, with anything approaching completeness. The electrical manuscripts were examined by a series of experts in that branch of physics, beginning with William Snow Harris, who described them in detail in an "Abstract." They were "more or less confused as to systematic arrangement," not "finished Philosophical Papers," Harris said, but they

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[20] Quoted in Susan Sheets-Pyenson (1990, 399).

[21] *Treasures from Chatsworth.The Devonshire Inheritance.* A Loan Exhibition from the Devonshire Collection, by Permission of the Duke of Devonshire and the Trustees of the Chatsworth Settlement, Organized and Circulated by the International Exhibitions Foundation 1979–1980, (1979–1980, 67).

[22] Quoted in John A. Garraty (1957, 247).

showed that “Mr Cavendish had really anticipated all those great facts in common Electricity which were subsequently made known to the Scientific World through the Investigations and writings of the celebrated Coulomb and other Philosophers.”[23] Primarily to show how much of the modern subject Cavendish had anticipated, Harris included extracts from Cavendish’s papers in a revision of his textbook on electricity.[24] In 1849 on a visit to Harris, William Thomson examined Cavendish’s electrical manuscripts.[25] Concluding that they should be published in their entirety, Thomson together with several other men of science put the case to the duke of Devonshire. In 1874 the duke placed the manuscripts in the hands of the first Cavendish Professor of Experimental Physics, James Clerk Maxwell, who for the next five years repeated Cavendish’s experiments, transcribed the manuscripts, and prepared a densely annotated and nearly complete edition of Cavendish’s unpublished electrical papers together with his published electrical papers.[26] This remarkable edition, *The Electrical Researches of the Honourable Henry Cavendish,* was published in 1879 by Cambridge University Press a few weeks before Maxwell’s death.[27] At about the same time as his electrical manuscripts, Cavendish’s chemical manuscripts came to the attention of the scientific world, in this case in connection with a resurrected priority dispute over the discovery of the composition of water. In defense of Cavendish’s claim, in 1839 Vernon Harcourt appended a selection of Cavendish’s chemical manuscripts to his published presidential address to the British Association for the Advancement of Science. At the time, Harcourt understood that an edition of Cavendish’s papers was being planned.[28] In fact there had been intermittent discussions of such a plan from the time of Cavendish’s death, but for one reason or another it had been put off, as it would continue to be long after Harcourt. In due course, with further delays caused by World War I, in 1921 Cambridge University Press reprinted Maxwell’s edition of the electrical papers and published a new, companion volume containing the rest of Cavendish’s published papers together with a selection of scientific manuscripts from outside the field of electricity, the two volumes appearing as *The Scientific Papers of the Honourable Henry Cavendish, F.R.S.*[29] The selection of manuscripts for inclusion in the companion volume was made by the general editor and chemist Sir Edward Thorpe together with four experts from physics, astronomy, and geology.

There are two book-length biographies of Cavendish in English, both written by chemists. The more recent one is by A.J. Berry, who gives an excellent technical account of Cavendish’s papers.[30] It does not present anything new about Cavendish’s life, in implicit

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[23] William Snow Harris, “Abstract of M.S. Papers by the Hon. H. Cavendish.” This twenty-five page abstract, which describes the contents of twenty packets of manuscripts on electricity and four packets on meteorology, is in the Royal Society, MM.16.125.

[24] William Snow Harris (1854). Wilson (1851, 469). James Clerk Maxwell, Introduction to Henry Cavendish (1879, xl).

[25] S.P. Thomson (1901, 218).

[26] Maxwell’s correspondence in 1873 concerning the Cavendish papers is published in *The Scientific Letters and Papers of James Clerk Maxwell*, Harman (1995, 785–86, 839, 858–59).

[27] Henry Cavendish (1879).

[28] W. Vernon Harcourt (1839, 45). The address is followed by an “Appendix,” 45–68, containing extracts of Cavendish’s papers on heat and chemistry, which in turn is followed by some sixty pages of lithographed facsimiles.

[29] Cavendish, Henry (1921). *The Scientific Papers of the Honourable Henry Cavendish*. 2 vols. Ed. by J.C. Maxwell and E. Thorpe. Cambridge: Cambridge University Press. The subtitle of the first volume, edited by Maxwell and revised by Joseph Larmor, is *Electrical Researches.* The subtitle of the second volume, under E. Thorpe’s general editorship, is *Chemical and Dynamical.* Hereafter, this work is cited as *Sci. Pap.* 1 and 2. Because this book is cited often, the full reference is not repeated in each chapter.

[30] A.J. Berry (1960).

agreement with what the editor-in-chief of the collected papers, Thorpe, said of Cavendish's "personal history": little is known of it, "nor is there much hope now that more may be gleaned," since it is doubtful that "there is much more to learn" about this "singularly uneventful" life.[31]

If ever a biography violated Adams' advice about proportion, it was George Wilson's *The Life of the Hon^{ble} Henry Cavendish*, published in 1851.[32] Cavendish's "life," in the ordinary sense of the word, occupies only two chapters, the first and the fourth, which comprise fifty pages out of a total of nearly 500 pages. The "life" in the *Life* was attached to a book with a different purpose, which was to put to rest the controversy over the discovery of the composition of water. The controversy, which had simmered briefly in Cavendish's lifetime, was fanned to white heat in the middle of the nineteenth century by a French éloge of one of the discoverers James Watt. Dealing almost exclusively with the water controversy, Wilson's account has elements of a detective story, legal drama, and contest of honor, and for all these reasons it is eminently readable. Independently of the controversy, the book is a useful work in the history of chemistry, though it does not seem to have been used that way. What it has been used for is its "life" of Cavendish.

Wilson's biography was undertaken at the request of the Cavendish Society. Founded in 1846, the Society was one of a number of early nineteenth-century subscription printing clubs, this one for chemical works, named after Henry Cavendish no doubt because of the furor going on then.[33] In addition to the water controversy, there was another reason for Wilson's *Life*. In the middle of the nineteenth century, a call went out for biographies of scientists, presumed to be a neglected category of eminent Britons. In 1845 Henry Brougham published biographical sketches of Cavendish and several other scientists in the belief that scientists together with men of letters gave their age "greater glory than the statesmen and warriors."[34] In 1848 the historian of the Royal Society Charles Richard Weld condemned the lack of a biography of the late president of the Society Joseph Banks as a "reproach to scientific England," confident that if Banks had been a military man or a romantic hero, his biography would long since have been written.[35] In 1843 Wilson began collecting materials for a book on the lives of British chemists; although he never published it, he completed three biographical essays intended for it. He said of one of his subjects, William Hyde Wollaston, that if he had been a German, "some patient, painstaking fellow-countryman would long ago have put on record all that could be learned concerning his personal history"; or had he been a Frenchman, "an eloquent Dumas or Arago would have read his *éloge* to the assembled men of science in the French capital." But Wollaston's "fate as an Englishman, is to have his memory preserved (other than by his own works) only by one or two meagre and unauthenticated sketches, which scarcely tell more than that he was born, lived some sixty years, published certain papers, and died." In the book about the life of a chemist he did publish, Cavendish, in 1851, Wilson regretted that "no other European nation has so imperfect a series of biographies of her philosophers, as Britain possesses." There was not even a good biography of Newton, Wilson said, let alone biographies of recent British scientists such as Thomas Young, John Dalton, and Wollaston, and only belatedly was there

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[31] Thorpe, "Introduction," *Sci. Pap.* (2:1–74, on 1).

[32] George Wilson (1851).

[33] W.H. Brock (1978, 604–605).

[34] Brougham (1845, xi).

[35] Charles Richard Weld (1848, 2:116–117).

a biography of Cavendish.[36] That Wilson included a “life” at all in his book on Cavendish would seem to have come from his sympathy with the prevailing desire for biographies of scientists.

When Wilson applied to the Cavendish family for the loan of Henry Cavendish’s manuscripts, he said that he had delayed asking because he understood that Lord Burlington was going to write an account of Cavendish’s discoveries. (The earl of Burlington, we should explain, was an extinct title resurrected as a courtesy title for Henry Cavendish’s heir, Lord George Cavendish, thereafter going to the eldest son of the eldest son of the duke of Devonshire.) This Lord Burlington was the forty-eight-year-old William Cavendish, who would go on to become the seventh duke of Devonshire. Scientifically gifted, as a student at Cambridge he had posted second wrangler in the competitive mathematical examinations and first Smith’s Prizeman, only to return to Cambridge in 1861 to succeed Prince Albert as chancellor. The richest of all the dukes, in 1870 he drew upon his wealth to build a laboratory for experimental physics at Cambridge, where its first professor, Maxwell, would repeat Cavendish’s experiments for his edition of Cavendish’s electrical papers. The laboratory was going to be called the Devonshire Physical Laboratory after the seventh duke, but it was named the Cavendish Laboratory instead, after Henry Cavendish according to one account,[37] though this version of the naming has been called into question.[38] The duke did not write an in-house study of Cavendish’s work after all, but he established one of the world’s great physical laboratories, which bears the name Cavendish.

Wilson told the future duke that he had been studying Cavendish’s works for ten years, that he admired Cavendish’s character, and that he intended to do him justice in the water controversy.[39] He was allowed to see the manuscripts, which proved useful to him in vindicating Cavendish of any wrongdoing in the water controversy, but they did not give him the materials he needed for a “life” of Cavendish. For this purpose, he relied largely on short accounts published in most cases soon after Cavedndish’s death, and on first-hand accounts that he and a colleague obtained from older fellows of the Royal Society and former neighbors of Cavendish’s. The accounts of Cavendish’s death, as Wilson noted, were conflicting, as we might expect, given that the words and actions of a person approaching the end were believed to be revealing, but Wilson found the accounts of the rest of his life to be largely consistent. We do too, even as we recognize that they were anecdotal and depended on recollections of events that occurred at least forty years earlier. Guided by these accounts, Wilson tried to understand Cavendish, to “become for the time Cavendish, and think as he thought, and do as he did,” but as he closed on his subject, he conflated it with the remorse he felt on devoting so much time and effort to “so small a matter.” Like all of his past efforts, this effort Wilson saw as “bleak and dark,” and the image of the man he distilled from the accounts of Cavendish corresponds.[40]

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[36] George Wilson (1862b, 254). Wilson (1851, 15).

[37] John Pearson (1983, 214).

[38] Peter Harman, editor of Maxwell’s papers, has kindly informed us that he has found no documentation of the switch in name from Devonshire to Cavendish. He thinks it is likely that the name Cavendish stands for the family. Personal communication. J.D. Crowther too does not think that Maxwell regarded the laboratory as a memorial to Henry Cavendish (1974, 35).

[39] George Wilson to Lord Burlington, 15 Mar. 1850, Lancashire Record Office, Miscellaneous Letters, DDCa 22/19/5.

[40] The quotations are from a letter Wilson wrote at the time, included in his sister’s memoir, Jessie Aitken Wilson (1862b, 340–41).

Wilson kept his promise to Burlington. He portrayed Cavendish as a man of exemplary probity, but there is more to character than honesty, and Wilson did not admire much of what he saw. A deeply religious man, Wilson was then contemplating writing a *Religio Chemici* modeled after Sir Thomas Browne's *Religio Medici*, and in the year following the publication of his biography of Cavendish, he published a biography of the physician John Reid, a man of "Courage, Hope, and Faith," whom he greatly admired. Wilson tried to penetrate to where Cavendish's courage, hope, and faith lay, only to discover that Cavendish was a "man without a heart."[41] In the *Life*, Wilson said that Cavendish was "passionless," "only a cold, clear Intelligence." Wilson is entitled to his image of Cavendish, but we should point out that in addition to being Wilson's conviction, that image is a mid-nineteenth-century Romantic cliché, echoing Keats's Apollonius, whose cold mathematical philosophy denied the imagination by subjecting the rainbow and other mysteries to its "rule and line," conquering them and emptying them of their charm. We have dwelt this long on Wilson's biography because it is the source of the standard interpretation. Wilson accomplished what few biographers do: he made his subject vivid and still after over 150 years compelling. We admire Wilson's biography of Cavendish, and in our own, we make extensive use of his insights and of the accounts of Cavendish on which he based his portrait. But we have consulted a much wider range of sources, and our portrait naturally differs. In addition, times have changed and biographies with them.

We can, it would seem, agree on the appearance of Henry Cavendish, since there is only one picture of him, an ink-and-wash sketch, from which Wilson had an engraving made for his biography. Cavendish is shown walking with something of a slouch, possibly an inherited trait, since a "peculiar awkwardness of gait is universally seen" in the Cavendishes.[42] The sketch shows him in a rumpled coat and wearing a long wig, both long out of date. Thomas Young, who knew Cavendish in his later years, said that he always dressed in the same way, presumably as in this picture.[43] Young also described Cavendish as tall and thin, which is where agreement ends; another contemporary, the chemist Thomas Thomson, described Cavendish as "rather thick" and his neck as "rather short."[44] The circumstances under which the sketch was made make for one of the better stories about Cavendish, and one there is no reason to doubt. When earlier he had been approached to sit for a portrait, Cavendish had given a blunt refusal. William Alexander, a draftsman from the China embassy, succeeded by subterfuge; with the help of a member John Barrow, he was invited as a guest to the Royal Society Club, at which Cavendish dined once a week. As advised, Alexander sat at one end of the table close to the peg on which Cavendish invariably hung his gray-green (or faded violet, by another account) coat and three cornered hat, both of which Alexander surreptitiously sketched. He then sketched Cavendish's profile, which he later inserted between the hat and coat in the finished portrait. Cavendish, of course, was not shown it, but people who knew him were, and they recognized him. The artist left the sketch at the British Museum, where Wilson obtained it.[45] It is a wonderful sketch, and part of the wonder is that it ever came into being in the first place. Because of the scarcity of

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[41] Ibid. (338, 342–43). Wilson completed several chapters of his projected book on chemistry and religion. They were brought out after his death in a volume of essays bearing the title *Religio chemici*, note 39 above.

[42] "Joseph Farington's Anecdotes of Walpole, 1793–1797," in Horace Walpole (1937–1983, 15:316–317).

[43] Thomas Young, "Life of Henry Cavendish," *Encyclopaedia Britannica*, Supplement (1816–1824), in *Sci. Pap.* (1:435–447, on 444).

[44] Thomson (1830–1831, 1:339).

[45] John Barrow (1849, 146–147).

personal sources, we have had to rely upon other kinds of evidence in preparing Cavendish's biography. To form our image of him, to draw the human face between the three-cornered hat and the crumpled greatcoat, we have placed him in all of the settings in which we know he appeared.

"I desire" was one of Cavendish's favorite expressions. His life was filled with desire, and to a greater extent than most persons, what he desired he could have. For he was perfectly placed: born an aristocrat when the aristocracy was in high tide, he could expect his desires to be taken seriously. Because he was not a peer, he escaped the time-consuming duties, rituals, and displays; he was free to choose inherently more rewarding pursuits, while at the same time he could feel as confident of his place in society as if he were a peer. (His diffident behavior in particular social settings was an entirely different matter.) What he desired more than anything else, we know, was to understand the natural world. Given his enviable position, he could separate the rewards of scientific work from those of society at large, which were in any event given to him without having to desire them, an advantage which lent his life its peculiar direction and intensity.

This biography opens in the 1680s, when science began to dominate educated thought in Western Europe,[46] and it ends just over a century later, at the beginning of the nineteenth century. It was a time of impressive advances in scientific techniques and beginnings of new major fields of investigation. Charles Cavendish took up challenging problems in them, and his son Henry explored them systematically. In terms of the Cavendish family, the period covered by this biography begins when the rooms of the great Cavendish country house, Chatsworth, resounded with the sound of the pugnacious first duke of Devonshire's clanking sword, and it ends when the tone of those same rooms was set by the Proustian languor of the fifth duke of Devonshire. Where the first duke saw a world to conquer, the fifth duke saw an already conquered world in which his comfort was well secured. The fifth duke was no fool. He recognized that his relative, Henry Cavendish, lived partly in a different world, though he may not have recognized it as a new world to conquer, demanding of Henry what had been demanded of the first duke, hard work. (By "conquer," in the borrowed sense, we mean to understand the workings of nature, ruled by the authority of natural laws.) The fifth duke got it nearly right when he ordered his wife Georgiana, duchess of Devonshire to stay away from Henry Cavendish's laboratory on the grounds that "he is not a gentleman—he *works*."[47] Henry Cavendish and before him his father belonged to what Sir Benjamin Brodie called the "working men of science."[48] In this biography, we show what it meant for two gentlemen, first Lord Charles Cavendish and then the Honorable Henry Cavendish, to work in science.[49]

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[46] Margaret C. Jacob (1988, 105).

[47] Bickley (1911, 202).

[48] In reference to the membership of the Royal Society in Henry Cavendish's day: Benjamin Brody to Charles Richard Weld, 7 Apr. 1848, quoted in Weld (1848, 2:153).

[49] *Work* in the setting of professional science in the next century is the theme of Christa Jungnickel and Russell McCormmach (1986).

Part I: Lord Charles Cavendish

Chapter 1
The Dukes

Repeated rejections by the aristocracy of attempts by the crown to increase its power culminated in the Revolution of 1688–89, which made the state subservient to the landed aristocracy.[1] This class was not separated off from the rest of society by legal privileges. By and large, it was well intentioned and able to rise above self-interest, though it believed that only it was capable of governing the country, and its well of sympathy for the poor was shallow. It included a wide varety of individuals, most of whom were admirable enough, though there were always some who pursued their pleasures with evident disregard for the other orders of society. The historical judgment is that the aristocracy acted responsibly overall. In the century following the Revolution, it recognized that its advantages came with an obligation to undertake unpaid and often demanding work in the interest of the common good. Its example of public service assured its survival at the same time as it contributed to the governing of the nation. This tradition implicitly contained the direction that Lord Charles and Henry Cavendish took with their lives.

In the spring of 1691, two young English aristocrats on a grand tour of the Continent met in Venice and apparently liked one another well enough to begin a correspondence after they parted.[2] The older of the two was Henry de Grey, Lord Ruthyn, then not quite twenty, the younger, the nineteen-year-old William, Lord Cavendish. Forty years later, in 1731, they were to become the grandfathers of Henry Cavendish, although William did not live long enough to know of this grandson.

The eldest sons of propertied English earls, the two young men, accompanied by tutors and servants, met as seasoned travelers despite their youth. William Cavendish had already been abroad for over two years, Henry de Grey for over a year.[3] William was on his way to Rome, Henry returning from there. Both of them were no doubt acquiring the rudiments of their later great interest in the arts and architecture, but letters about their travels do not show any youthful ardor for the beauties of Italy, Switzerland, or Holland. In Rome, William Cavendish and his younger brother Henry did "little or nothing … that was worth giving your Lordship and account of."[4] From Padula, Frankfurt, and The Hague, they reported seeing friends or missing them, as they crisscrossed the Continent, but said not a word about the

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[1] M.L. Bush (1984, 12).

[2] William Cavendish to Henry de Grey, 30 May/9 June 1691 and 23 Dec. 1691, Bedfordshire Record Office, Wrest Park Collection, L 30/8/14/1–2.

[3] One of William Cavendish's first stops on the Continent was Brussels. From there he wrote to his mother-in-law, Lady Russell, that he was about to continue on his tour, and she approved, "for to live well in the world; 'tis for certain most necessary to know the world well." Rachel Russell (1793, 415–416). Henry de Grey, as "Lord Ruthven," had been issued a pass on 16 April 1690 "to travel abroad for purposes of study." George Edward Cokayne (1982, 3:176–178).

[4] William Cavendish to Henry de Grey, 7/19 May 1691, Bedfordshire Record Office, Wrest Park Collection, L 30/8/21/1.

finer things of classical civilization these young English barbarians had been sent abroad to experience.

What did interest them was the war threatening between England (and its allies) and France, and the dynastic quarrels that were giving rise to it. The war might affect their travel plans as it did Henry de Grey's, but, more important, it was to be fought to secure the rights to power and property of certain European ruling families; that was the usual purpose of wars then, and understandably a matter of concern to aristocrats of high rank like young Cavendish and Grey. "The Elector of Brandenburg has declared, that he will fulfill the Promise he made to the Duke of Lorraine, at the siege of Bonn, *to maintain the interests of his children* and to contribute to their restoration. The Emperor and all the allys have declared the same thing," William Cavendish reported to Henry de Grey in the summer of 1691.[5] Concern for the dynastic interests of the ruling family that an aristocrat chose to ally himself with was very much a concern for the interests of his own family. That was why William Cavendish was ready to risk his life in battle in 1691 and why his father had risked his life only three years earlier to secure the interests in England of the Protestant branch of the Stuarts.

In 1688, William Cavendish's father, the earl of Devonshire, had joined six other English aristocrats in the risky business of inviting William of Orange to the British throne, even though that throne was then rightfully occupied by James II and could someday be legally claimed by James's son, who had just been born. If their scheme of deposing James had misfired, they might have suffered the fate of traitors. But luck was with them, and with the succession of William and his Stuart wife, Mary, to the crown, the earl ensured abundantly the survival of the Cavendish family in political power and in the enjoyment of their property. In 1691, in the spring in which William and Henry met in Venice, the earl of Devonshire outshone "most of the Princes," including the Elector of Brandenburg, with his "magnificent" establishment at the Royal Congress at The Hague, to which he had accompanied King William as lord steward. Three years later, in 1694, the royal couple rewarded his services by raising the earl to duke of Devonshire, the highest rank short of royalty.[6]

The Cavendishes rose to their title relatively quickly, in not much more than a century, and they prepared for it by a steady accumulation of landed property until they were among the richest landowners in England. Along the way, they used some of their money to buy first a baronetcy and then an earldom when the political shifts of the seventeenth century from monarchy to commonwealth and back prompted the granting of royal favors. They remained loyal to the Stuarts—being prudent enough to make their peace with the commonwealth as well—until under Charles II such loyalty was no longer in their financial and political interest.[7]

Kent

If the dynastic concern of the Cavendishes was to further strengthen their newly found hold on the top rung of the social ladder, that of the Greys was to reclaim their former footing. The Greys had been earls of Kent since the fifteenth century, Henry de Grey's father the eleventh of the line. But Henry's branch of the family had succeeded to the title and the

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[5] Cavendish to de Grey, 30 May/9 June 1691. Italics added.

[6] John Pearson (1983, 68–71), Francis Bickley (1911, 170–174).

[7] Pearson (1983, 61).

estate only in the middle of the seventeenth century, beginning with a country rector with a very large family who was too poor and too old to take his seat in the House of Lords. His successor, Henry's grandfather, did enter politics, but on the wrong side as it turned out, adopting the cause of parliament against the king. After the restoration of the Stuarts, the Greys prudently kept their distance from court and parliament. In any case, their most pressing need was still to secure their estate and finances; at court or in government in those troubled years, they would only have risked making enemies or spending money that they could not afford. Taking big chances, as the earl of Devonshire had on behalf of William of Orange, was acceptable to a prudent man only if he had power, and power then derived from landed property. Nor would they take chances with the life of their heir. Instructing Henry to leave Holland before the king arrived there for his campaign, Henry's father wrote to him: "It would be expected you should go to the campaign with him, and not to do it would be took ill both from your father and you." So Henry traveled on to Geneva, safely away from the king, and from there, against his cautious parents' wishes, into Italy.[8]

For ten years after his return from the Continent in 1691, Henry de Grey lived the life of a well-to-do private gentleman, in 1695 marrying Jemima Crewe (Fig. 1.2), daughter of the English politician Thomas Crewe, 2d baron Crewe. Taking up neither of the usual two occupations of young aristocrats, the military or parliament, Henry's public life began almost simultaneously with the reign of Queen Anne. At her coronation, Henry's father carried one of the swords of state; four months later, in August of 1702, his father died suddenly in the middle of a game of bowls, leaving Henry his heir, on his way to the House of Lords as earl of Kent. A nonpolitical man, Kent stood for neither power nor party, unlike his friend Devonshire, who sought and acquired political power and served the Whig cause with a fierce loyalty. Kent's political career had only this in common with Devonshire's, high ambition for his family, which in Kent's case took the form of self-interested maneuvering at court. For his long, faithful services at court, Anne elevated him to duke (Fig. 1.1).

If Henry de Grey had any brothers, they died young, for soon the love and hope of his family focused on him. He responded by developing into an affectionate young man, good-natured and easy-going. Once he had a family of his own, his concern for his wives—after his first wife died, he remarried—and his children was reflected in their letters to him, full of warmth and appreciation. He was not especially gifted in anything, but he had sufficient intelligence and curiosity to inform himself on a wide range of subjects, including science, as his substantial library attests. He had sufficient vanity to aspire to important positions at court, lacking only the drive to work for such positions by seeking political power. "A quiet mind is better than to embroil myself among the knaves and fools about either Church or State," he wrote in a moment of disappointment.[9] He sought offices in the courtier's way, by gaining favor with influential people and then using his connections to request honors and positions. The offices he accepted were administrative rather than political, requiring abilities well within his reach, drawing on skills he already exercised in the running of his estate. He attended the House of Lords dutifully even after he came to dislike the burden in his middle years.[10] He displayed the same levelheaded estimate of his abilities in his later years, when his chief occupation came to be his estate at Wrest Park; on its agriculture

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[8] Joyce Godber (1982, 2–3).

[9] Henry de Grey, duke of Kent to Prior, 26 July 1710, quoted in Ragnhild Hatton (1978, 121).

[10] "Memoir of the Family of De Grey," Bedfordshire Record Office, Wrest Park Collection, L 31/114/22, 23, vol. 2, 99.

and its gardens, he informed himself thoroughly, and he planned and directed the work on his properties with considerable and lasting success (Figs. 1.4–1.5). His enemies at court—political opponents who wanted the positions he held, or rivals for royal favors—gave Henry de Grey the name "The Bug";[11] they meant to ridicule him for being pompous and proud, for pretending to quality, but their view of him must be admitted to have some truth to it. A good looking man, he spent the money necessary to cut a fine figure; his annual clothes bills ran higher than those of his wife and several daughters combined, not only while he held high office at court and needed expensive formal apparel, but long before, as a young man about town. On his tomb, he had himself sculpted wearing a Roman toga over a strong, muscular body, his curly hair cropped close to his head, resembling in face and attire Laurent Delvaux's statue of George I, undeniably betraying a certain vanity. A large family portrait painted about five years before his death shows him to be, on the contrary, a relatively short, slender man whose simple velvet coat is decorated only with what appears to be the garter and ribbon. Far from posing as the patriarch in his own home, he has yielded center stage to his mother-in-law, the countess of Portland, who was governess of the royal children; he stands rather meekly by her side, receiving from her a cup of tea (Fig. 1.3).[12] His pride lay in his "ancient and noble" family as he called it, which he hoped, in vain, as it turned out, to continue through his five sons. Not one of them survived him. He achieved a dukedom for his family in 1710, but he ended without an heir to inherit it, reduced to looking forward to its extinction with his death. All that remained for him to do was to build an ostentatious marble mausoleum, which although pompous, also evoked his struggle against so much disappointed hope.

The duke of Kent's two sons Anthony earl of Harold and the duke's namesake Henry de Grey were tutored by Roger Cotes at Trinity College, Cambridge. Cotes was then Plumian Professor of Astronomy and Experimental Philosophy and the most gifted of Newton's disciples. When Cotes died at age thirty-three, Newton said, "Had Cotes lived we might have known something!"[13] As it happened, we do not know what his pupils might have done either, for they too died young, Anthony at twenty-seven, and Henry at twenty. It is noteworthy that Henry Cavendish's two uncles on his mother's side had a connection with a great mathematician who was active in founding the Newtonian school in Cambridge. In due time Cavendish would enter Cambridge knowing of his family's connection with it.

The Greys had a similar connection with another eminent scientist. For at least ten years beginning in 1736, the Kent estate served as a lecture theater in the physical sciences and an observatory of the heavens. In those years the duke of Kent and, after his death in 1740, the duchess of Kent employed Thomas Wright as a scientific teacher. He is the well-known astronomer who was first to describe the structure of the Milky Way in his *New Hypothesis of the Universe* appearing in 1750,[14] when Henry Cavendish was the University. Born into an artisan family, self-taught in astronomy, Wright made his living by teaching

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[11] The earl of Godolphin to the duchess of Marlborough, [24 April 1704]. John Churchill, duke of Marlborough (1975, 1:284).

[12] *Conversation Piece at Wrest Park*, around 1735. See Fig. 1.3.

[13] "Cotes, Roger," *DNB* 1st ed. 4:1207–9. There was a further connection between the Greys and Roger Cotes. Roger Smith, Cotes's cousin and future successor to the Plumian Professorship, wrote to Thomas Birch, "As his [Cotes's] father was rector of Burbage formally held by the Earl [later Duke] of Kent, so by his Mother (a daughter of Major Farmer [?] In Leicestershire) he was pretty nearly related to the present Duke." Letter of 6 Jan. 1735/36, BL Add Mss 4318, f. 215.

[14] Thomas Wright (1971).

science, mathematics, and surveying, by publishing on these subjects, and by surveying the estates of the aristocracy. His pupils included Jemima, duchess of Kent, and Kent's daughters, Ladies Sophia de Grey and Mary de Grey (but not Lady Anne de Grey, who married Charles Cavendish), his son-in-law Lord Glenorchy, and his granddaughter Jemima, the future Marchioness Grey. He taught the Kent women geometry, navigation, surveying, and no doubt other subjects from his ambitious curriculum. Residing for months at a time at Wrest Park, Wright probably did surveying there as well as teaching, for the duke was constantly building, and the duchess, Wright noted in his diary, surveyed the garden and made plans for it. We know that Wright designed a rustic, thatched cold bath for the Marchioness Grey at Wrest Park.[15] Wright also carried out his own astronomical studies at Wrest Park; from there in 1736, for example, he communicated to the Royal Society his observations of the eclipse of Mars by the moon.[16] Wright was still teaching the Kents when Henry Cavendish was fifteen, and no doubt he and his father became acquainted with him at Wrest Park and in London on their visits to the Grey townhouse. When the duke of Kent died, his "Closet" included a surveying instrument described as a "Spirit Level with a Telescope Light two foot long by Wright" together with a variety of other mathematical instruments.[17]

Occupying 120 acres and enclosed by a two-mile gravel walk, the elegant garden at Wrest Park contained mementos of friends and of royalty whom the duke had served or admired, which included statues of King William (because the duke was a "good Whig") and of Queen Anne (because she was a "good Servant"). Standing in a corner of the garden was a pyramid inscribed with the years of the beginning and end of the duke's proud improvements of the estate. The larger setting, the park, contained 800 acres, enclosed by a grass walk, with plantations of lemon and orange, irregular clusters of "venerable" oaks, canals containing fat carp and pike, an obelisk eighty-six feet high, extensive lawns, a pavilion, a greenhouse, a bowling green, statues, vases, a temple of Diana, falls, ridings, and herds of deer. In the distance, cottages and churches could be seen, including a church resembling a picturesque ruined castle. The grand house of the estate was approached by a broad, tree-flanked avenue lying in the park. This description is from a letter written at Wrest Park in 1743, three years after the duke's death, by Thomas Birch, a literary man and later secretary of the Royal Society who thought that the best room in the house was the library.[18] Wrest Park with its wealth of books and with its artful blend of geometrical precision and natural grandeur would have been a familiar scene to Charles and Henry Cavendish. Kent's legacy to them was a breath of cultural interests, including science, outside of politics and pride in the standing of his family, symbolized by his creation, Wrest Park.

Devonshire

Growing up in the shadow of the "Great Duke of Devon"—his contemporaries spoke of the first duke of Devonshire as if he were already a legend—Henry Cavendish's other grandfather, William Cavendish, the future second duke of Devonshire, could have been crushed

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[15] David Jacques (1983, 70).

[16] Entries from Thomas Wright's diary, in Edward Hughes (1951, 13–22). His observations at Wrest Park are reported in 28 Oct. 1736, Journal Book of the Royal Society, 15:371. Hereafter JB, Royal Society

[17] Bedfordshire Record Office, Wrest Park Collection, L 31/184.

[18] Draft of a letter by Thomas Birch from Wrest Park, 28 Sep. 1743, British Library Add Mss 4326B, ff. 180–182. Hereafter BL.

completely. The first duke was a willful, flamboyant man who defied and created kings, picked violent quarrels at the drop of a hat,[19] and rebuilt one of England's finest great houses, Chatsworth (Figs. 1.6–1.7). In any event, the son grew up to be more mature, better balanced, more reasonable, and on the whole a much more solid and, one suspects, more intelligent man than the father, and, in the trade-off, much less exciting. About the second duke there are none of the stories about duels and mistresses, street fights and defiance of authority that make the first duke so fascinating. Up to a point, young William, reasonably enough, allowed his life to be directed by his father: at sixteen, he was married to fourteen-year-old Rachel Russell, daughter of Lord William Russell, Devonshire's former political ally and friend and now "martyr" to the Whig cause.[20] As soon as William came of age, he followed his father into politics, in his early years serving as a Member of Parliament. He even imitated his father's boldness, taking initiatives and speaking frequently for his principles in the House of Commons, on one occasion going so far as to challenge an opponent. But when he spoke, he spoke his own mind, not his father's, and in addressing conflicts, he was much more likely to use reason, persuasion, and compromise than the sword. "His mansion was not a rendezvous for the assemblies of foppery," it was said of him: "none were permitted to partake of the… refined… pleasures of his house… but the ingenious, the learned, the sober, the wise."[21] He was not really that proper, but he did value learning and cool judgment, and in an environment of courtly intrigue and political passions, he impressed the duke of Marlborough as a "very honest man" and a man who "governs himself by reason."[22] George I, according to Lady Cowper, thought so too: he was one of only two men in the kingdom whom the king had found to be "very honest, disinterested."[23]

Of his relationship with his family we get a glimpse only now and then. On his Continental tour, as a newly married boy, too young yet to be allowed to live with his wife, he wrote considerate letters to his mother-in-law, Lady Rachel Russell, to which she replied: "I can have no better content in this world then to have your Lordship confirm my hope that you are pleased with your so near relation to us here, that you believe us kind to you, and value our being so."[24] The boy's thoughtfulness and good breeding made his high expectations all the more agreeable. Writing about William and Rachel's marriage, Lady Russell sensibly remarked: "We have all the promising hopes that are (I think) to be had; of those I reckon riches the least, though that ingredient is good if we use it rightly."[25] William and Rachel Cavendish used their riches responsibly and tried to teach their children to do the same, Rachel apparently being the parent who dealt with the children. "I must needs tel you yt yr your father can by noe means allow you to goe on in this way," she admonished their second son James for gambling while on tour abroad, "& so he bids me tel you ye expanses of yr travels have been very great already without ye addition, more I believe than is allow'd to most elder brothers, & tho I hope yr father is able to make you very easy in yr fortunes yet you may consider ye more you spend abroad so much ye less you will have at home whare it wou'd doe you more credit & I should think the more for yr owne satisfaction to spend

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[19] Great Britain, Historical Manuscript Commission (1924, 60, 240, 268–269, 271–272, 276).

[20] Lois G. Schwoerer (1988, 161–63).

[21] Hiram Bingham (1939, 308).

[22] Duke of Marlborough to earl of Godolphin, 14/25 June 1708, in Churchill (1975, 2:1011).

[23] George I quoted by Lady Cowper, 10 July 1760, in Mary, Countess Cowper (1864, 115).

[24] Lady Rachel Russell to William Cavendish, 5 Oct. 1688 (1793, 410).

[25] Lady Rachel Russell to Dr. Fitzwilliam, 29 June 1688 (1793, 399).

yr money amongst yr friends than strangers."[26] James never learned the value of careful husbandry of his means, but, as we shall see, his younger brother Charles, accompanying him on this trip, learned it very well. Like many of his well-to-do contemporaries, William, their father, did spend some of his fortune on works of art; however, even as a collector he managed to enrich the family fortune. Whether from frugality or good taste, he avoided the more expensive but often second-rate large works and instead acquired one of the finest collections of old master drawings, including works by Raphael, Dürer, Holbein, Rubens, Van Dyck, and Rembrandt.[27]

Dukes, Duchesses and Properties

Figure 1.1: Henry de Grey, Duke of Kent. By Jacopo Amiconi? Courtesy of the Bedfordshire Record Office

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[26] Rachel, duchess of Devonshire, to James Cavendish, [late 1722 or early 1723], Devonshire Collection, Chatsworth.

[27] Pearson (1983, 87–88).

Figure 1.2: Jemima Crewe, Duchess of Kent. First Wife of the Duke. By Riley. Courtesy of the Bedfordshire Record Office.

Figure 1.3: The Kents. *Conversation Piece at Wrest Park.* Probably by Charles Phillips, around the year Anne de Grey, Henry Cavendish's mother, was born. At the duke of Kent's country house at Silsoe in Bedfordshire. From left to right: Mary de Grey, William Bentinck, Barbara Godolphin, Lord Berkeley, Charles Bentinck, earl of Clanbrassil, countess of Portland, Henry de Grey (duke of Kent), Jemima Campbell (later Marchioness Grey), Sophia de Grey (duchess of Kent), Elizabeth Bentinck, countess of Clanbrassil, and Countess Middleton. Courtesy of the Bedfordshire Record Office.

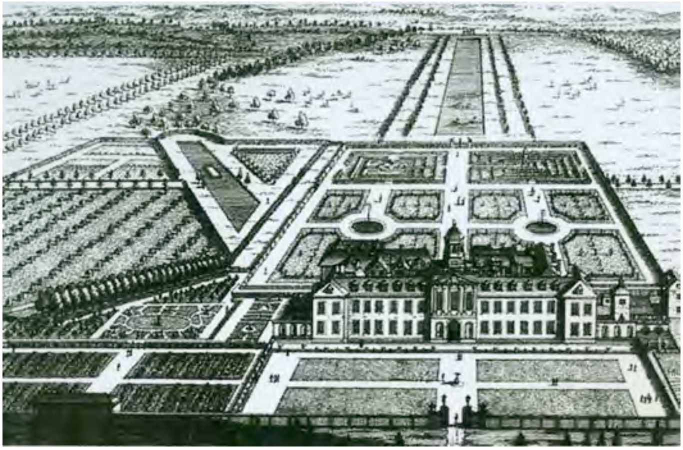

Figure 1.4: Wrest House and Park. By Pieter Van der Aa. In Bedfordshire. This shows the house, garden, and park as they appeared around 1708. The present Wrest House was built in the nineteenth century.

Figure 1.5: Wrest Park. Photograph by the authors. Wikimedia Commons.

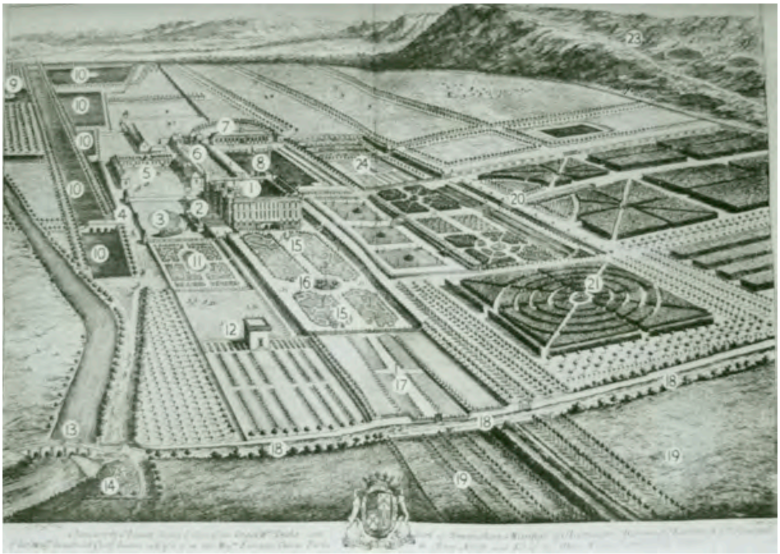

Figure 1.6: Chatsworth House and Gardens. By Pieter Tillemans. Turn of the eighteenth century. Seat of the dukes of Devonshire, in Derbyshire. Construction began in 1687.

Figure 1.7: Chatsworth House. Photograph by the authors. Henry Cavendish’s papers are kept there.

Figure 1.8: William Cavendish, Second Duke of Devonshire. By Charles Jervas. Devonshire Collection, Chatsworth. Courtesy of the Chatsworth Settlement Trustees. Photograph Courtauld Institute of Art.

Figure 1.9: Rachel (Russell), Duchess of Devonshire. By M. Dahl. Devonshire Collection, Chatsworth. Courtesy of the Chatsworth Settlement Trustees. Photograph Courtauld Institute of Art.

William's reliance on reason and integrity, a quality apparently shared by his wife, also is reflected in their family life. "I have always taken you to have a very good understanding," Rachel wrote to James; "if you make but the right use of that, you will know what is most for yr owne good."[28] They encouraged their children to think for themselves. In the matter of an allowance, for example, Rachel twice asked James what he might need while he was abroad, his parents reserving the right to disagree with him: "I thought I was right to ask yr opinion as to ye sum, concluding I knew you soe well yt if I shou'd happen to think it too much, you wou'd not take it ill yt I told you soe."[29] Their difference of opinion resulted in a compromise, with James sending pleasing reports of his economy to his parents. With regard to the boys' travels, too, "yr father in that wo'd be willing to do what he thought was most agreeable to yr own inclinations … you may let me know what yr own thoughts are."[30] In a future son-in-law, William and Rachel valued that he was said to be "very sober & of an extreem good character wch is above every thing elce."[31] This sensible family life not only nurtured love and respect but also the clear thinking and the levelheaded assumption of responsibility of Charles Cavendish.

From the time he returned from his Continental tour until his death in 1729, William Cavendish, second duke of Devonshire from 1707, continuously devoted his life to public service at the highest level of government. To the Whig interest, he brought not only his own political but also his wife's strong personal desire. Rachel Russell had been brought up not to forget the injustice done her family by her father's execution in 1683 at the hands of the Stuarts. Nine years old at the time of her father's trial and execution, she had been taken by her mother to see her father imprisoned at the Tower.[32] Her mother had later written about her: "Those whose age can afford them remembrance, should, methinks, have some solemn thoughts for so irreparable a loss to themselves and family."[33] Attending the proclamation of William and Mary as king and queen, Rachel pronounced herself "very much pleased" to see them take the place of "King James, my father's murderer."[34] Lady Russell tried to turn the family suffering for the Whig cause to her son-in-law's political advantage. Soon after William Cavendish's return in 1691, his "friends," including Lady Russell, exerted their influence to have him stand for Member of Parliament for Westminster. Lady Russell warned off other potential Whig candidates, reminding them of their political debts: "I believe the good his father did in the House of Commons […] will be of advantage to this [William Cavendish's candidacy]. And it will not hurt his interest that he is married to my Lord Russell's daughter."[35] The Russell name was then thought so great a guarantee of political success that in 1695 two of the principal government Whigs unsuccessfully tried to talk Lady Russell into letting her fifteen-year-old son stand for Parliament, certain that he would be elected and bring in another Whig with him.[36]

That year her oldest son, William, began his parliamentary career as member for Derbyshire, his home county. The Russells, like the Cavendishes, had received official recog-

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[28] Rachel, duchess of Devonshire, to James Cavendish, [late 1722 or early 1723].

[29] Rachel, duchess of Devonshire, to James Cavendish, 20 Mar. 1723, Devon. Coll.

[30] Rachel, duchess of Devonshire, to James Cavendish, 13 Feb. 1724, ibid.

[31] Rachel, duchess of Devonshire, to James Cavendish, [late 1722 or early 1723].

[32] Mary Berry (1819, 36).

[33] Lady Rachel Russell to her daughter Rachel Russell, [1687], Berry (1819, 81).

[34] Rachel, duchess of Devonshire, to a friend, Feb. 1689, Berry (1819, 93–96, on 95).

[35] Lady Rachel Russell to Mr. Owen, 23 Oct. 1691 (1793, 533).

[36] W.L. Sachse (1975, 107).

nition for their services the year before, when William's father was raised to a duke and Rachel's grandfather, William Russell, became the first duke of Bedford, an honor that would have gone to her father if he had lived.

The Revolution of 1688–89 elevated the Cavendish family and at the same time gave them a political direction. The Declaration of Rights of 1689, enacted as the Bill of Rights, prescribed the religion of the monarch, limited his prerogative powers, increased the powers of Parliament, and in general discouraged the prospects of a despotic monarchy.[37] The Declaration had left open to dispute the exact relations between king and Parliament, and William Cavendish, as marquess of Hartington, stood over the gaps. (The duke of Devonshire had a subsidiary title marquess of Hartington, which his eldest son was allowed to borrow as a courtesy title.) Hartington's actions in the House of Commons suggest the political identity he created for himself. Rarely participating in committee work on so-called private bills, which dealt with local problems such as bridge repair and individual estates, he preferred to address general questions, for example, the king's request to retain a large army after the peace of Ryswick. He opposed the request, as did Parliament, on the grounds that it was forbidden by the Bill of Rights as a threat to English liberty.[38] At the beginning of the eighteenth century, the criticism of government was redirected toward the king and his ministers for corrupting Parliament, which itself was now seen as a threat to liberty.[39] William extended his concern with rights of the House of Commons to the "Rights and Liberties" of "all the Commons of England," asserting the subject's right to address the king for calling, sitting, and dissolving Parliament, his right to a speedy trial on every charge including impeachment, and his right to vote as standing above the privileges of the House. In the House of Commons, William came to be closely associated with Robert Walpole.[40] William subsequently moved to the House of Lords as the second duke of Devonshire when his father died in 1707, having ordered inscribed on his tomb, "Here lies William duke of Devonshire, a faithful subject of good princes, and an enemy to tyrants."

Although this is not the place to discuss in detail the career of the second Duke of Devonshire, we believe it is important to give the reader an idea of it, since it enters into our understanding of his son Charles and his grandson Henry Cavendish. First, his, the second duke's, public position affected theirs; for them, and for all those with whom they came into contact, their being a Cavendish was a matter of no small significance. Second, the nature of the duke's career reveals much about his understanding of his public role and obligations, and, as we will see, Charles brought a similar understanding to his own public service, as did his son Henry. In his scientific work, Henry would not have had in mind his family's political principles, but his aspiration suggests a comparison; the political Cavendishes secured the rights and laws of the kingdom, and another Cavendish in another endeavor sought the ruling laws of nature.

At the time Cavendish entered science, the Whig cause was nearly spent, and in a very general sense, power in society was coming to be determined less by custom and more by rule over nature, which included the experimental manipulation of nature. As human progress was seen to depend less on traditional authority and increasingly on the "authority of experiment," landed families such as the Cavendishes had a vested interest in the world of Henry

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[37] Lois G. Schwoerer (1996, 47–57).
[38] Henry Horwitz (1977, 250).
[39] Schwoerer (1996, 49–57).
[40] Horwitz (1977, 302–303). William Cobbett (1810, 5: cols. 256–57, 301).

Cavendish. As improvers of their estates, which comprised gardens, farms, mines, and investments in technical properties such as canals, they were unwitting Baconians, advocates of applied science.[41] Through their work in and for science, Charles in the second half of his life and Henry throughout his life were not as removed from the practical concerns of their family as might first appear. The fifth duke of Devonshire, a man of conventional opinions, may have had a glimmering of it, even as he judged Henry Cavendish, his working cousin, to be the black sheep of the family.

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[41] Larry Stewart (1992, 253, 384–385, 391–393).

Chapter 2
Politics

Early Years and Education

Born on 17 March 1703/1704,[1] Charles Cavendish joined three sisters and two brothers in the nursery of William and Rachel Cavendish, Lord and Lady Hartington. Two brothers had died in infancy, the first born male, William, and the first boy to be named Charles, in the year before our Charles was born. Three more girls and one boy entered the family over the next few years. Charles grew up probably not particularly noticed in the middle of his siblings.

Like all his brothers and sisters, Charles was born at Hardwick, Derbyshire. Rebuilt in the late sixteenth century by the energetic Elizabeth, countess of Shrewsbury, Hardwick Hall was a fine specimen of Elizabethan architecture. This founder of the House of Cavendish also built Chatsworth House in Derbyshire, further testimony to her opulent ambition. When Charles was three, his paternal grandfather died, and his father took title and possession of the extensive properties of the Devonshires: Hardwick, Chatsworth, and other houses, including Devonshire House in Piccadilly, all of which the Cavendish children could call home, even if they did not live in all of them. For a while their homes also included Southampton House, the London residence of their maternal grandmother, Lady Rachel Russell. They visited the houses of their other Russell relatives: Woburn Abbey in Bedfordshire, their mother's girlhood home; Stratton House in Hampshire, their grandmother Russell's country estate; and Belvoir Castle in Leicestershire.[2]

Inside their various substantial four walls, the Cavendishes enjoyed informal relationships. Unlike many aristocratic families, for example, the duke of Kent's, Charles's family did not use formal titles for one another. In their letters, even after they were adults, Charles's sisters referred to their mother as "Mama," not "her Grace," the title appropriate for a duchess, and they wrote of "brother Charles" rather than "Lord Charles" and of "Granmama Russell" rather than "Lady Russell." Charles's sister Elizabeth suggests the warmth of their relationships when in 1721, after the death of their oldest sister, Mary, and their youngest brother, John, she wrote to another brother James, who was abroad, about Charles, who was about to join him: "It was some comfort to have one of you but when both are gone I shall find great change when I consider I was once happy in ye company of so many brothers and ss ; but it is a thought I cannot bear to think of."[3]

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[1] *The Peerage: A Geneological Survey of the Peerage of Britain …*, comp. D. Lundy (http://www.thepeerage.com).

[2] Lois G. Schwoerer (1988, 222). The author lists the Russell family homes and refers to Lady Russell's closeness to her children. Various family letters refer to members of the family visiting one another.

[3] Elizabeth Cavendish to James Cavendish, 13 Feb. and 24 Apr. [1721], and Rachel Morgan to James Cavendish, 26 Sep. [1723], Devon. Coll., 166.0, 166.1, and 167.0.

Of his siblings, two brothers, William and James, and four sisters, Mary, Rachel, Elizabeth, and Anne, survived into adulthood with Charles. Their earliest education was probably under the care of tutors and governesses. Their grandmother, Rachel Russell, who on her mother's side was of Huguenot origins, had advocated using French refugees as tutors in the 1680s.[4] Later she entertained some negative views of instruction by French tutors, but she nevertheless took considerable trouble to find one for her grandchildren by another daughter.[5] The Cavendishes may have followed her lead, since the whole family continued the close connection with their Huguenot Ruvigny relatives, now settled in Greenwich and parts of Hampshire.[6] When James and Charles toured the Continent in 1721–24, they did so under the care of a Frenchman, a Mr. Cotteau.[7] The Cavendish daughters were educated to interests as commonsensical as their brothers. On her honeymoon, Rachel reported to her brother James on a visit to the Derby silk mills, "thought to be one of the finest inventions that ever was seen of the kind."[8] Elizabeth was impetuous and independent, if we can judge from the few extant letters. Seeing her life as "idle," she wrote to James: "I only wish I was your brother instead of your sister and then I would have bin partaker with you in your travels." Forced to remain behind, she informed her brothers of the politics of the day. Looking at it from the heights of her father's position, she approved of a minister who did not enrich himself by his office, and she reported the birth of a prince causing "very great" joy among the people as a political advantage, the birth coming "very seasonably to stir up ye spirit of loyalty in ye people who are in a general dissatisfaction with ye king and parliament who they think don't go ye way to redrys their grivances caused by ye south sea."[9]

The Cavendish boys received only the beginnings of their education at home. Their grandmother Rachel Russell was of the opinion that "our nobility should pass some of their time" at the university, noting that among them university education "has been for many years neglected,"[10] a view which was shared by her daughter and son-in-law Devonshire, who sent their eldest son, the sixteen-year-old William, to Oxford in 1715, entering him at New College. As a member of a Whig family in a Tory citadel, William joined with other Whigs, only to find their group the target of a mob. In 1717, two months after they were attacked, he was granted the degree of Master of Arts and left Oxford. The family biographer comments on how quickly a duke's son could attain that degree; considering that prudence was a characteristic trait of the Cavendish family and, in particular, of William's parents, his political adventures and his leaving Oxford may have been related.[11] His brothers, in any case, were not sent to a university.

Charles and James began their formal schooling at Eton, where they were entrusted to Dr. Andrew Snape, headmaster from 1711 to 1720, on the recommendation of Robert Walpole, their father's friend and political ally. In 1718, for which there exists a "Bill of Eton Schole," Charles, then fourteen, was in the fifth year, a grade in the Lower School

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[4] Mary Berry (1819, 73).

[5] Schwoerer (1988, 227).

[6] Samuel Smiles (1868, 208–211, 314).

[7] Rachel Cavendish, duchess of Devonshire, to James Cavendish, 20 Mar., 12 July, and 11 Nov. 1723, and 13 Feb. 1724, Devon. Coll., 30.10–14.

[8] Rachel Morgan to James Cavendish, 26 Sep. [1723].

[9] Elizabeth Cavendish to James Cavendish, 24 Apr. [1721].

[10] Lady Rachel Russell to John Roos (Manners), 5 Nov. 1692, in Lady Rachel Russell (1793, 550).

[11] Joseph Foster (1891, 231). Francis Bickley (1911, 189–190).

known as Lower Greek, and James was two years ahead of him.[12] Neither boy finished the entire course, which for Charles would have required another five years. Both were heading in a direction other than the university, for which they probably were not prepared in their knowledge of ancient languages in any case. Young noblemen had other options, as the advice given to the father of one of them in 1723 shows. Though his son "does not ply his book close," it may not proceed from the want of capacity and inclination:

> but rather from his studying in the dead languages, which he has not been well grounded in. I have knowen severall instances of this and if it be the case or perhaps his being too much indulged in sloth when younger, I do not see why either of them should be a reason for breaking off his studies. He can read in Italian and French most of the things that are necessary for a gentleman, and tho' he should not give a very close application, something usefull will stick; and who knows but by degrees he may come to like what he now has ane aversion to. Were he mine, I would make him spend some time at Geneva in the studie of the law, should it be only to keep him from being imposed upon by pettyfoggers. Historie and geometry are accomplishments fitt for a gentleman and surely he can never serve his country or famely without knowledge, and geometry, if he give in to it, will at all times be ane amusement when he cannot be more profitably imploy'd. When he has made a tolerable progress in these, it will not be amiss that he make a tour in France and Italy that he may learn from observation what he has not gote by reading.[13]

The reference was to the by now obligatory grand tour that began in France, perhaps passed through Holland and Switzerland, and then settled down to a residence in Italy, home of Rome and the Renaissance. No Englishmen could pretend to an education or any degree of sophistication without this tour, two or three years abroad being the rule, a just compensation for having been born in backwater England.[14] Some formal study might be combined with the sightseeing and cultural exposure. Anthony and Henry de Grey, sons of the duke of Kent and brothers of Charles's future wife, Anne, had followed this course several years earlier. In 1716, as Henry de Grey was planning to go to Geneva, Anthony sent him advice from Venice:

> Att Geneva you will find several persons that will be very helpful to you I don't doubt, and I shall send a letter or two to some of the best I knew there who are of the best familys, men who are pretty well acquainted with the world and whose conversations will be agreable as well as instructive, that shall wait upon you and do any service that lies in their power as soon as ever you arrive; there are like wise some of the young men I was acquainted with who will be ready enough to introduce you into any other company you shall like or care for. I

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[12] R.A. Austen Leigh (1907, xxiv–xxvii, 14–18). J.H. Plumb (1956–1960, 1:253). The "lower master" of the lower school in 1718 was Francis Goode, who held that position from 1716 to 1734, succeeding Thomas Carter. There were four lower school assistants that year, Thomas Thackeray, Adam Elliot, John Burchett, and Charles Willats, three of whom were drawn from King's College, Cambridge, the other, Burchett, from Peterhouse, Cambridge. *Eton College Lists*, xxxv. It was customary at Eton for the "sons of wealthy persons to have private tutors," who were not the same as the assistant masters. H.C. Maxwell Lyte (1911, 284).

[13] Great Britain, Historical Manuscripts Commission, (1924, 3:287–288).

[14] J.H. Plumb (1963, 55–60).

> suppose you intend to study a little of the Civil Law there; the person I had and who is accounted one of the best is Mr. Guip a diligent and Studious man and likewise understanding in History and Chronology.

Having followed his own stay in Geneva with travels in Italy, Anthony displayed in the remainder of his letter that he had profited from the lessons in history, having become a careful observer of "antiquities."[15]

James Cavendish, whose later exploits suggest an early interest in horsemanship and an active life, was probably, and quite appropriately, intended for the military. By 1721, he had gone from Eton to the "academy" in Lorraine, and Charles was then about to join him. Two years later, James wrote to his mother from Geneva, with the likelihood that he continued his education in both Lorraine and Geneva.[16]

The "Académie d'Exercises" at Nancy, the capital of Lorraine, had been established in 1699, soon after Lorraine had been taken back from the French and reconstituted a duchy by the Treaty of Ryswick of 1697. Although the dukes of Lorraine were allowed no army of their own, their military Academy attracted young foreign aristocrats, some carrying "the greatest names of Europe." By 1713 the Academy had added a course in public law to its curriculum, and Duke Leopold himself established one in natural law. The Academy had the purpose of educating cadets for the court guards, the only military body aside from a civilian militia still remaining to the dukes. This close association with the court affected the location of the Academy. In 1702, at the beginnings of the War of the Spanish Succession, the French had reoccupied Nancy, forcing Leopold to withdraw with his court to his castle at Lunéville, a building then too ancient to be suitable for an eighteenth-century ducal residence. Leopold replaced the old structure with a large, new residence, which gradually became the official capital of the dukedom even after Nancy had been freed from the French again in 1714. In 1719 a fire temporarily set back this development by destroying the ducal apartments at Lunéville, apparently forcing the court back to Nancy for a short time. It was during this period that James Cavendish joined the Academy. Seeing an opportunity for further building, Duke Leopold added a "cabinet des herbes," a good library, and a physical cabinet to his Lunéville residence. Under the influence of Newton's physics and determined to do his own experimenting, he constructed some of the necessary instruments himself, buying for the rest a beautiful and expensive collection from London. In the spring of 1721, just before Charles joined his brother in Lorraine, the duke moved his military Academy from Nancy to Lunéville,[17] bringing it into the immediate neighborhood of the scientific facilities he had assembled there.

Charles Cavendish left London for his education and tour abroad in March 1721, undoubtedly with another party traveling to Paris, since he was to be met there by his brother James's valet, and as the seventeen-year-old son of a duke he would not have been sent off alone.[18] Expected to be with James by mid-April, he instead stayed on in Paris three weeks

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[15] Anthony de Grey to Henry de Grey, about 1760, Bedfordshire Record Office, Wrest Park Collection, Dale 30/5.

[16] Elizabeth Cavendish to James Cavendish, 13 Feb. and 24 Apr. [1721]. Rachel Cavendish, duchess of Devonshire, to James Cavendish, 11 Nov. [1723].

[17] Michel Parisse, Stéphane Gaber, and Gérard Canini (1982, 43). Michel Antoine (1968, 70–72), and Claude Collot (1968, 218). Edmond Delorme (1977, 3, 17, 18, 111). Pierre Boyé (1980, 3–4).

[18] Elizabeth Cavendish to James Cavendish, 13 Feb. [1721]. A party bound for Paris that Charles might have joined was that of the English ambassador to France Lucas Schaub, who planned to leave London for Paris on 23 February/ 6 March. That plan, given that the trip took four to five days if all went well, would have put him in Paris in the

longer than planned. As Anthony de Grey had informed his brother a few years earlier, in Paris there were “many things” to be “observed“

> You will not stay long there perhaps the first time only to see a little of the Town. … You wont ommitt however the sight of the most principal things, as the Louvre, the Tuilleries, Place Vendosme & Victoire, Place Royal, the Luxemburg, the Church of Notre dam, L’hotel des invalids, Versailles, Trianon.[19]

Both his initial visit to Paris and his stay there with James for several months in 1723–24 came at a favorable stage in English-French relations, during the regency of the duke of Orléans and immediately after. The friendly climate toward England at court was accompanied by a resurgence of cultural life in Paris as, following the death of Louis XIV in 1715, French aristocrats returned from Versailles to Paris.[20] The flourishing arts, operas, theater, and other entertainments lured so many of the British to Paris in these years that the resident at Paris, Thomas Crawford, complained in 1723 that we “should have had the halfe of the people of England” there if it had not been for the unsafe conditions of the roads; “this town began to be full of London apprentices that came running over here with their superfluous money instead of going to Tunbrige,” an English resort.[21] The regency was also marked by another interest of the duke of Orléans, this one much closer to Charles’s eventual concerns, the natural sciences and the “improvement of the implements and appliances of the mechanical arts.”[22] René Antoine Réaumur, the regent’s protégé at the Paris Academy of Sciences, published his important study of the iron and steel industry in Paris in 1722, which may well have come to Charles’s attention, given the practical bent of his family and their ownership of Derbyshire lead mines.[23] As a Cavendish, indeed, he may have enjoyed even more direct exposure to the Parisian scientific world, but we have no evidence for that.

After Paris, if he proceeded as planned, Charles joined James at Lunéville, and for nearly two years after that, until late in 1722 or early in 1723, his activities and whereabouts can only be conjectured. Given the pattern of his brother’s stay abroad, Charles may well have spent a year at Lunéville. During the winter of 1722–23, the brothers were traveling together with a tutor, probably in the south. James had been tempted into gambling, prompting his mother to point out to him that the “right use” of their travel should be “seeing what is most curious in ye places you pass thru & making yr observations upon ‘em.” The following March, James was staying with a prince and princess, an “expensive enuff ” way of life, his mother commented in a discussion of his allowance. Neither the duchess’s letter to James in March nor another one in the middle of July refers to Charles, making it likely that Charles spent some time on his own in Geneva, from where he had written to his mother that summer or fall.[24]

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second week of March, the time when James was to send his valet to meet Charles. In the event, Schaub did not leave London until March 1/12, a possible reason for the delay in Charles’s plans too. Great Britain, Historical Manuscripts Commission (1931, 3:49–52).

[19] Anthony de Grey to Henry de Grey, about 1716.

[20] James Breck Perkins (1892, 374–396, 554–557, 559–562).

[21] Thomas Crawford to Lord Polwarth, 9 Oct. [28 Sep.] 1723, in Great Britain, Historical Manuscripts Commission (1931, 3:308–9).

[22] Perkins (1892, 556).

[23] J.B. Gough (1975, 328).

[24] Rachel Cavendish, duchess of Devonshire, to James Cavendish, late 1722 or early 1723, and 20 Mar. 1723.

The "Académie de Calvin" in Geneva had attracted not only the sons of the duke of Kent, but also the sons of several great English and Scottish families, including the Cavendishes. In 1723, four professors at the Academy offered courses in civil and natural law and in philosophy, including, apparently, natural philosophy, since one of its students, the later mathematician Gabriel Cramer, had only recently completed a thesis on sound and next year would compete for the chair of philosophy; he received a share in the chair of mathematics instead, with the assignment of teaching algebra and astronomy.[25] If Charles did not meet Cramer at the Academy that year, he may have become acquainted with him through Cramer's brother Jean, the new professor of civil and natural law, who was only twenty-two at the time. At any rate, when Gabriel Cramer visited London sometime between 1727 and 1729, he was easily received into the circle of mathematicians and fellows of the Royal Society connected with Charles.[26]

In November of 1723, James and Cavendish were together again, having only just arrived in Paris. Their stay in France required a doubling of their allowances, each now getting £100 annually, and advice about greater caution on the roads: "be very carefull now you are in France," their mother wrote, "how you travel, & also of being out late in ye streets wch they tel me is very dangerious , murthers being there soe common."[27] They spent the winter there, still under the care of Mr. Cotteau, with mail reaching them through the banker Jean Louis Goudet. In February 1724, when the end of their tour was in sight, they appealed to their parents to stay a few months longer. "Relating to yr return into England," the duchess wrote, "I believe yr father in that wo'd be willing to do what he thought was most agreeable to yr own inclinations. Mr. Cotteau writs were you employed yr time so well, that he thinks it might be for yr advantage if you stay'd in France some months longer, but in yr next you may let me know what yrown thoughts are, yr coming back by Holland is what I believe my Ld designes if you like it."[28] Charles and James had their way. They also followed their father's plan of returning home by way of Holland, a detour that very nearly cost Charles his life. On 24 September that year, in "blowing Stormy weather," Captain Gregory of the Katherine Yacht at Ostend "about Three in the afternoon was unhappily Surprised by a Passage Boat oversetting just under my Stern, in which were Two of his Grace the Duke of Devonshire's Sons, viz the Lord James and Charles, with their Governor and Servants, who by the assistance of my People were all most miraculously Saved, particularly Lord Charles, who Sunk under My Counter, and Was Carried by a Very Strong Tide between me and another Ship under water, till he got as far forward as my Stern, where he arose, and got hold of my Shoar fast, from whence we Saved his Lordship, though almost Spent." James and Charles had been on their way to Calais, which suggests that they were coming from

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[25] Charles Borgeaud (1900, 442, 641–642). According to the registers of students, the Cavendishes who attended the Geneva Academy were Charles Cavendish's great-grandfather William Cavendish, who was accompanied there by his tutor Thomas Hobbes, the philosopher, and Charles's grandfather William Cavendish, later first duke of Devonshire. However, the registers are not complete, particularly on foreign nobleman, who might have stayed in Geneva only a few months. Anthony de Grey, who studied law in Geneva for a while, for example, does not appear in the registers; the absence of Charles's name is not an indication that he did not attend the Academy or study with a private teacher in Geneva. Sven Stelling-Michaud and Suzanne Stelling-Michaud (1959–1972). On the registers: Michael Heyd (1982, 245–247).

[26] Cramer and Charles Cavendish were exact contemporaries. Cramer's travels were a part of his appointment at Geneva and intended for his further education. The scientists he met in England included Nicholas Saunderson, Edmond Halley, Hans Sloane, Abraham de Moive, and James Stirling. Phillip S. Jones (1971, 459).

[27] Rachel Cavendish, duchess of Devonshire, to James Cavendish, 11 Nov. [1723].

[28] Rachel Cavendish, duchess of Devonshire, to James Cavendish, 13 Feb. [1724].

Holland, probably The Hague. After losing "most of their Baggage and Apparel, except what they had Ordered to Calais," in the accident, the Cavendish brothers decided to stay with Captain Gregory for the crossing. The captain's report of the accident reached their father by courtesy of the Admiralty on 5 October.[29] Charles and James undoubtedly followed close behind, Charles having been abroad for three and a half years.

House of Commons

In 1725 the year after his return from his tour of the Continent, Charles Cavendish was elected to the House of Commons. Taking his seat as a Member of Parliament for Heytesbury, Wiltshire,[30] in the parliamentary session of 1725–26, he joined all but two of the adult males of his family: his eldest brother, Lord Hartington, his uncle Lord James Cavendish, his two brothers-in-law, Sir Thomas Lowther and Sir William Morgan, and a first cousin. The two exceptions were his father, who as duke of Devonshire sat in the House of Lords and was then lord president of the privy council, and his brother James Cavendish, who was in the military, putting off his brief stint in the House of Commons by fifteen years, until just before his death. Charles Cavendish could have had no doubt about what was expected of him. To get a proper image of the inevitability of that particular blueprint for an aristocrat's life it should be noted that except for his uncle, Charles and his relatives in the Commons were all under thirty, he being the youngest at twenty-one. This dense representation in the Commons of an aristocratic family was only partly due to politics; apart from his father's close association with Robert Walpole, the head of the current Whig administration, Charles was in the Commons as a representative of his family's private interest. Very suitably, he made his first appearance in the Journal of the House of Commons in April of 1726 in connection with a private bill drawn up by his brother concerning the estate of his brother-in-law Sir Thomas Lowther, who had petitioned the Commons that his family be granted the inheritance of Furness monastery in Lancashire, establishing permanently an old family claim.[31] In the same year Cavendish dealt with another private bill that was at the same time about a matter of public importance, and it was also his first parliamentary exposure to a technical problem. The bill followed a long series of parliamentary acts providing for the draining of the Bedford Level fens, a huge track of marshland to the south and west of The Wash in eastern England. In the seventeenth century, Francis Russell, fourth earl of Bedford, and his son and successor, William, later first duke of Bedford (Charles Cavendish's ancestors), had organized about eighty landowners into a corporation of "adventurers" to finance the draining of these plains, which were still common land, in return for a portion of the resulting farmland. Having invested more in this undertaking and also profited more than any of the other members of the corporation, the Russells were still at the head of it in 1726, but the present duke was then a minor and the project was in the hands of his uncle and guardian, the duke of Devonshire. For Charles Cavendish, it even had a direct connection, since as a younger son he derived income from his mother Rachel Russell's interest in the Russell estate. With the methods then in place to drain the Bedford Level, the new farmland was

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[29] "Copy of a Letter from Captain Gregory of the Katherine Yacht to Mr Burchett dated the 25th of September 1724 O.S. From Ostend," Devon. Coll., 179.0.

[30] Romney Sedgwick (1970, 536).

[31] Great Britain, Parliament, *House of Commons Journals* 20:600–70. Entries from 4 Mar. 1726/27 to 19 Apr. 1727. Hereafter *HCJ*.

frequently flooded, and the bill Cavendish was involved in was a proposal to reduce flooding by constructing a new, steeper "outfall."[32]

Figure 2.1: House of Commons, 1741–42. From an engraving by Benjamin Cole, after John Pine, 1749. Lord Charles Cavendish represented three successive constituencies in the Commons between 1725 and 1741. Frontispiece, Romney Sedgwick (1970).

Reelected in 1727, but from the large constituency of Westminster instead of small Heytesbury,[33] Cavendish's participation in the House's activities increased in 1728 and 1729, only to be followed by four years of personal problems arising especially from his wife Anne's struggle with tuberculosis, which kept him away from his duties much of the

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[32] Samuel Wells (1830, 424–426, 661–662, 744–745). 4 Mar. 1725/1726 and 10 May 1726 *HCJ* 20:599, 697. H.C. Darby (1936, 456–459).

[33] Sedgwick (1970, 1:285). 21 July 1727, St. Margaret's Vestry, Minutes 1724–1733, Westminster City Archives, E 2419.

time. When, in 1733, his wife died, Cavendish immersed himself in his duties in the Commons. The regular problems of Westminster were typical of cities: repairing streets in "ruinous Condition," clearing them of "Filth and Dirt," and keeping them safe at night.[34] In 1729, for example, Cavendish and his colleagues crafted a bill to correct the ill effects of having several different privately owned "waterworks" lay water lines and cover them with pavement that was neither level nor strong and lasting enough.[35] A few weeks later he and his fellow member of Parliament William Clayton were ordered "to bring in a Bill for appointing a better nightly Watch, and regulating the Beadles… and for better enlightening the Streets, and publick Passages."[36] He worked on such problems for Westminster again in 1736 and 1737 though he had left this constituency.[37] Westminster was at times difficult to represent because it was the seat of Parliament and because it was contiguous with London. Popular dissatisfaction with local or national matters sometimes took on tangible form: the street bills in 1729, for example, brought out a great crowd, whose complaints the Commons refused to hear. During these years the city was in vehement opposition to much of Walpole's administrative program, as in 1733, when Walpole's handling of the proposed excise on tobacco brought not only local opponents but also the London mob to Westminster. Members of Parliament complained of a "tumultuous Crowd" who "menaced, insulted, and assaulted" them as they left the House. By order of the Commons, Cavendish and Clayton were directed to notify the high bailiff of Westminster that such actions constituted a crime and an infringement on the privileges of the Commons.[38]

After representing Westminster for seven years Cavendish was elected Member of Parliament for Derbyshire in 1734, his last constituency, which he also served for seven years. At Westminster, like his predecessors there, Charles had been elected with Whig support. Derbyshire, however, had long been in the hands of the Tories, Cavendish being the first Whig to be elected for the county since his father had lost his seat over thirty years before, and Cavendish's election was close.[39] His fellow Member of Parliament there was in fact a Tory, Nathaniel Curzon, a lawyer and land- and mine-owner who voted consistently against the administration.[40] Other counties in the area, such as Lancashire, Cheshire, and Yorkshire, were also represented by Tories, even ardent Jacobites. Cavendish was often not nominated to committees dealing with matters of concern to Derbyshire, although as its representative he could not be excluded from such committees, since the speaker of the House had the obligation to add to a committee any member who had a legitimate interest in the matter in question.[41] Cavendish was very actively engaged in only a few private acts initiated by his constituency in Derbyshire, drawing up only four bills for them, but he worked on

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[34] 4 Feb. 1728/1729, *HCJ* 21:208.
[35] 19 Feb. 1728/1729, ibid., 229.
[36] 10 Apr. 1729, ibid., 313.
[37] 16, 25 Mar. 1735/1736 and 14, 21 Feb. 1736/1737, *HCJ* 22:633, 652, 746, 756.
[38] 12, 13 Apr. 1733, ibid., 115–126. Plumb (1956–1960, 2:262–271).
[39] Sedgwick (1970, 1:223). In his first run for a seat from Derbyshire, Cavendish's vote was 2081, the runner-up Tory Curzon's, 2044, and the third candidate, the loser Harper's, 1795. Places where the Cavendishes owned property such as Normanton gave almost all their votes to Cavendish. Other places such as Thornhill and Pisley, just outside Chatsworth, gave him virtually no votes. *A Copy of a Poll Taken for the County of Derby, The 16th, 17th, 18th, and 20th Days of May, 1734 before George Mower, Esq.; High-Sheriff for the Said County* (Derby, n.d.), Devon. Coll., 95/81.
[40] Sedgwick (1970, 1:599).
[41] P.D.G. Thomas (1971, 58).

a number of private acts that benefited Derbyshire even if they did not deal with the county directly.

The subject of these private acts was road repair. The administration of English roads had been undergoing an important change from the beginning of the century. As the uses of the roads evolved from mainly local foot and animal traffic to through traffic for carriages and wagons, the roads were gradually converted into turnpikes, forcing the principal users to contribute to their upkeep. At the initiative of the local parishes responsible for road maintenance, and other interested parties, Parliament passed private acts establishing trusts responsible for setting up, financing, and maintaining the new turnpikes. The earliest of these had been along the main roads leading to London, two of which, the Great North Road and the road from London to Manchester, by the 1730s had already been turnpiked over considerable distances and in some areas the original turnpike trusts were already up for renewal. For Derbyshire coal trade, industry, and agriculture, it was important to complete the turnpiking of these roads and the east-west roads lying between them as well.[42]

In 1735 Cavendish had himself assigned to his first turnpike committee, this one dealing with the part of the London-Manchester road closest to London.[43] Three years later he and Curzon drew up the act that was to close the longest stretch of that road yet to be turnpiked, thirty-nine miles between Loughborough and Hartington, in Leicestershire and Derbyshire, respectively.[44] Altogether he worked on twelve private acts for turnpikes either on or near the two important highways and in addition on five turnpike bills for roads west and southwest of London.[45] To no other subject did he devote as much work; his interest is strongly confirmed by his related committee work on repairing bridges, above all, by the decade of work he devoted to the building of Westminster Bridge.

For the entire sixteen years Cavendish served in Parliament, Walpole was prime minister; Cavendish stepped down in 1741, Walpole in 1742. If Cavendish felt a family loyalty to Walpole, he did not always vote with Walpole. In 1725, the year Cavendish entered Parliament, William Pultney broke with Walpole,[46] and there is at least the suggestion that Cavendish sympathized with Pultney's opposition Whigs. In any event, Cavendish had other important interests to serve, namely, his familiy's, of course, but also Westminster's. His interest would seem to have been closer to the commercial and financial interests of the city then to those of the country (he sold his country home in 1736 and moved to the city) and the colonies, as is borne out by the episode of Walpole's excise tax on tobacco in 1733. Walpole almost fell from power because of it, with Cavendish doing nothing to help him. Walpole's tax was in the interest of Virginia growers, who had long resented control over their business by the London tobacco brokers. There was violent opposition to this tax in the city. Walpole's bill passed by a narrow vote, whereupon the city raised a petition against it, and Walpole's majority melted away, though he did manage to get the Commons to refuse to hear the petition. Walpole survived but not without a riot outside the Commons. Cavendish supported the bill in the beginning, but then he voted with the opposition on the city's petition against it. The king, who strongly sided with Walpole on this bill and regarded opposition

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[42] Sidney and Beatrice Webb (1920, 70). William Albert (1972, 31–43).

[43] 18 Apr. 1735, *HCJ* 22:469.

[44] 9, 20 Mar. 1737/1738, *HCJ* 23:73, 107.

[45] Information from *HCJ*.

[46] Plumb (1956–1960, 2:122–124, 127).

to it as treason, called Charles Cavendish "half mad" and James Cavendish, who voted as Charles did, a "fool."[47]

Cavendish's political career ended not by defeat but by choice. In 1741 he turned his Derbyshire seat over to William Cavendish, marquess of Hartington. Whether he sensed it or not, he left politics at about the time his family could dispense with his services. Up to the 1740s, but not beyond, the outcome of the Revolution of 1688–89 remained in question, for until then the Tories were predominantly a Jacobite party ready to ally with France to restore the Stuart dynasty. With the defeat of the Jacobite rising of 1745, intended to seat the Catholic Stuart pretender on the throne, the vigilance of the Devonshires could be relaxed, and Charles Cavendish could with clear conscience leave politics for good and consider another path for the remainder of his long life.

As we will see, the Royal Society largely assumed the place that the House of Commons had occupied in Cavendish's life. In making this change, he followed his own bent, for his political activities and associations did not in any obvious way point him in the direction of science. Of the roughly 200 members of Parliament with whom he served on committees during his sixteen years in the Commons, only a few were fellows of the Royal Society, at most a dozen, with maybe another half dozen becoming fellows after he had left Parliament, and none was to become a close scientific associate of his. Elected to the Royal Society about two years after he was elected to the Commons, Cavendish served on the Council of the Society for the first time in 1736. He did not serve again until the year after he left Parliament; after that time he served on the Council almost without interruption for twenty-five years.

Gentleman of the Bedchamber

The duke of Kent was gentleman of the bedchamber to George I, and in 1728 his future son-in-law Charles Cavendish was appointed to the same position, only to the Prince of Wales Frederick. Cavendish was indeed a "gentleman," though as son of the duke of Devonshire he was referred to as "lord" of the bedchamber.[48] With this position, Cavendish was a consort to the person who stood next in line for the throne, required to be in attendance for much of the day when it came his turn. The activities surrounding the prince's court could be tedious and stupid, but Frederick had a serious interest in the arts, being a passable cellist and a collector of works by old masters. Although he probably had little more interest in science than had his father, George II, which was practically none, he was willing to be seen in the company of men of science, attending a meeting of the Royal Society at which experiments were performed.[49] Known for his rakehell living, the prince would have had little in common temperamentally with his studious gentleman of the bedchamber, but the relations between the two young men evidently were good, for Cavendish's second son was named Frederick after the prince, who served as his godfather.

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[47] Plumb (1956–1960, 2:250–271). Thomas (1971, 68–71). Lord John Hervey (1848, 200). Sedgwick (1970, 1:537).

[48] John Edward Smith and W. Parkinson Smith (1923, 272). James Douglas, earl of Morton, who became president of the Royal Society while Cavendish was a member, had held a parallel position at court, as lord of the bedchamber. "Douglas, James, Fourteenth Earl of Morton," *DNB,* 1st ed. 5:1236–37, on 1236.

[49] Michael De-la-Noy (1996, 107, 115–116, 127, 194).

As it turned out, this prince did not live long enough to become king, but long enough to be a political force in his own right and the scandal of the reign. Frederick was born in Hanover in 1707 and remained there until December 1728, when he was brought suddenly to England because word was received at court that he was about to marry the princess royal of Prussia. The marriage had been negotiated and sanctioned by George I, but in 1727 Frederick's father, now George II, called it off. Although Frederick submitted, he detested his father for keeping him dependant, and when he married, with his father's approval, Princess Augusta, daughter of Frederick, duke of Saxe-Gothe, he turned this marriage into a weapon against his father. Competing with the king for popularity in the country, the prince formed an opposition court, welcoming into his household ambitious young men like Pitt, Lyttleton, and the Grenvilles, and he developed an intense dislike for his father's favorite minister, Robert Walpole. Confronted with the prince's passionate rebellion, the king drew the line in 1738; thereafter no one who paid court to the prince of Wales or his wife was admitted to the king's presence at any of the royal palaces.[50] Charles Cavendish, however, had left his post before the prince's banishment, having resigned in October 1730.[51]

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[50] Duke of Grafton to [Theophilus, earl of Huntington], 27 Feb. 1738, in Great Britain, Historical Manuscripts Commission (1928–47, 3:22).

[51] Entry on 17 Oct. 1730 in *The Historical Register*, vol. 15: *The Chronological Diary* (London, 1730), 64.

Chapter 3
Science

De Moive Circle

Technically speaking, Lord Charles Cavendish was a commoner, but he was nevertheless a member of the highest circle of the British aristocracy, and as such he had been brought up to the values of the aristocracy, including the principal value of "duty of service."[1] To an aristocrat such as Charles, the only acceptable form of occupation (aside from administrating, but definitely not farming, his property) was public service, usually either in government or in the military, or possibly in the church. It came down to a narrow but attractive choice of occupations. The Cavendishes had served in some of the highest offices at court and in the government for almost half a century, and Cavendish, as we have seen, followed their example as soon as he reached maturity. Other interests, in the arts, architecture, belles lettres, various areas of scholarship, or natural science, no matter how expertly pursued, had to keep the outward appearance of an aristocrat's private indulgence, at best to be shared with friends. Cavendish's contemporary Lord Chesterfield made what many would have perceived as a sensible judgment for the time when he censored the architectural expert Lord Burlington for having more technical competence than his rank permitted.[2]

From the perspective of the larger society, Charles Cavendish, who was drawn to experiment and to the instruments of experimental science, would have been seen as overstepping the bounds of his station if he had allowed his experiments to take over his life. The occupational limitations of the aristocracy almost certainly affected the way he worked in science and his scientific reputation, or lack of it. For many years he carried on scientific investigations that were valued and used by other investigators, but he published only the one paper for which he received the Royal Society's Copley Medal. He contributed publicly to science in the same manner in which he had served the government: as a "parliamentarian" of science, a member of the Royal Society who served on its councils and committees, and as a member of boards and committees of other organizations. As a result of this activity, he became one of the most important official representatives of science of his time in Britain, and its untiring servant. His qualifications were his scientific talent, practical ability, long parliamentary experience, and the Cavendish name. He was a good example of a kind of scientific practitioner who was useful in eighteenth-century British science but who did not survive into the later organization of science.

In 1725, the year after he returned from his Continental tour, Cavendish became a Member of Parliament, as we have seen, but since he was so very young, completely inexperienced, and relatively unknown, he entered slowly into the work of the Commons. As he

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[1] John Cannon (1984, 34).

[2] Dorothy Marshall (1968, 219).

was also relatively free of family duties, he had time to continue his education. His teacher, or one of his teachers, was almost surely the talented mathematician Abraham de Moivre.

De Moivre's friend Matthew Maty drew up a list of his eminent mathematical friends[3]: Newton, Edmond Halley, James Stirling, Nicholas Saunderson, Martin Folkes, and, on the Continent, Johann I Bernoulli and Pierre Varignon. (To this list we add from other sources William Jones[4] and Brook Taylor,[5] and there were still others.) Maty also listed De Moivre's friends and disciples, all former pupils of his: Lord Macclesfield, Charles Stanhope, George Lewis Scott, Peter Davall, James Dodson, and "Cavendish." (The Lucasian Professor in Cambridge John Colson should be included among his pupils, and no doubt others who come up in this book.)[6]

Since Maty gave only last names, we must decide which "Cavendish" he intended. Writing in the late 1750s, Maty would not have meant Henry Cavendish, who had only recently come down from Cambridge and was not yet a fellow of the Royal Society. Nor was it likely that he had in mind William Cavendish, duke of Devonshire, whom in any case he would have called Devonshire instead of Cavendish. The judgment Maty wanted his readers to make was of De Moivre's standing among accomplished mathematicians, not among unknowns or persons not known to have had significant mathematical interests. There are two likely possibilities, Charles Cavendish and his uncle James Cavendish. Both were active in the Royal Society, and both were proposed for membership in the Society by De Moivre's good friend, the eminent mathematician William Jones.[7] Together with Devonshire, both also subscribed to De Moivre's *Miscellanea analytica de seriebus et quadraticus*; published in 1730, which was the first mathematical or scientific book to which Charles subscribed. James Cavendish was born in 1678, and if he had been a pupil of De Moivre's he would have belonged to a generation earlier than that of the pupils named by Maty, indicating Charles as the more likely pupil of De Moivre's. Authors of a study of De Moivre's "knowledge community" write that both Charles and his uncle James and also his father William "were all taught by De Moivre."[8]

Among Charles's papers, kept and labeled by his son Henry, is a group "Mathematics." Because of the likelihood that by "Cavendish," Maty meant Charles Cavendish, and because of the evidence it provides of the mathematical education of the Cavendish family, we include the following brief discussion of De Moivre. De Moivre fostered a sense of connection between his pupils, evidently bringing them together at social evenings, and later keeping them "together as a kind of clique." Maty kept track of their publications in his *Journal Britannique*,[9] and they appeared together in the list of subscribers to De Moivre's

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[3] Matthew Maty (1760, 39).

[4] De Moivre called William Jones his "intimate friend" in the preface to his book *The Doctrine of Chances; or, A Method of Calculating the Probability of Events in Play* (London, 1718), x.

[5] De Moivre called Brook Taylor his "Worthy Friend" in his *Doctrine of Chances*, 101. His correspondence with Taylor is described in Ivo Schneider (1968, 196–197).

[6] In the foreword to his first book, *Animadversiones*, De Moivre referred to John Colson as one of his pupils, noted by Schneider (1968, 189).

[7] James Cavendish was proposed for membership in the Royal Society on 19 Mar. 1718/1719, and was admitted on 16 Apr. 1719, JB, Royal Society 11:311, 326.

[8] The likely intermediary who supplied De Moivre with a letter of introduction was one of two Huguenot friends, Abraham Meure or John Buissière. D.R. Bellhouse, E.M. Renouf, R. Raut, and M.A. Bauer (2009). Published online before print (25 Feb. 2009 http://rsnr.royalsocietypublishing.org/content/early/2009/02/23/rsnr.2008.0017.full).

[9] Uta Jannsens (1975, 17). Augustus De Morgan (1857, 341).

republication of his mathematical papers.[10] Through De Moivre, his pupils formed a living connection with great mathematicians and scientists of the recent past. The intermediary De Moivre was Newton's junior by twenty-five years and Cavendish's senior by about the same number of years.

If we leave aside the foreigners named by Maty, we are directed to a select few mathematicians within the larger group of British mathematicians in the early eighteenth century with whom Cavendish came to be associated. For convenience, we will speak of a "De Moivre circle," whose members give us an idea of the mathematical setting in which Charles Cavendish probably completed his education.

The learned world of London had recently been enriched by an influx of Huguenots, Protestants forced by Louis XIV to leave France with the revocation of the edict of Nantes. Within the Cavendish family, as we have seen, the Ruvignys settled in Greenwich, home to the Royal Observatory, a prophetic location, and they encouraged other refugees to follow.[11] De Moivre and his father, one of a number of Huguenot surgeons and physicians to seek asylum in England, were naturalized in 1687;[12] Abraham was then twenty and an advanced student of mathematics.

In De Moivre's mind, his arrival in England was so closely identified with his discovery of Newton's work that although two years elapsed between the two events, to him they seemed simultaneous. For biographers of Charles and Henry Cavendish, it is gratifying that De Moivre first encountered Newton's work in the house of the earl of Devonshire. It was probably in 1689, when Newton spent a good deal of time in London as a member of the Convention Parliament for Cambridge, and when Devonshire enjoyed the fruits of the Revolution as a prominent politician in Parliament and at the court of William and Mary. De Moivre first saw Newton as he was leaving Devonshire's house after presenting the earl with a copy of his *Principia*. Shown into the antechamber where Newton had just left his book, De Moivre picked it up expecting to read it without difficulty, but he found that he understood nothing at all. He felt that all of his mathematical studies so far, which he had considered entirely up to date, had really taken him only to the threshold of a new direction.[13] He promptly mastered the new mathematics, with the result that Newton is said to have referred persons asking him about his work to De Moivre, who knew it better than he did.[14] Through the astronomer Edmond Halley, De Moivre was properly introduced to Newton and as well to the scientific society of London, leading to his election to the Royal Society. He made himself available to Newton in a variety of capacities: he sent

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[10] The collection is *Miscellanea analytica de seriebus et quadraturis* (London, 1730), dedicated to Folkes. The list of subscribers could serve as a guide to British mathematics and its patrons in the early eighteenth century.

[11] Mary Berry (1819, 73).

[12] Father and son, "Abraham and Daniel De Moavre," are listed as being in London as of 16 December 1687, in a request to the attorney or solicitor general to prepare a bill for royal signature making them free denizens of the kingdom. Cooper (1862, 50). Samuel Smiles (1868, 235–238).

[13] Maty (1760, 6–7). Although the *Principia* was published in the summer of 1687, there is no evidence that Newton came to London to distribute copies of it at that time, and Edmond Halley handled the presentation copies. Moreover, it would have been of no advantage to him that summer to seek Devonshire's patronage, since he was then out of favor at court, having taken refuge at Chatsworth to avoid being arrested by the king in 1688. By 1689, however, James II had been displaced by William and Mary, at whose court Devonshire had a great deal of influence.

[14] Ian Hacking (1974, 452).

news and results of Newton's work to colleagues abroad;[15] he took charge of Newton's publications;[16] he defended Newton;[17] and he kept philosophical company with Newton at the Rainbow or Slaughters' coffeehouse and elsewhere.[18] De Moivre's own work drew heavily on Newton's, as he acknowledged by dedicating to him his masterwork, a treatise on probability, *Doctrine of Chances*. We can estimate when Cavendish probably studied with De Moivre, the friend of Newton, Halley, and other prominent British scientists, and correspondent of leading mathematicians on the Continent. De Moivre wrote to Leibniz in 1710 that most of his students were adolescents, and if that applied to Cavendish, he would have been with De Moive soon after he left Eton, before he went on his grand tour, sometime in the early 1720s.

In the course of his teaching, De Moivre established extensive and remunerative connections with the Whig aristocracy. It has been suggested that the connections began with De Moivre's call on the earl of Devonshire, as related by Maty above. Newton was probably there on political business, and De Moivre may have been there for the same reason, bearing a letter of introduction from a Huguenot friend. After the meeting with Devonshire, De Moivre presumably was taken on as tutor to Devonshire's sons, William and James. The eldest son William, who became the second duke of Devonshire and Charles Cavendish's father, was closely associated with Robert Walpole, the Whig prime minister and one of the subscribers to De Moivre's book. If this is how it went, De Moivre's entry into the Whig political world came about through the earl of Devonshire and was "tied to events surrounding the 1688 revolution."

Mathematical tutoring served an assortment of ends. It constituted a finishing school for "gentlemen," which probably would not have attracted Cavendish. Nor would have other common ends such as providing a useful skill for persons who sought public office but lacked the advantage of rank,[19] preparing government officials for handling finance, preparing teachers and others who intended to make a living directly from mathematics, and equipping landowners for surveying and military officers for navigation and gunnery. Instead it helped prepare Charles Cavendish for scientific research and administration.

Most of De Moivre's mathematical friends and pupils will enter this biography again as leading members of the Royal Society.[20] Here we briefly discuss two of them, William Jones and George Parker, second earl of Macclesfield. William Jones was a second mathematics teacher Cavendish may have studied with. It was Jones's practice to hand out transcripts of Newton's mathematical writings to his pupils, and Cavendish owned a copy of Jones's transcript of Newton's "Artis Analyticae Specimina vel Geometria Analytica."[21]

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[15] For example, concerning copies of Newton's *Principia* promised by De Moivre: letters from Pierre Varignon to Newton, 24 Nov. 1713, and from Johann Bernoulli to G.W. Leibniz, 25 Nov. 1713; in A.R. Hall and L. Tilling (1976, 42–45).

[16] David Brewster (1855, 248). Schneider (1968, 212–213).

[17] In Newton's dispute with Leibniz over the invention of the calculus. Hacking (1974, 452).

[18] Frederick Charles Green (1931, 31).

[19] A.J. Turner (1973, 51–54).

[20] For example: in addition to Newton, Folkes and Macclesfield were presidents of the Royal Society; Cavendish, Jones, Davall, Scott, and Stanhope were members of the Council; Maty and Taylor were secretaries; Halley was corresponding secretary and editor of the *Philosophical Transactions*.

[21] Cavendish later loaned his copy of the transcript to the mathematician Samuel Horsley, who was preparing a general edition of Newton's papers. D.T. Whiteside in Isaac Newton (1967–1969, 1:xxiii; 8:xxvii).

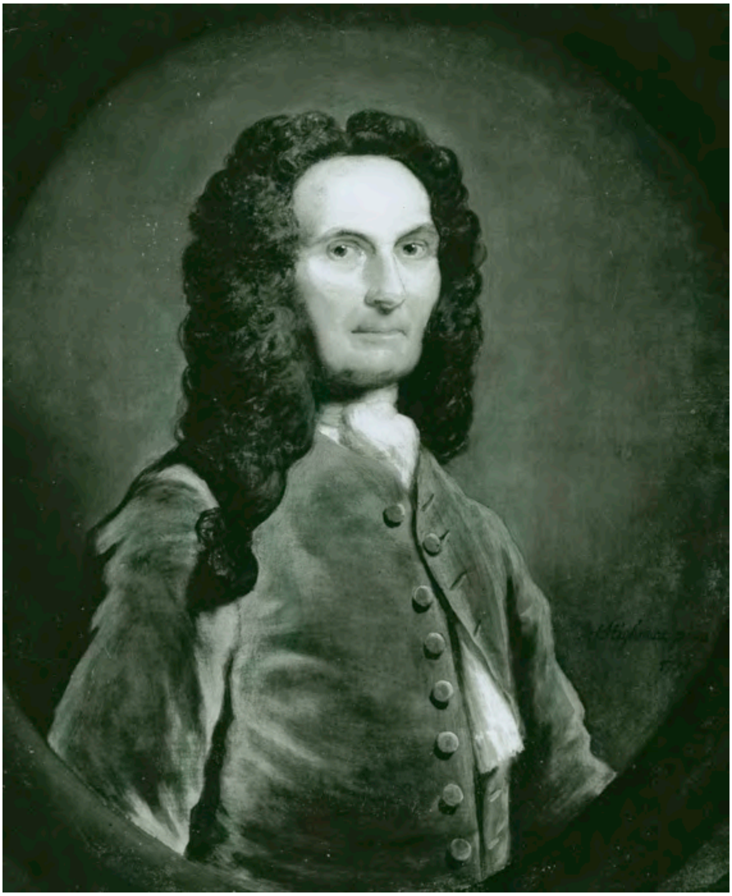

Figure 3.1: Abraham de Moivre. Painting by Joseph Highmore, 1736. Reproduced by permission of the President and Council of the Royal Society.

Jones published a book on navigation and another book, a syllabus of mathematics, which drew the attention of Halley and Newton, both of whom became his friends. Elected to the Royal Society in 1712, he was one of its more active members. As a tutor in mathematics, he became friends with Philip Yorke, later earl of Hardwicke and lord chancellor, and traveled with him on his circuit. He taught Thomas Parker, first earl of Macclesfield as well as his son, and for many years he lived with the Parker family at Shirburn Castle. On Macclesfield's recommendation Jones was appointed deputy-teller to the exchequer. He published a number of original papers in the *Philosophical Transactions*, edited important tracts of Newton's, and served with De Moivre on the committee of the Royal Society on the discovery of the calculus. He intended to write an introduction to Newtonian philosophy but died before he completed it. His library of mathematical books was reputed to be the best in the country.

Like De Moivre, Jones was an important personal and scientific link between Newton and the scientific men coming after him, including Charles Cavendish.[22]

Macclesfield was the other aristocrat besides Cavendish to be listed by Maty as a pupil of De Moivre's. Macclesfield's father the lord chancellor was impeached by the House of Lords under a long list of articles, which taken together include most of the ways money can be misused. He procured for his son an appointment as a teller of the exchequer for life. Like his father, Macclesfield studied law and became a Member of Parliament, but his first interest was always the sciences. He studed under both De Moivre and Jones, and he may have profited from still another Newtonian teacher, Richard Laughton, who was at Clare Hall, Cambridge when he studied there. Elected fellow of the Royal Society in 1722, he served on its Council while Newton was still president. In 1752 he succeeded Folkes as president. That year he was instrumental in bringing about a practical application of astronomy, a change in the calendar, assisted by former pupils of De Moivre's: Davall who drew up the bill and made most of the tables, and Folkes who examined the bill. In the calendar then in use, the new year began on 25 March; in the new style calendar, it began on 1 January, and there was a correction for the accumulated errors in the calendar owing to the precession of the equinoxes, a one-time elimination of eleven days in September. (When running for a seat in Oxfordshire, Macclesfield's son was met by a mob crying, "Give us back the eleven days we have been robbed of.") Macclesfield's private astronomical observatory in Shirburn Castle was said to have had the best equipment of any. He published three papers in the *Philosophical Transactions*, all minor: one on the date of Easter, one about an eclipse of the Sun, and one about the temperature in Siberia. His importance for science was as an administrator and patron.[23]

Royal Society

Early in June 1727, De Moivre's friend William Jones proposed the twenty-three-year-old Charles Cavendish for fellowship in the Royal Society , and two weeks later, on 22 June, he was formally admitted.[24] At a meeting of the executive Council of the Society on that same day, its president, Hans Sloane, raised the question of qualifications for admission of new members. Under English law, sons of peers were commoners until they inherited the family title, but in the Royal Society, by statute as a son of a peer, Cavendish was treated as if he were a peer, having to furnish no proof of scientific achievement, ability, or even interest. To raise the standards of membership of the Society and to reduce the exceptions to the general rules of admission, Sloane proposed to treat all commoners the same way with respect to requirements. The issue came to a head a few months later, in February 1728, when William Jones proposed yet another son of a peer, whereupon the members at large engaged in "Debates arising upon the sense of the Statute with Relation to peers Sons and privy Councellors whether any other Qualifications of such Gentlemen are required

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[22] "Jones, William," *DNB,* 1st ed. 10:1061–62. E.G.R. Taylor (1966, 293–294). "Jones (William)," in Charles Hutton (1795–1796, 1:43–644).

[23] "Parker, Thomas, first Earl of Macclesfield," *DNB*, 1st ed. 15:278–282, on 280. "Parker, George, second Earl of Macclesfield," ibid. 15:234–235. Brydges (1812, 4:192–194). Charles Richard Weld (1848, 1:514–516). Maty (1787, 696).

[24] 8 June 1727, JB, Royal Society 13:103.

to be mentioned or not." In the event, the Society changed some of its requirements for membership, but let stand those for peers and sons of peers.[25]

Until the end of his life, Newton was active as president of the Royal Society, and when he was absent, Folkes or Sloane took the chair in his place. Newton died three months before Cavendish was admitted to the Royal Society, but his presence was still felt. Several members of the Council were his friends and, as we have noted, De Moivre's friends too. One of them, the astronomer Halley, was especially active in scientific discussions at the meetings. Folkes, Jones, and the astronomer James Bradley were on the Council, as were the two secretaries of the Society, the physician and polymath James Jurin, a pupil of Newton's, and John Machin, an astronomer who Newton thought understood his *Principia* best of anyone, and who with Halley and Jones had been appointed to the committee on the invention of the calculus. Other Council members who had a close association with Newton were Richard Mead, a physician and author of a Newtonian doctrine of animal economy; Thomas Pellet, a physician who with Folkes brought out an edition of Newton's *Chronology of Ancient Kingdoms* in the year after Newton's death; Henry Pemberton, who edited the third edition of Newton's *Principia*; and John Conduitt, the husband of Newton's niece. Sloane was a physician, natural historian, and good friend of Newton's and Halley's. Several members of the Council were physicians with scientific interests: John Arbuthnot, Paul Bussiere, James Douglas, and Alexander Stuart. Roger Gale was a commissioner of excise. The one peer, Thomas Foley, who was repeatedly elected to the Council, had an observatory at his country seat near Worcester, from where observations were sent to the Royal Society from time to time. Two members of the Council represented a distinctive British contribution to science in the eighteenth century, the making of scientific instruments: John Hadley, who was first to develop the reflecting telescope introduced by Newton, and who later introduced a reflecting octet based on a proposal by Newton; and George Graham, to whom Bradley later said that his own success in astronomy had "principally been owing."[26] The governance of the Royal Society was entrusted to the users and makers of scientific instruments and to a good number of able mathematicians. This diverse and, by and large, eminent group of scientific men on the Council enlarged Cavendish's world in 1727. Later he would serve with seven of them on the Council.

Historians are divided over the question of the quality of science in the Royal Society in the eighteenth century,[27] but there would seem to be no doubt that from the standpoint of experimental science, 1727 was an auspicious year for the Society. That year Stephen Hales brought out *Vegetable Staticks*, the most impressive demonstration yet of the promise of Newton's philosophy to clarify a new experimental domain of facts. Educated at Cambridge, where he began experimenting on animal physiology, Hales continued his scientific studies while earning his living as a provincial cleric. With the help of Newton's speculations about forces of attraction and repulsion between particles, contained in the Queries of his *Opticks*, Hales investigated the composition of plants and the air "fixed" in plants. In the chapter of *Vegetable Staticks* concerned with air, Hales went beyond his original inquiry into plants to conclude that air is in "all Natural Bodys" and is "one of the Principal Ingredients or Elements in the Composition of them." His experiments on fixed air helped lay the foundations of pneumatic chemistry, the field in which Charles Cavendish's son Henry would

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[25] 8 Feb. 1727/1728, JB, Royal Society 13:175. Weld (1848, 1:461).

[26] Bradley in 1747, quoted in Taylor (1966, 120–121).

[27] Richard Sorrenson (1996, 29–30).

make his major contribution. The full significance of *Vegetable Staticks* could not have been foreseen—it was to encourage a generation of experimentalists—but it was valued from the beginning. Hales was included in the Council of the Royal Society at the next election, at the end of 1727. Newton, who had presided during the final reading of Hales's chapter on air, died five weeks later, shortly before his hand-picked experimenter, J.T. Desaguliers, demonstrated experiments from that chapter,[28] one of which falling on the day Cavendish was elected to the Society. At that meeting, the president of the Royal Society Hans Sloane said that he and Abraham Hill had decided that the £5 interest on the £100 legacy of Godfrey Copley's hereafter would be paid to a person to perform an "Annual Experiment" before the Society.[29] Four years later, in 1731, Copley's legacy was used to award the first Copley Medal to the author of the "most important scientific discovery or contribution to science, by experiment or otherwise."[30] Both Charles and Henry Cavendish would receive the medal.

To follow Charles Cavendish's education in science, we look at the kinds of subjects that came up in the meetings around the time of his election, beginning with practical schemes. In 1627, exactly 100 years before Cavendish entered the Royal Society, Francis Bacon published his scientific utopia, *New Atlantis.* Salomon's House, Bacon's projected cooperative scientific college, whose goal was the "effecting of all things possible," was the original inspiration for the Royal Society. The expectation was that the Royal Society, like Salomon's House, would advance human welfare through science. That a century later the claims for the utility of the Royal Society could still be seen as utopian is shown by Jonathan Swift's satire of it in *Gulliver's Travels*, published one year before Cavendish entered the Royal Society. The Royal Society, renamed by Swift the Academy of Lagado, labors to extract sunbeams from cucumbers to warm the air on cold days.[31] The source of this ridicule was probably Hales's experiments on the effect of sunlight on the respiration of plants, which had been read to the Royal Society before being collected in his *Vegetable Staticks*.[32] Swift, to whom the disparity between the utopian faith of improvement and the hard reality of life was self-evident, was repelled by the Baconian optimism of the Royal Society. Whatever its logic, Swift's satire was overtaken by events. At a meeting of the Royal Society three months before Cavendish became a member, a letter was read from the secretary of the newly founded scientific academy at Petersburg, giving the plan of the academy, which largely followed the plan of the academies in Paris and Berlin, which in turn had benefited from the original academy, the Royal Society of London. Like its predecessors, the Petersburg academy would seek to promote human betterment by improving medicine and encouraging inventions.[33] Scientific academies with their Haleses—Stephen Hales was an avid applier of science as well as a plant and animal physiologist—would have a permanent presence in the Cavendishes' world and in the world to come.

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[28] Stephen Hales (1727). Henry Guerlac (1972, 35–36, 41–43). References to the reading of Hales's discussion of air and to Desaguliers's repetition of experiments from it: 2, 9, 16 Feb., 13, 20 Apr., 4 May, 8 June, 16 Nov. 1726/1727, JB, Royal Society 13:44–45, 48–50, 70 , 74, 83, 103, 144. Newton's death caused a cancellation of the Society's meeting on 23 Mar. 1726/1727, JB, Royal Society 13:62.

[29] 8 June 1727, JB, Royal Society 13:99–100.

[30] The criteria of the award have been stated variously at different times. It remains the oldest and most prestigious award of the Society.

[31] Jonathan Swift (1726/1962, 177).

[32] It is widely thought that Hales was Swift's source, though evidently it is not proven. Clive T. Probyn (1978, 148).

[33] 2 Mar. 1726/27, JB, Royal Society, 13:52.

Long practiced in the East, inoculation against smallpox had just been introduced in Britain when Cavendish entered the Royal Society. The eminent physician and secretary of the Royal Society James Jurin warmly supported inoculation in the face of opposition from doubting physicians and clerics. The operation posed a risk to the community as well as to the patient, but so did the disfiguring and killing epidemic disease, and Jurin argued with figures that the danger from inoculation was less than that from exposure. After the operation had been tried, at royal request, on several condemned criminals, without loss of any, the royal children were inoculated.[34] It is unknown if Cavendish was inoculated, but he certainly was exposed; at the time he and James went abroad, their sister Elizabeth wrote that "the small pox continued here very fatal."[35]

Inoculation was based on an empirical observation—a mild form of smallpox often prevented a serious infection—insuring that it would become a topic of interest in the Royal Society. From far and near, Jurin received reports of inoculations written down methodically in columns, like weather reports, with which they had a connection. Despite Jurin's best efforts, inoculation fell into disfavor in Britain owing to deaths in prominent families. It revived in the 1740s as a remunerative surgical practice, but the Baconian promise began to be realized only at the end of the century, when the English physician Edward Jenner introduced cowpox vaccination, a safe method of controlling smallpox, which he came upon in the course of his practice of giving original smallpox inoculations. George III, who was roughly Henry Cavendish's age, was given Jenner's cowpox vaccination. Medicine was a large concern of Charles Cavendish's Royal Society, and though it did not happen to be one of his own, he was an active and longtime governor of the Foundling Hospital where his good friend William Watson regularly gave smallpox inoculations to children over three (Fig. 4.7).[36]

Inventions came up repeatedly at meetings of the Royal Society. For industry and for domestic heating, coal was increasingly in demand. British forests, the source of firewood and charcoal, were becoming depleted, encouraging the use of coal as the alternative for domestic heating and industry. Mining coal was hazardous because of the accumulation of unhealthy and inflammable air in the pits. Two weeks before Cavendish's election to the Royal Society, as the annual Sir Godfrey Copley's Experiment, the curator of experiments Desaguliers reported on his invention to remove bad air from mines and demonstrated it with a working model.[37] Through a sister, Charles would become involved in the coal mines of Sir James Lowther, who brought samples of air from his mines to the attention of the Royal Society.

Navigation was a natural subject for the Royal Society, joining science, invention, and national welfare. Ships were lost or delayed because navigators did not know their position

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[34] King George I allowed two of his grandchildren, the children of the future George II, to be inoculated in 1722, and they survived. However, two of King George III's children did not; about three percent of those inoculated did not. Susan Flantzer, "August 20, 1783—Death of Prince Alfred, Son of King George III of the United Kingdom" (http://www.unofficialroyalty.com/featured-royal-date-august-20-1783-death-of-prince-alfred).

[35] Elizabeth Cavendish to James Cavendish, 24 Apr. [1721], Devon. Coll., No. 166.1.

[36] 7 Dec., 7, 21 Mar., 11 Apr. 1727/28, JB, Royal Society 13:148, 191, 198, 210. "Jurin, James," *DNB,* 1st ed. 10:1117–18, on 1118. Leonard G. Wilson (1973, 96). William H. McNeill (1993, 249–250). After 1800, smallpox mortality in London fell to one half of what it had been in the eighteenth century. Charles Creighton (1965, 479–481, 504, 568).

[37] Desaguliers published the experiments on his model pump for removing bad air from mines in the *Philosophical Transactions of the Royal Society of London* 34 (1727). Hereafter *PT.*

relative to the neighboring land, a scientific and practical problem. A ship's latitude could be found by taking the altitude of the Sun or a star, but its longitude was not that simple. The astronomer royal Halley, an advocate of the lunar method of determining longitude at sea, criticized a book on longitude referred to him by the Society: the author, Halley said, made two mistakes, one in thinking his method was original, the other in assuming what did not yet exist, "a true Theory of the Moons Motion." Later Charles Cavendish advised on an alternative to the lunar method, a marine clock, discussed in the section on his scientific work below. Other practical problems of the sea such as measuring its depth and mapping its coast came up at meetings of the Society.[38] One of Charles Cavendish's self-registering thermometers was suited for measuring the temperature of the sea at considerable depths, and under Henry Cavendish's supervision, it was used for that purpose.

The atmosphere of the Earth was another kind of fluid of practical importance and scientific interest. In 1723 James Jurin, secretary of the Royal Society, invited uniformly recorded weather observations—date, time, thermometer, barometer, wind, and general observation of weather[39]—and around the time of Cavendish's election, the Society received observations of everyday weather in considerable numbers. In addition it received occasional observations of remarkable atmospheric events such as great cold spells and auroras.[40] The weather was one of Charles Cavendish's persisting scientific interests, as it would be Henry's later.

Like Hales's fixed air, electricity was a relatively new field of experimental study in the early eighteenth century. It had no immediate utility yet, but it posed scientifically curious questions. Desaguliers alternated his demonstration of Hales's experiments on air with experiments on the communication of electrical virtue to a glass, demonstrated by the attraction and repulsion of fibers of a feather and of gold leaves. Within a year of Cavendish's election, Desaguliers announced that Stephen Gray intended to bring before the Society experiments showing that rubbed glass communicates its electrical quality to any body connected to it by a string.[41] Cavendish would make valuable experiments on the conduction of electricity, as again would Henry.

The breadth of topics discussed at the Royal Society around 1727 was greater than these examples suggest. For instance, from the side of medicine, there were reports on stones, cataracts, and aneurysms. From the side of natural history (and the far-flung British colonies), there were reports on coconuts, cinnamon, and poison snakes, and fossils, curious specimens such as two headed calves, and various natural collectibles were regularly displayed at the meetings. Investigative reports of earthquakes and other singular natural disasters were heard as often as opportunity allowed. Apart from certain formalities—correspondence read, books received, and guests introduced—the meetings were kept reasonably lively by the variety of their proceedings. A fairly typical meeting from around the time Cavendish was elected to the Society was recorded in a private journal kept by John Byrom, a fellow of the Royal Society and frequent attender: "Vernon there from Cambridge; Dr. Rutty read about ignis fatuus; humming bird's nest and egg, mighty small; Molucca bean, which somebody had sent to Dr. Jurin for a stone taken out of a toad's head; Desag-

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[38] 11 May, 29 June 1727, JB, Royal Society 13:84–85, 113; 25 Jan. 1727/28, 2 May 1728, 23 Jan. 1728/29, ibid., 168–169, 214, 287. Humphrey Quill (1966, 1–6).

[39] William E. Knowles Middleton (1969, 138).

[40] 12 Jan. 1726/27, JB, Royal Society 13:34–36, and many other places.

[41] 27 Feb., 13 Mar., 1 May 1728/29, ibid., 307, 316, 330.

uliers made some experiments about electricity."[42] That evening there was something for just about everybody.

The contents of the *Philosophical Transactions of the Royal Society of London* are not identical with the papers read at meetings of the Society, but they give an idea of what went on. In the decade of the 1720s, when Cavendish entered the Society, the numbers of papers on natural history and on mixed mathematics (scientific fields with mathematical content but not pure mathematics) were about equal, together accounting for about half of the total number of papers. Medicine came next, accounting for about a fifth of the papers, then experimental natural philosophy and anatomy, each with above a tenth, and there were a few other categories such as speculative natural philosophy, pure mathematics, and antiquities. The two categories to which Cavendish's work belonged, mixed mathematics and experimental natural philosophy, accounted for one third of the papers, a proportion which did not change much over the next fifty years, into the time when Cavendish's son Henry was active in the same areas.[43]

The Royal Society wore two crowns, one scientific and one royal. Newton lived on in the causes that continued to be championed in his name. Thomas Derham wrote to the Society from Rome about a book by an Italian who "pretends" to refute propositions in Newton's *Opticks*; Desaguliers responded to the perceived danger. The dispute over whether the measure of force is as the velocity, as Newton said, or as the square of the velocity, as foreign mathematicians said, was settled by Desaguliers (he thought) by experiment and clarified by Jurin, who regarded it as a dispute arising from an ambiguity in the meaning of the word "force." Andrew Motte presented to the Society his English translation of Newton's *Principia*, and William Jones was asked to give the Society an account of it.[44] In the year Newton died, King George I died, and his successor to the throne, George II, agreed to succeed him as patron of the Royal Society. The change in monarch entailed protocol, such as carrying the charter book to St. James's for the royal signature, making an address, and paying compliments to the queen. There was also a change of heir to the crown, Prince of Wales Frederick, to whom the volume of the *Philosophical Transactions* for 1728 was dedicated. That year Cavendish became gentleman of the bedchamber to Frederick.[45]

Directly below the rank of royalty, within the dukedom of the Devonshires, there was about to be another succession, but for the time being Cavendish's father, the second duke of Devonshire, was still alive. The duke was the owner of a great magnet, which turned up in discussion at the Royal Society a few months after Cavendish was elected. Supported in a fine mahogany case and raised by screws, the "famous Great Lodestone of his Grace the Duke of Devonshire" had prodigious force, as Folkes bore witness, having seen it lift "more than its own weight."[46] In 1730 the magnet was produced again, this time by Desaguliers, who lifted 175 pounds with it.[47]

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[42] Entry for 27 Feb. 1728/1729: R. Parkinson, (1854–1857, vol. 1, pt. 1, 334). 27 Feb. 1728/1729, JB, Royal Society 13:303–307.

[43] Sorrenson (1996, 37). From another source, there is a similar estimate: physics, including mechanics, meteorology, and various border subjects, accounted for about a third of the papers appearing in the *Philosophical Transactions*. John L. Heilbron (1983, 43).

[44] 8 Feb., 4 July, 24, 31 Oct., 7, 14 Nov. 1727/28, JB, Royal Society 13:175–176, 242, 252, 257, 262; 22 May, 5 June 1729, ibid., 339–340, 341.

[45] 11 May, 6 July 1727, ibid., 86, 114.

[46] 13 Mar. 1728/1729, ibid., 314.

[47] 9 Apr. 1730, ibid., 454.

Encouraged to learn that the king of France had just instituted a medical society, Heberden wrote to a colleague that “the knowledge of other parts of nature has increased more, by means of such societies, within the last hundred years, than it had done from the age of Aristotle to the time of their foundation.”[48] To judge by their work in the Royal Society, Charles and Henry Cavendish would have agreed with their friend on the importance of scientific societies for the improvement of scientific understanding.

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[48] William Heberden to Charles Blagden, 9 Dec. 1778, Blagden Letters, Royal Society, H.22.

Chapter 4
Family and Friends

Marriage and Money

On 9 January 1729, Lord Charles Cavendish married Lady Anne de Grey, daughter of the duke of Kent. Charles was in his middle twenties instead of in his middle thirties, a more common age for younger sons of nobility to marry,[1] and Anne, who was born in 1706, was two years younger. We know nothing of the affection between Charles and Anne, but certainly wealth, rank, and respectability would have been considerations in this match. There were earlier connections between the two familes too: as we saw in Chapter 1, Charles's and Anne's fathers came together on a Continental tour, and at the beginning of the previous century, Henry Grey, earl of Kent, married Elizabeth, granddaughter of Sir William Cavendish of Chatsworth.[2]

We begin this account of the new family with what we can speak of with confidence, money. Younger sons of the aristocracy customarily received £300 a year, which is what Charles received since 1725. His father intended for the annuity to be raised to £500 at his death, but he moved to plan ahead starting with Charles's marriage. In addition his father granted him the interest on £6000 and eventually the capital itself.[3]

The marriage settlement of Charles and Anne involved land as well as money. Following a practice that had been more common in the seventeenth century than in the eighteenth, the second duke of Devonshire devolved property on Charles and his heirs: tithes, rectories, and lands in Nottinghamshire and in Derbyshire. Charles received the rents in 1728 and the lands the following year. At the beginning the rents brought in somewhat over £1000 a year (out of which there were expenses), and after the enclosures of the 1760s and 1770s they increased considerably. Beyond the welcome income, Charles's property brought him intangible benefits in a society, in which "men were measured by their acres."[4]

At the time of his marriage, Charles had a substantial residence on Grosvenor Street off Grosvenor Square, a fashionable location in Westminster.[5] The marriage settlement enabled

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[1] Lawrence Stone (1982, 42).

[2] George Edward Cokayne (1982, 3: cols. 173–174).

[3] Charles had just turned twenty-one when on 6 April 1725 his father settled on him a £300 annuity. He had use for it, for one week later he was returned as M.P. for Heytesbury. The £6000 paid 3.5% interest. The £500 annuity and the £6000 capital were determined by an earlier settlement, in 1678. Devon. Coll., L/13/9, L/19/31, L/19/33, and L/19/34.

[4] Devon. Coll., L/19/33. H.J. Habakkuk (1950, 15–16, 18, 20–24). J.H. Plumb (1963, 72).

[5] Charles Cavendish appears on the poor rolls of Westminster Parish of St. Margaret's in 1728, paying £5.5.0 annually, the same as the duke of Kent, his father-in-law, who had a house in the parish. Westminster Public Libraries, Westminster Collection, Accession no. 10, Document no. 343. Charles's address in 1729–32 was 48 Grosvenor Street, a three-story, brick, terrace house, with four windows on each floor, and with touches of elegance: extensive panelling, marble chimney pieces, and a "Great Stair Case" in the entrance hall. British History Online, "Grosvenor Street South" (http://www.british-history.ac.uk).

Charles and Anne to acquire a country residence as well. Securities worth £12,000 and £10,000—Charles's due from his mother's estate and Anne's portion—were transferred to the trustees, who raised a sum for the purchase of the estate of George Warburton's. This consisted of three manners, Lilley, Hackwellbury, and Putteridge, which Charles and Anne made their home, together with several farms, located directly north of London, at about half the distance of Cambridge, in the adjacent counties of Bedford and Hertford.[6] There was another provision in the married settlement from which Charles would benefit eventually: after the duke of Kent died—he died in 1740—Charles would receive interest on £12,000 left to Anne's trustees.

From the time of his marriage, Charles could probably count on an annual income of around £2000. We get an idea of what this income meant from Samuel Johnson, a professional man who rarely made above £300, who said that £50 was "undoubtedly more than the necessities of life require." A gentleman was said to live comfortably on £500 and a squire on £1000.[7] Cavendish's income enabled him to live comfortably, acquire books for his library, and pursue his scientific interests. Within the conventional financial arrangements of wealthy English families, the Cavendishes and the Greys combined to create what was in effect a modest scientific endowment for Charles.

In addition to his active life in the city, at court, and in Parliament, Charles took on responsibilies in the Royal Society, serving on his first committee two years after his election.[8] The portrait of him included in this book gives us an idea of what he looked like around then (Fig. 4.1). There are two portraits of Anne, one of her together with two sisters, and one of her by herself and somewhat older (Fig. 4.2). Like Charles, she was slender, with distinctive features: large eyes, high rounded eyebrows, and dark hair. At the time of the portrait, she was evidently in good health, which was not to last. There is evidence that she was not strong before her marriage; in the summer before, the house account for the duke of Kent repeatedly recorded "Chair hire for Lady Anne," while none of the duke's other daughters required chairs.[9] In the winter of the following year she definitely was ill. Sophia, duchess of Kent, her stepmother, wrote to her father, the duke, that she had just dined at the Cavendish's: "Poor Lady Anne does not seem so well as when I saw her last. Her spirits are mighty low and she has no stomach at all. She has no return of spitting blood nor I don't think she coughs more than she did so that I hope this is only a disorder upon her nerves that won't last."[10] The next winter, 1730–31, was bitterly cold, colder, William Derham reported to the president of the Royal Society, than the winter of 1716, when the Thames froze over.[11] That winter, we believe, Charles and Anne went abroad, possibly in the company of his brother James.[12] From Paris, Anne wrote to her father that in Calais she had been very ill with a "great cold" and that she had been blooded and kept low to prevent

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[6] Devon. Coll., L/19/33 and L/5/69.

[7] George Rudé (1971, 48, 61).

[8] On 17 July 1729, Cavendish was appointed to a committee to inspect the library and the collections. It met every Thursday from 24 July until 6 Nov. 1729, and on 11 Dec. it was ordered to continue its work. Minutes of Council, Royal Society 3:28–30, 34–36, 39, 55–56, 114–116.

[9] July 1728. House Account. To ye 28 December 1728," Bedfordshire Record Office, Wrest Park Collection, L 31/200/1.

[10] Sophia, duchess of Kent to Henry, duke of Kent, 21 Feb. 1729/1730, Bedfordshire Record Office, Wrest Park Collection, L 30/8/39/5.

[11] William Derham (1731/1733).

[12] James was at least abroad at the same time as Charles. On 10 Oct. 1731, James "came to Town from France." *Weekly Register,* Oct. 16, 1731. BL Add Mss 4457, 76.

fever. She did not expect to see much of Paris for fear of being cold, and in any case they were about to leave the city for Nice.[13]

They would not have gone there as conventional tourists, for although Nice did become popular with English tourists, this did not happen until the second half of the eighteenth century. In 1731, Charles Cavendish was the only Englishman to stay in Nice who did not have commercial or diplomatic ties there, the only permanent English resident being the consul, who did double service as a spy on the French.[14] Owing to the combination of Sun and sea, Nice was considered a suitable location for people convalescing from lung ailments,[15] in all likelihood the reason Charles and Anne went there.

Perhaps her health did improve. In any case, about three months after leaving Paris, Anne conceived; in Nice on Sunday, 31 October 1731, she gave birth to her first child, named after her father, Henry de Grey. No birthplace could have been less predictive of the outcome: beginning life in a sleepy Mediterranean town of about 16,000 inhabitants situated among olive groves, Henry Cavendish grew up to be one of the most confirmed Londoners ever (Fig. 4.3).

In anticipation of Henry's birth, Charles asked the British consul at Turin for help in obtaining permission from the duke of Savoy for "one of the Vaudois Protestant Ministers" to come to Nice to baptize the infant. No doubt Charles knew that the closest region in which the Protestant religion could be practiced was the valleys of the Vaudois in Piedmont. There was a family connection, if coincidental: the Vaudois Protestants, historically a persecuted group, kept in close touch with another persecuted Protestant group, the Huguenots, to whom Charles was related through the Ruvignys. Cyprian Appia, who with his brother acted as chaplain in the British embassy in Turin, and who had studied at Oxford and was ordained as an Anglican priest, was sent to Nice on 15/26 October 1731. His services were performed under the express condition that the "baptism should be performed in a manner as little publick as well might be," reflecting the reserve of Charles and Anne, a trait which would be intensified in Henry Cavendish.[16]

The next stage of Charles and Anne's marriage is brief and ends sadly. A year and a half after their arrival on the Continent, they were back in France. From Lyon in the summer of 1732, Anne wrote to her father about her health and happiness. It was with her usual perfected penmanship, the letters large, uniform, and inclined at precisely the same angle, but her hand was unsteady, like that of an elderly person. Yet her fever had not returned, and she was so far recovered that she and Charles were going to Geneva the next day, for a three-day journey. If she handled that well, they would stay there two or three days and then go directly to Leiden. She closed the letter with word of her baby, Henry. "I thank God," she wrote, "my boy is very well and his being so very strong and healthy gives me a pleasure I cannot easyly express."[17]

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[13] Anne Cavendish to Henry, duke of Kent, 4 Nov. [1730], Bedfordshire Record Office, Wrest Park Collection, L 30/8/11/1.

[14] Henri Costamagna (1973, 26). Daniel Feliciangeli (1973, 55–56). Anon. (1934, 660–663).

[15] "Nice," *Encyclopedia Britannica* (Chicago: William Benton, 1962) 16: 414–15).

[16] Sugiko Nishikawa (1997).

[17] Anne Cavendish to Henry, duke of Kent, 22 June [1732], Bedfordshire Record Office, Wrest Park Collection, L 30/8/11/2.

The Scientific Branch of the Family

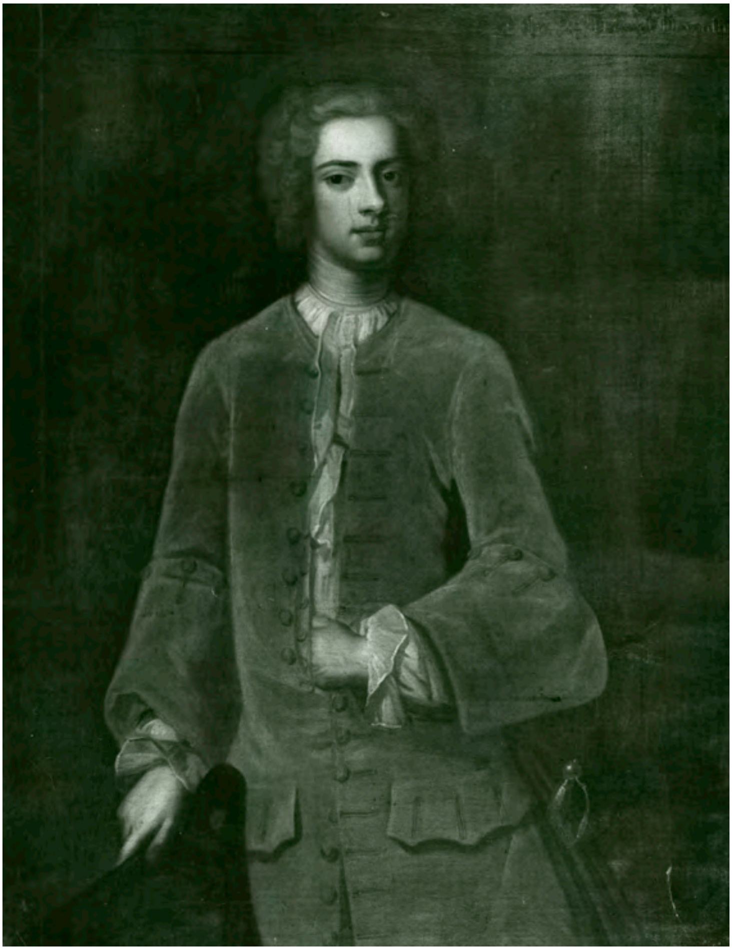

Figure 4.1: Lord Charles Cavendish. Father of Henry Cavendish. By Enoch Seeman. Devonshire Collection, Chatsworth. Courtesy of the Chatsworth Settlement Trustees. Photograph Courtauld Institute of Art.

Figure 4.2: Lady Anne de Grey. Mother of Henry Cavendish. By J. Davison. Courtesy of the Bedfordshire Record Office.

They were going to Holland to see the great teacher and healer Herman Boerhaave. Nearing the end of his career at the University of Leiden, where he taught medicine and until recently botany and chemistry, he was still giving clinical instruction in 1732. Having written major treatises on medicine, he was by many accounts the most famous physician in the world. From all parts, but especially from Britain where his ties were close, students came to Leiden to attend his lectures: of the nearly 2000 students enrolled in Leiden's medical faculty, fully one third were English-speaking. British physicians who had studied under Boerhaave consulted him when their treatment of important patients had not succeeded, and British travelers included Leiden on their itinerary just to meet him.[18] Boerhaave returned the compliment: an ardent admirer of British experimental science, he was one of the first exponents of the Newtonian philosophy in Europe. Anne told her father that they thought it would be right for Dr. Boerhaave to "see me pretty often in order to make a right judgment of my illness." Since we have no other letters by her, we do not know what Boerhaave decided and prescribed.[19] Tuberculosis was a common disease for which medicine then had no cure.

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[18] Bolingbroke wrote to his half sister Henrietta, "I was yesterday at Leyden to talk with Doctor Boorehaven, and am now ready to depart for Aix-la-Chapelle." Letter of 17 August 1729, in Walter Sydney Sichel (1968, 525).

[19] Anne Cavendish to Henry, duke of Kent, 22 June [1732]. G.A. Lindeboom (1974, 18); (1970, 227–228).

Figure 4.3: The Honorable Henry Cavendish. Engraving by John Weale from a graphite and gray wash sketch by William Alexander. Cavendish refused to sit for a portrait. To get around this, Alexander, a draftsman in the China embassy, attended a dinner of the Royal Society Club, where he surreptitiously sketched Cavendish's profile and separately sketched his coat and hat hanging on the wall. At home, he combined the two sketches into one. Persons who were shown it recognized Cavendish. Frontispiece of George Wilson (1851).

At some point Charles and Anne returned to England. Three months after her consultation with Boerhaave, Anne was well enough to conceive again, and on 24 June 1733 she delivered another son, Frederick. The next we hear is that Anne Cavendish died at Putteridge on 20 September 1733.[20] Henry was not quite two years old, Frederick was three months, and Charles was twenty-nine. For a man in his social position, remarriage was uncommon, and Charles would live for fifty years as a widower.

Although for Anne who had reached her twenty-seventh year, life expectancy was over sixty in the eighteenth century, life then at any age was precarious. Hygiene was unknown, medicine was largely helpless, and death was indifferent to privilege. Henry and his brother Frederick grew up with one parent, a not uncommon fate under the prevailing conditions of life.[21]

Family of the Greys

As a widower, Charles kept in touch with Anne's family. For this valuable fact we are indebted to Thomas Birch, who enjoyed the patronage of a branch of that family, the Yorkes. Philip Yorke, first earl of Hardwicke, engaged Birch as tutor to his oldest son, also named Philip. He then kept Birch on from 1735 as a secretary with light duties, leaving Birch with plenty of time for his calling, which was writing (Fig. 4.6).[22]

In 1740, Philip married Jemima Campbell, granddaughter of the duke of Kent. That same year the duke died, whereupon Jemima became Marchioness Grey and baroness Lucas of Crudwell. (Shortly before he died, the duke of Kent was made Marquess Grey with a remainder to his oldest granddaughter Jemima Campbell and her male heirs, establishing the only continuing title.) In the years to come, in the off-season Philip and Jemima lived at the duke of Kent's country estate Wrest Park in Bedfordshire, and the rest of the time in Kent's townhouse on St. James Square (Fig. 1.6). No match for his self-made father the lord chancellor, Philip rejected his ample opportunities for high political office, withdrawing into his chief pleasure in life, literature. He was personable, languid, reserved, and not robust, spending much of the day dressing, visiting, and reading long letters from Birch.[23]

Birch was personally close to the younger Philip Yorke, becoming his secretary, literary assistant, and eyes and ears in the wider world. Although Wrest Park appears frequently at the head of Birch's letters, his principal assignment was London, from which watch he kept his patron informed on literary affairs and also on science. Given Yorke's friends and membership in the Royal Society, Birch expected him to take an interest in, for instance, the test of a chronometer for determining longitude at sea. Jemima Yorke evidently took an interest in science too, for we find Birch writing to her about the contents of the *Philosophical Transactions*. When Philip and Jemima Yorke were in London, Birch joined them for weekly breakfasts at St. James Square.[24] The duchess of Kent was usually there along with Mary and

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[20] Four days later, on 24 September 1733, Anne Cavendish was buried in the Grey family vault at Flitton. "Extracts from the Burial Register of Flitton," Bedfordshire Record Office, Wrest Park Collection, L 31/43. We assume that she died of her lung illness, though it could have been related to giving birth.

[21] Stone (1982, 46–48, 54, 58–59).

[22] Albert E. Gunther (1984, 8, 35).

[23] There are many letters from Thomas Birch to Philip Yorke reporting on scientific news between 1747 and 1762, in BL Add Mss 35397 and 35399. Thomas Birch to Jemima, marchioness de Grey, 12 Aug. 1749, BL Add Mss 35397, ff. 200–201.

[24] Gunther (1984, 35–39).

Sophia de Grey and other members of the Grey family, including in-laws Lords Glenorchy and Ashburnham. In the presence of Birch, Charles Cavendish visited the Greys often in 1741 and 1742, though less often over the next ten years, sometimes bringing his son Henry to visit his maternal grandmother and aunts and uncles.[25] Henry Cavendish may not have had a memory of his mother, but his father made certain that he knew the other dukedom from which he descended.

Great Marlborough Street

In 1738, five years after his wife died, Charles Cavendish sold Puttteridge together with the rest of his country estate. To empower the trustees to make the sale, an act of Parliament was needed, and for that, a reason had to be given for wanting to sell; Cavendish said that Putteridge was too far from the rest of his estate. Parliament directed the trustees to sell the country estate for the best price possible.[26]

It would seem that the property sold for about what it had cost, and the price of the house Cavendish bought in its place that same year was only one tenth of that: for the absolute purchase of a freehold in Westminster, he paid £1750.[27] The location was near Oxford Road, at the corner of Great Marlborough and Blenheim, streets named to commemorate a military action of the duke of Marlborough's: a stone tablet in the wall read "Marlborough Street, 1704," the year of his greatest victory, at the battle of Blenheim."[28] Later on, when rockets were observed in the middle of Great Marlborough Street, it was not to commemorate victory but to determine Cavendish's longitude from Greenwich (Figs. 4.4–4.5).[29]

The inhabitants of Great Marlborough Street were gentlemen and tradesmen, about evenly balanced. In its plan, the street was atypical for London: long, straight, and broad, with a touch of Roman-like grandeur. Its drawbacks were that it opened onto no vistas, and its houses were undistinguished, giving the street a uniform, somewhat boring aspect. The house that Cavendish bought, number 13, was unusual in one respect: it was *two* houses, as it had been since around 1710, when John Richmond, who had actually fought at Blenheim and had risen to the rank of general, leased and joined the separate houses. Following the general's death in 1724, the house went on the market as two houses in one. From a newspaper advertisement the next year, we learn of its size and layout. The property was 45 feet wide and 200 feet deep. Behind the house lay a garden, at the end of which was an apartment with a passageway to the house. The apartment was advertised as "beautiful" and "newly built," with its own plumbing, underground kitchen, and four rooms on the single floor above. Adjoining the apartment were stables and a coach house. Parallel to Great Marlborough Street and running behind the house was a backstreet, Marlborough Mews (in

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[25] We do not know the frequency of Charles Cavendish's visits to his wife's family. We do know that he *and* Birch were at the Grey's together twenty-six times between 1741 and 1751, on two of which occasions, Henry Cavendish came with his father. He was nine and ten at the time. Thomas Birch Diary, BL Add Mss 4478C.

[26] "An Act for Discharging the Estate Purchased by the Trustees of Charles Cavendish … from the Trusts of his Settlement, and for Enabling the Said Trustees to Sell and Dispose of the Same for the Purposes Therein Mentioned," Devon. Coll.

[27] "Assignment of two Messuages in Marlborough Street from the Honourable Thomas Townsend Esq. to His Right Honourable Lord Charles Cavendish," 27 Feb. 1737/1738, Chatsworth, L/38/35. London County Council (1963, vol. 3, pt. 2:261–256).

[28] E. Beresford Chancellor (1931, 207).

[29] "Explosions of Rockets Observ'd at Lord Charles Cavendish's. The Middle of Gr. Marlbro St.," Canton Papers, Royal Society 2:13.

1799 Blenheim Mews), giving access to stables and an apartment adapted from stables, or "mews." We think that as an adult Henry Cavendish lived in this apartment, with the separate address 1 Blenheim St. Thomas Thomson, who knew Henry Cavendish, described his apartment as converted stables.[30]

Figure 4.4: No. 13 Great Marlborough Street House. Demolished. View of the back premises of the house on Blenheim Street. This was Lord Charles Cavendish's house from 1738 to the end of his life. Courtesy of the Westminster City Archives.

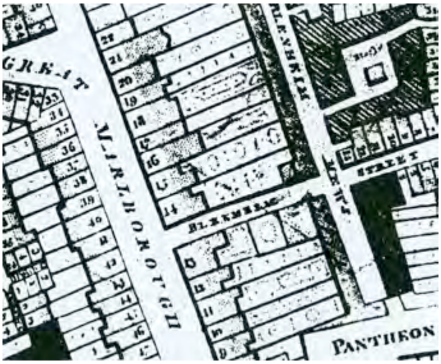

Figure 4.5: Map of Great Marlborough Street. Detail from Richard Horwood's Plan of London … 1792–99, updated to 1813. No. 13 on the corner of Great Marlborough and Blenheim shows a building at the end of the property, designated No. 1 Blenheim Street. There looks to be a divided garden between it and the main house. It seems that Henry Cavendish lived in the rear building.

In the manner described, Charles and Henry maintained partially separate establishments, though mail was sent to him at his father's address on Great Marlborough Street.[31]

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[30] London County Council (1963, vol. 3, pt. 2:256) Richard Horwood (1966). Thomas Thomson wrote that Cavendish's "apartments were a set of stables, fitted out for his accommodation." (1830–1831, 1:59).

[31] James Clerk Maxwell in Cavendish (1879, xxviii).

We find that the rate books for the property do not list Henry until the year Charles died, so that from an official standpoint, Henry lived with his father, who paid the rates. In the rate books for June 1783, two months after his father died, Charles's name still appears beside the assessment for the apartment, but now Henry's name is listed for Great Marlborough Street; notations in the book suggest that the premises behind the house and the main house were both empty.[32]

Two years after Cavendish bought the house on Great Marlborough Street, in 1740, he was elected to the local governing body of the parish, the vestry of St. James, Westminster. The vestry dealt with every kind of practical problem of civilized life: road repair, paving, night watch, workhouses, petitions for the commons, rates, levies, grants, and accounts. No detail was too small: the vestry approved a new umbrella for ministers attending burials in the rain. It was characteristic of Cavendish to turn up faithfully at vestry meetings, which were held as needed, roughly once a month. Others who attended regularly included persons he was either related to, such as Philip Yorke, or with whom he served on boards of other institutions, such as Lord Macclesfield. Cavendish served his parish for thirty-three years, attending his last meeting in early 1783, the year he died.[33]

Friends and Colleagues

Like the house, the life of science on Great Marlborough Street was double. Here Charles Cavendish lived most of his life, and it was Henry Cavendish's address for over half of his life. Here, together and individually, they carried out experimental, observational, and mathematical researches in all parts of natural philosophy.

The wider setting for the scientific activity on Great Marlborough Street was London. Around the time Charles bought his house, one sixth of the people of England either lived or had once lived in London. During his son Henry's lifetime, owing to an influx from the provinces and from abroad, its population rose to nearly a million. Whereas the filth, poverty, and drunkenness of eighteenth-century London are truthfully depicted in Hogarth prints, the city's allure is equally well depicted in Boswell's London journals. London was wealth, power, patronage, and opportunity to rise in the world. It was the seat of national government, a great port city, the commercial center of a colonial system, headquarters of great trading companies, and the financial capital of the world. Westminster could boast of almost 400 distinct trades, among which were those of special interest to Charles and Henry Cavendish, the flourishing scientific instrument and book trades. Whether a Londoner was rising or was, like a Cavendish, already at the top, he had access to every convenience known to civilization. He could feel himself at the center of the world, yet whenever he felt that

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[32] Charles Caverndish was assessed rates for his house on Great Marlborough Street based on a rent of £90; his house being double and also end-of-row, his assessment was more than double that of other occupants on his side of the street. Beginning in 1774, he was also assessed rates for the back mews. Rate books Great Marlborough Street/Blenheim Street, parish of St. James Westminster Archives, film nos. D64, D72, D87, D673, D683, D708, D1102–1110, D1260–1265.

[33] From Cavendish's election to the vestry on 26 Dec. 1740 (D 1760, 145) to his last meeting on 13 Feb. 1783 (D 1764, 518), Minutes of the Vestry of St. James, Westminster, D 1760–1764, Westminster City Archives. Cavendish had other duties in the parish; he was a trustee, for example, of the King Street Chapel (also known as Archbishop Tenison's Chapel) and its school and met with other trustees at the end of the year to pass the accounts. Great Britain, Historical Manuscript Commission, (1923, vol. 3, 270 (4 Jan. 1742/1743), 306 (4 Jan. 1744/1745)). London and Westminster were geographically distinct until the sixteenth century, when the cities spread onto the fields separating them.

the world was too much with him, he had only to step back out of the street to find himself inside his own house, his castle, "in perfect safety from intrusion." For Henry Cavendish, who was interested in the great world and at the same time was extremely shy, it was no small advantage of London that there "a man is always *so near his burrow*."[34]

For most of Charles Cavendish's life and for a good part of Henry's, London was the center of scientific activity in Britain. Even in the second half of the eighteenth century, when much of the important scientific activity took place elsewhere, in the Scottish university towns and in the rising industrial towns such as Birmingham and Manchester, London remained "intellectually pre-eminent," a "magnet for men with scientific and technical interests," the "Mecca of the provincial mathematical practitioner."[35] Over half of the British men of science of the eighteenth century who enter the *Dictionary of Scientific Biography* worked mainly in or near London. The city was large enough to be home to numbers of experts in every part of science yet compact enough for persons of common interest to meet frequently in halls, coffee houses, and private homes. Scientifically interested and interesting visitors from the provinces and from abroad were welcomed. To paraphrase Samuel Johnson, as Charles and Henry Cavendish might have, anyone who was tired of London was tired of science.

The Royal Society, although it was open to national membership and included foreign members, was the Royal Society *of London*. For the Londoner Charles Cavendish, the Society was the center of his scientific activity, and his friends, so far as we know them, were almost all fellows of the Royal Society. The membership of the Society reflected the social distinctions of the wider society,[36] but in its operations, it was relatively unaffected by them.[37] Cavendish's associations within the Society were based on mutual interest, not on family or aristocratic ties; in that setting, his birth was no advantage and no impediment in his association with persons from other walks of life.

Cavendish also belonged to the Royal Society Club, officially named the Society of Royal Philosophers, its members usually referring to it simply as "the Society." The Society or Club undoubtedly had a predecessor, but if Cavendish had been a member of the earlier club, as has been asserted, it remains that he was not elected to the new one until eight years after its founding in 1743.[38] From the beginning, the Club included close friends of Cavendish's, such as Watson, Heberden, and Birch, and members of the De Moivre circle, such as Folkes, Davall, Scott, and Stanhope. The occasion of Cavendish's election was the fatal illness of the president of the Club Folkes, who was also the president of the Royal Society. This was at the end of 1751, when the regular time for electing new members to the Club was many months off. Cavendish as vice president had already taken Folkes's place in the Royal Society, and on the expectation that he would become the next president of the

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[34] Quoting an acquaintance on the importance of living in London: James Boswell (1963, 3:73). Rudé (1971, 4–7, 25, 28, 32–33).

[35] A.E. Musson and Eric Robinson (1969, 57). E.G.R. Taylor (1966, 14).

[36] Cavendish, as son of a peer, was admitted under a special rule of privilege; persons from the lower orders were not admitted at all; and only "rich Philosophers" could afford to pay its admission fee of twenty-two guineas. John Smeaton to Benjamin Wilson, 7 Sep. 1747, quoted in Larry Stewart (1992, 251).

[37] Richard Sorrenson (1996, 33, 35).

[38] T.E. Allibone says that the Royal Society Club was continuous with "Halley's Club," for which he has several pieces of evidence, but for his assertion that Charles Cavendish was probably a member of Halley's Club he offers none, and so this lead we are unable to follow up. T.E. Allibone (1976, 45, 97). An opposing view of Halley's part in the origins of the Club is Archibald Geikie (1917, 6–9). Charles Cavendish was elected to the Club on 25 July 1751 and he became a member on 9 January 1752.

Royal Society, the Club wanted him to take Folkes's place there too. Cavendish's election was made an exception and in January 1752 he assumed the chair at the Royal Society Club.[39]

For convenience, the Club met on the afternoon of the same day the Royal Society met, Thursday, and when the Royal Society was not in session, the Club continued to meet without a break. Members of the Club did not have to be members of the Royal Society, but normally they were, and the president of the Club was always the president of the Society. Its membership was fixed at forty, though members could bring guests; when Cavendish was admitted, the usual number of members and guests at a dinner was about twenty in the winter and fourteen in the summer. The dinners, which were heavy (fish, fowl, red meat, pudding, pie, and cheese), were held for the first three years at Pontack's and then, throughout Cavendish's membership, at the Mitre Tavern on Fleet Street. The Club provided a fuller opportunity than the formal meetings of the Royal Society for members to discuss science. Cavendish belonged to the Club for twenty years and dined with it often. He normally assumed business responsibilities for the organizations he served, but he did not attend the yearly business meetings of the Club with any particular regularity, unlike Watson, Birch, Heberden, and several other friends, and for that matter, unlike his son Henry, who was a member later.

The Royal Society Club was certainly the most prestigious and probably the largest of the learned clubs in eighteenth-century London, of which there were many. Meeting to discuss science, literature, politics, business, or any other interest that drew men together, London clubs often had a more or less formal membership, with rules and dues, but often too they were informal, certain persons forming the habit of appearing during particular hours at particular coffee houses or eating establishments. Folkes dined not only at the Royal Society Club but also at a club of his own, which met at the Baptist Head in Chancery Lane. Another club of scientific and literary men met at Jack's Coffee House on Dean Street, Soho, and later at Old (or Young) Slaughter's Coffee House on St. Martin's Lane, where in his later years De Moivre solved problems of games of chance for money.[40] Birch met with groups at Tom's Coffee House and at Rawthmell's Coffee-House on Henrietta Street, Covent Garden, later the place of origin of the Society of Arts, which Cavendish would join. At Rawthmell's, Charles Cavendish and James Cavendish joined Birch and other fellows of the Royal Society, such as William Jones, Richard Graham, John Colson, Daniel Wray, and John Machin.[41] Public houses provided clubs with a measure of privacy in their supper rooms, but because they were noisy at best, private houses offered advantages of intimacy for small groups. A group met at Lord Macclesfield's and Lord Willoughby's houses;[42] Lord Willoughby also presided at a club that met at a tavern—a life insurance club based on

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[39] 28 Nov. 1751, Minute Book of the Royal Society Club, Royal Society. Cited in Allibone (1976, 44–45). Cavendish was a member of the Club for twenty-one years, resigning at the annual meeting in 1772. He continued to take an interest in it, making it a gift of venison five years later. 9 Sep. 1779, Minute Book of the Royal Society Club, Royal Society, 7.

[40] 19 Oct. 1736, Thomas Birch Diary. W. Warburton to Thomas Birch, 27 May 1738, in John Nichols (1817–1858, 2:86–88, on 88). Bryant Lillywhite (1963, 280–281, 369–370, 421–423, 595).

[41] Parkinson (1854–1857, vol. 2, pt. 1, 221, 280, 322).

[42] Request to be "admitted to the private meetings, of several learned Gentlemen, at Lord Macclesfield's and Lord Willoughby's." Rodolph De Vall-Travers to Thomas Birch, [4 Apr. 1757], BL Add Mss 4320, f. 9.

the principles of the De Moivre pupil and mathematician James Dodson, which met at the White Lion Tavern—and at another club that met alternately in his and Birch's houses.[43]

Another group met at a private house located in the Strand. Charles and Henry Cavendish belonged to it, as did Charles's friends Heberden, Watson, and Israel Mauduit. The other members, so far as the membership is known, were John Ross, Peter Holford, and the physicians George Baker, Richard Huck Saunders, and John Pringle.[44] The interest that brought these men together was undoubtedly science, though in general outlook there would seem to have been a common spirit of enlightened criticism and reform. Upon becoming bishop of Exeter and entering the House of Lords, the antiquarian John Ross advocated the extension of tolerance to religious Dissenters.[45] Of Huguenot descent, Israel Mauduit wrote about religious freedom and politics. John Pringle, a president of the Royal Society, made reform of medicine and sanitation in the military his life work.[46] George Baker having found that in his county drinkers of cider were being poisoned by lead persuaded his fellow Devonians to stop using cider vats made of lead, going on to clarify the whole subject of lead poisoning.[47] Watson and Huck Saunders were among the twenty-nine "rebel Licentiates" who joined John Fothergill in urging the Royal College of Physicians to admit physicians who did not have an M.D. from Cambridge or Oxford.[48] Heberden, from within the College of Physicians, sided with them; a fervent Whig, Wilkite, and supporter of petitioning clergy, he was already a thorn in the side of the College, having denounced mithridatum, a presumed antidote to poison, as an ineffective farrago; the College kept it in its pharmacopeia until late in the century, when Heberden's former pupil George Baker took over the presidency and put an end to it.[49] Science provided Cavendish not only an outlet for his intellectual and administrative energies but also the company of men who worked for improvement in a wide range of endeavors.

We have a record of fifteen dinners Cavendish hosted between 1748 and 1761, to which a total of thirty-two guests came, or if we include his son Henry, thirty-three. Birch was at all of these dinners, necessarily, for our knowledge of them comes from his social calendar, kept in the form of a diary. Cavendish dined at his guests' houses as well, suggesting that they formed a club.

Cavendish is first mentioned in Birch's diary in 1730 as if he were public news: "Ld Ch Cavendish resigns,"[50] a reference clearly to Cavendish's resignation as gentleman of the bedchamber to the prince of Wales. Birch's first mention of a personal contact with Cavendish came six years later, in 1736. Their connection then was probably formal, since in that entry and in an entry a year later, Birch identified Cavendish as the brother of the duke of Devonshire.[51] The occasion was Birch's scholarship, for Birch recorded that Cavendish gave him original papers concerning his grandfather William Russell, who, Birch noted,

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[43] Lillywhite (1963, 745).
[44] Andrew Kippis's life of the author published in John Pringle (1783, lxiii–lxiv). Kippis says that the group met at Mr. Watson's. This Watson he identifies as a grocer.
[45] "Ross or Rosse, John," *DNB*, 1st ed. 17:266–267.
[46] "Pringle, Sir John, ibid. 16:386–389, on 388.
[47] "Baker, Sir George," ibid. 1:927–29, on 928.
[48] Dorothea Waley Singer (1949, 161–162).
[49] Humphry Rolleston (1933, 412–413, 567–568).
[50] 12 Oct. 1730, Thomas Birch Diary.
[51] 29 June 1736 and 1 Aug. 1737, ibid.

was beheaded in Charles II's reign.[52] Here Cavendish was acting as a representative of his family, but he and Birch were to become close personal friends.

A letter from Birch to Philip Yorke in 1750 gives us an idea of Cavendish's social life as it related to science. Cavendish invited Birch and six other "Bretheren of the Royal Society" to a "small Party," at which he offered a "philosophical Entertainment of an artificial Frost by a Solution of Sal Ammoniac in common Water," after which he provided "what was equally relish'd, a very good Dinner."[53] (This experiment on artificial frost anticipated Henry Cavendish's later researches on freezing solutions.) If Cavendish performed experiments at his other dinners, we do not know, but it was an acceptable home entertainment or instruction at the time. Earlier that same year, Cavendish attended a dinner at Martin Folkes's house, to which John Canton was invited. Folkes told Canton that Cavendish was "very curious" to see him perform his experiment with artificial magnets, which he could watch "more at ease" at his house than he could at the Royal Society. The next year, when Folkes was ill, Cavendish presided at the Royal Society, where he gave an undoubtedly well-prepared, "excellent discourse" on artificial magnets, for which Canton received the Copley Medal.[54]

To get a fuller idea of Cavendish's social life, we look at who came to dinner at his house on 21 October 1758. He had eight guests, all professional men, all but one middle-aged, some but not all married. They were friends, not persons Cavendish brought together for introductions. They were all active fellows of the Royal Society, though none was on the Council at the time; Birch was a secretary of the Royal Society, and Cavendish was possibly a vice president (he had presided at one meeting that year). It is possible that the social evening was combined with a meeting for a special purpose, perhaps relating to the Royal Society, though the regular meetings of the Society had not yet resumed after the long summer recess. Cavendish, the only aristocrat, at fifty-four was the next-to-oldest member at the party. His senior by two years, Thomas Wilbraham was physician to Westminster Hospital. Birch was fifty-two, like Cavendish long a widower, with an adult daughter about thirty. Watson was forty-three and married, or at least he had been married, with a son of about fourteen and a daughter. Having started out as an apothecary, Watson was practicing as a physician, and had just begun to be listed as "Dr. Watson" in the minutes of the meetings of the Royal Society. Heberden was forty-eight, another widower, with a son about five who was probably living at home. At one time he had lectured on medicine in Cambridge, but for the past ten years he had been practicing in London. Israel Mauduit at fifty was a rich bachelor who liked to entertain at home himself. Samuel Squire, about forty-five, was an ambitious clergyman about to rise to bishop; he was married and probably had children by now (he eventually had three). Gowin Knight, forty-five and apparently unmarried, was then giving attention to the mariner's compass and to his new duties as principal librarian of the British Museum. John Hadley, at twenty-seven the only young man in the company, had just been elected to the Royal Society. He was still trying to find his place, dividing his time between Cambridge, where he was professor of chemistry, and London, where he was soon to become physician to St. Thomas's Hospital. These were men of liberal outlook

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[52] 1 Aug. 1731, ibid.

[53] Thomas Birch to Philip Yorke, 18 Aug. 1750, BL Add Mss 35397. The guests were Birch, Folkes, Heberden, Watson, Thomas Wilbraham, and Nicholas Mann.

[54] 30 Nov. 1751, JB, Royal Society 20:571–573.

and so far as we know their political leaning, Whig. Some of them were university men, some—including Birch, Watson, and the host—were not.

Among Cavendish's guests that night were several very good scientific men. The year before, Cavendish had been awarded the Copley Medal, as earlier had two of his guests, Watson and Knight, but this dinner was not, scientifically speaking, particularly high-powered. Some of the party were primarily interested in antiquities, which made it a mix like the membership of the Royal Society itself. Only Watson had published extensively in the *Philosophical Transactions*, addressing a variety of subjects including his professional field, medicine, and with considerable success electricity. Knight's papers on magnetism were just that year coming out in a collection. Heberden had published four papers on a miscellany of topics, one, a human calculus, falling within his professional field, medicine. Birch had published five papers, one on Roman inscriptions, belonging to his field, history. Half of the guests were, like Cavendish, one-paper men. Wilbraham had published a medical account of a hydrophobia. Hadley's one paper was yet to come, on a mummy examined in London. Mauduit's paper was on a wasp nest. Squire's was on a person who had been dumb for four years and had recovered his tongue upon experiencing a bad dream. Since the guests were all men of learning, some, like Birch, had substantial publications outside of the *Philosophical Transactions*.

Friends and Colleagues

Figure 4.6: Thomas Birch. Painting by J. Wills, engraving by J. Faber, Jr. Reproduced by permission of the Trustees of the British Museum.

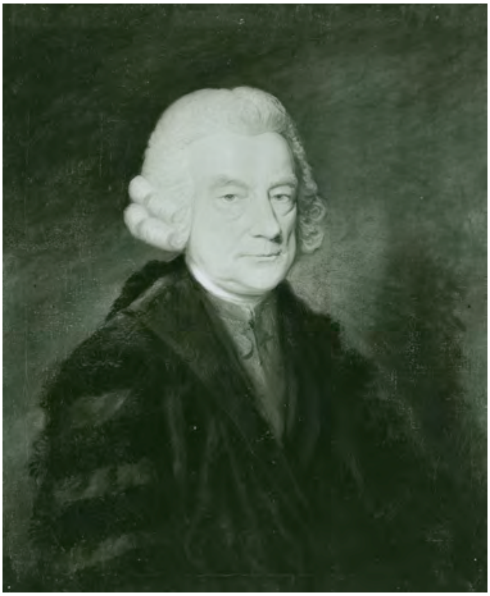

Figure 4.7: William Watson. Painting by L.F. Abbot. Reproduced by permission of the President and Council of the Royal Society.

Figure 4.8: William Heberden. Painting by Sir William Beechey, engraving by J. Ward. Wikimedia Commons.

Over the period for which we have a record of dinners, 1748 to 1762, Cavendish together with Birch also dined at Heberden's and Stanhope's houses as often as at Cavendish's, and at Watson's, Macclefield's, and Yorke's about half as often.[55] With Birch, together with other men of science and learning, Cavendish dined two hundred times, at houses and at the Mitre with the Royal Society Club.[56] What brought Cavendish and the others together was, apart from conviviality, a common public life centering on the Royal Society.

Cavendish was especially close to three of the above colleagues, Birch, Heberden, and Watson. Birch was a historian, biographer, and cleric, who met scientific men more than halfway (Fig. 4.6). His membership certificate at the Royal Society, which was signed by Halley, read that he was "well-versed in Mathematics and Natural Philosophy." When Pierre Bayle's biographical dictionary was translated into English in 1709, the London publisher planned a revision with the intention of doing more justice to English notables, and Birch, at age twenty-six, was invited to be one of the three editors. Appearing in ten volumes between 1734 and 1741, three volumes of which were dedicated to presidents of the Royal Society, the revision contained biographies of English scientific notables written by Birch. His most important literary contribution to science was his biography of the seventeenth-century chemist Robert Boyle, which appeared as the third volume of the biographical dictionary, together with his edition of Boyles's papers. He was drawn to Boyle for his religious and scholarly knowledge as well as for his scientific work, a combination of interests Birch himself had. He implied the importance for a scholar's work of living near other scholars, as Boyle did at Oxford, and as Birch did in London.[57] In 1757, he completed a history of the Royal Society, which he had intended to bring up to date, but in four volumes he did not get past the seventeenth century. He based his history on the original journals, registers, letters, and Council minutes of the Society, reproducing much of the material and chronicling the Society meeting by meeting; his method of history was the method of science, as he understood it, the orderly bringing together of facts.[58] He depended on clerical livings, even there making a connection with science; he was chaplain to the College of Physicians, and he cited Newton in notes to his sermons.[59] An irrepressible conversationalist, Birch was "brisk as a bee" according to Johnson, a connoisseur of conversation.[60] A historian who wrote of science to praise it, a man of facts, convivial and energetic, Birch was a welcome addition in scientific circles.

Like Birch, the physician Heberden met men of science more than halfway (Fig. 4.8). His goal was to make the College of Physicians a medical version of the Royal Society, a proper scientific body. He used his influence in the College—he took on the duties of counselor, censor, and elect, one of the powerful senior fellows who chose the president from among themselves—to establish a committee of papers and a journal modeled and named after the Royal Society's, the *Medical Transactions*. Consistent with his belief that until a

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[55] Birch's Diary records dinners at which Cavendish was present at the homes of fourteen persons, all but one of whom were fellows of the Royal Society. The names are familiar: in addition to those mentioned above, they include Josiah Colebrooke, Mark Akenside, Daniel Wray, and William Sotheby.

[56] Thomas Birch Diary. The number two hundred is a minimum, since Birch made his entries hastily, not always giving the names of everyone he dined with. Cavendish's name was probably among those he sometimes omitted.

[57] Gunther (1984, 13–19). Thomas Birch (1744, 113–114, 304–307).

[58] Thomas Birch (1756–1757).

[59] C. Barton to Thomas Birch, 19 Sep. 1754, BL Add Mss 4300, f. 174. Thomas Birch's Sermons, vol. 7, f. 188, BL Add Mss 4232C.

[60] "Birch," *DNB* 2:531.

Newton appeared in the science of the animal world to discover the "great principle of life," medicine had only one recourse, experience. He regarded his task as the patient and laborious assembling of facts; a painstakingly accurate observer, he made no large generalizations (or discoveries). Despite his admonitions to physicians to publish, he himself was reluctant to do so. His high reputation was based on his medical practice and his knowledge of the classics, a combination then in irreversible decline. Upon being asked what physician he wanted in his final illness, Johnson called for Heberden, "the last of our learned physicians."[61]

More than any other member, Watson made the meetings of the Royal Society rewarding, keeping it informed of major developments in science in Britain and abroad. As the reviewer for the Society, he was well prepared, equally capable of giving the Society a thorough exposition of Franklin's work in electricity and of Linnaeus's work in botany. Forceful, knowledgeable (because of his remarkable memory, he was referred to as the "living lexicon of botany"), and a good judge of men, Watson entered energetically into the administration of the Royal Society as he did into that of the other institutions he served, which were more or less the same ones that Birch, Heberden, and Cavendish served.[62]

We learn more about Cavendish's friendships and associations by looking at his activity in the Royal Society. Although there is no record of how he voted on candidates for admission to the Society, we know which candidates he recommended and the members with whom he signed recommendations. Before a candidate was proposed for membership, he was usually canvassed by the Council. The candidate had then to be formally recommended by three or more members, who drew up a sheet with their signatures, the candidate's name, address, and profession, and a brief description of his qualifications for membership. The sheet would be dated and posted by one of the secretaries in the meeting room for the period of several ordinary meetings before the candidate was put to the vote. An exception was made for peers and their sons and various dignitaries, for whom only one recommender was required. Election was by two thirds of those present.[63]

To further a candidate's chances of election, other members could add their signatures to the sheet. Ten, not an uncommon number, signed Henry Cavendish's certificate in 1760. Occasionally there was a groundswell of enthusiasm for a candidate, as there was for Captain James Cook, whose certificate was signed by twenty-five members, including Henry Cavendish. Certain members constantly put up candidates, bearing a good share of responsibility for the early rapid growth of the Society. In the first forty years, the number of ordinary members tripled to three hundred, with the number of foreign members growing even faster, rising to almost half the number of ordinary members.[64] During the twenty-five years that Cavendish recommended candidates, the growth of the Society markedly slowed. Cavendish's own contribution was moderate: between 1734 and 1766, he recommended twenty-eight candidates, fewer than one a year.

Birch, who recommended a large number of candidates, on the order of a half dozen a year, signed recommendations with Cavendish more often than any other member, nineteen times.[65] Next came Folkes with ten recommendations in common, then Watson and Wray,

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[61] Rolleston (1933, 414–417). Audley Cecil Buller (1879, 16, 21–22). William Munck (1878, 2: 159–164). William Heberden (1802, 483, and appendix, "A Sketch of a Preface Designed for the Medical Transactions, 1767," 486–494).

[62] "Watson, Sir William," *DNB*, 1st ed. 20:956–958.

[63] 20 Aug. 1730, Minutes of Council, Royal Society 3:51, 77.

[64] 23 Nov. 1775, Certificates, Royal Society 3:237. Henry Lyons (1944, 125–126).

[65] Between 1748 and 1760, Birch recommended seventy-six candidates. Royal Society, Certificates.

each with nine; the four were good friends and probably knew the same candidates and had similar ideas on qualifications for membership. Next came Jones, from the De Moivre circle, who was Cavendish's own recommender, then Burrow and Willoughby. The one person who signed recommendations often with Cavendish who does not seem to have belonged to his circle was John Machin, professor of astronomy at Gresham College and secretary of the Royal Society, who died early in this account and was in poor health during his last years. It should be noted that Cavendish frequently joined Sloane in his early recommendations until Sloan retired as president in 1741. Among Cavendish's ninety-three cosigners, most of the other familiar names appeared too, though with less frequency: Heberden, Bradley, Stanhope, De Moivre, Macclesfield, Scott, Jurin, Davall, and Richard Graham, to name several.

We turn to the candidates Cavendish recommended. In 1753 the Council resolved that candidates were to be known "personally" to their recommenders, a practice which in the past had usually been followed though not invariably.[66] We can be reasonably certain that Cavendish was familiar with most if not all of the persons he recommended. Seventeen of the certificates he signed said that the candidates were proficient in the sciences, designated variously as natural philosophy, experimental philosophy, natural knowledge, natural history, philosophical knowledge, philosophy, and various branches of science; six certificates mentioned mathematics, three useful learning, two mechanics, and another two astronomy. Seven of the candidates were said to be distinguished in literature or polite learning, though never that alone. There were a few other accomplishments: antiquities, architecture, medicine, anatomy, musical theory, and (not very helpful) learning and knowledge. Two candidates were professors at Cambridge and Oxford, about whom nothing more needed to be said than the names of their professorships, which in their cases were astronomy and experimental philosophy. For one other candidate no explanation was given other than his position, an underlibrarian at the British Museum. Cavendish recommended three foreign members, whom he did not have to know personally, only their work. They were a French astronomer and two French authors of a commentary on Newton's *Principia*. The persons Cavendish helped to gain entry into the Royal Society favored the physical and mathematical sciences, as might be expected, but they were not narrowly identified with particular fields, a generality which is also to be expected given the composition of the Society.

With one exception, every candidate Cavendish recommended was elected. The exception was the first candidate, a surgeon whose rejection may have been due to a general suspicion of surgeons in the Society. In 1734, Cavendish joined Sloane, two others, and John Stevens, one of the surgeons to the prince of Wales, to recommend John Wreden, another surgeon to the prince of Wales, both of whom Cavendish probably knew, since he had recently served as gentleman of the bedchamber to the prince. The vote against Wreden was decisive.[67] In general, a recommendation by Cavendish was helpful to a candidate. Joseph Priestley, who unlike Cavendish had to make his living, which he did in part by writing, was informed that membership in the Royal Society would encourage sales of his book on the history of electricity. In discussing his prospects and strategy with his friend John Canton in the Royal Society, Priestley expected that not only Canton but Watson and Richard Price would support his candidacy, constituting the necessary minimum number of three recommenders, and "If L.C. Cavendish could be prevailed upon to join you," he told Canton, "I

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[66] 10 May 1753, Minutes of Council, Royal Society 4:118–119.

[67] 42 members voted, 24 rejecting Wreden. Maurice Crosland (1983, 171).

should think the rest would be easy." Canton, it would seem, refused to approach Cavendish on the technical ground that Priestley was not a "personal acquaintance" of his.[68]

A historian of science has placed Cavendish in a small group of fellows of the Royal Society who in the 1750s and 60s acted in concert, especially in the election of officers. Described as the "Hardwicke Circle" owing to the patronage of the first and second earl Hardwicke, they included Wray, Birch, Folkes, Heberden, Macclesfield, Maudit, Squire, Willoughby, and Watson, all familiar friends and colleagues of Cavendish's. In politics they were Whig, their influence in the Royal Society declining in step with the decline of Whig power in the nation. The group often gathered at Wrest Park, whose present owner, Philip Yorke, second Earl Hardwicke, was Cavendish's nephew-in-law and close friend. He probably did not benefit from the patronage of the Hardwickes, but through the family tie he was associated with the group. For a biography of Cavendish, the Hardwicke connection is noteworthy, for it relates his scientific life to the Grey side of his family, which tends to be overshadowed by the magnificent Cavendishes.[69]

Relatives

As he grew up, Frederick Cavendish—Fredy, his family called him[70]—followed in his older brother Henry's footsteps, at a two-year interval, first attending Hackney Academy and then Peterhouse, Cambridge. In the year after Henry left Cambridge, his next to final year at Cambridge, Frederick Cavendish had a bizarre accident, falling from an upper window in one of the courts and striking his head. There is no indication of what he was doing. Riotous behavior at Cambridge was common enough, prompting the poet Thomas Gray to change his living quarters and affiliation from Peterhouse, Frederick's college, to Pembroke across the street. Whatever the reason, the fall was serious, leaving Frederick's life in the balance for a time and his head with a deep indentation as a reminder of it. The accident happened in late July or early August 1754; by mid-August Frederick was "mending, but not out of danger."[71] That summer, Charles Cavendish had been dining frequently with his scientific friends, but then he dropped out due in part to Frederick's condition.[72] In mid-October, Thomas Birch wrote to Charles to say that his friends hoped that "Mr. Frederick Cavendish's Recovery" would soon allow Charles to join them "in town."[73] Frederick did gradually regain his health, but his brain was permanently impaired.

Of how Frederick occupied himself in the years after his accident, there is no record, but we have his father's view of his mental "state." As was the custom, in married settlements the younger son Frederick's eventual prosperity was looked after by his mother, who at her death

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[68] Joseph Priestley to John Canton, 14 Feb. 1766, Canton Papers, Royal Society 2:58. Priestley was elected that year without the help of Cavendish, Benjamin Franklin joining the other three instead. Joseph Priestley to Richard Price, 8 Mar. 1766, in Priestley (1966, 17–19, on 19).

[69] Other members were Davall, Charles Yorke, and John Ward. Considered their successes in elections were Birch and Paul Maty as secretaries and Macclesfield, Morton, and Pringle as presidents of the Royal Society. David Philip Miller (1998, 75–77, 81, 89).

[70] Henry Cavendish referred to "Fredy's" letters and expenses in "Papers in Walnut Cabinet," Devon. Coll.

[71] Charles Cavendish's legal case involving his marriage settlement and Frederick's expenses, 30 Apr. 1773, Devon. Coll., L/114/32. Anonym, "Memoirs of the Late Frederick Cavendish, Esq.," *Gentleman's Magazine* 82 (1812): 289–91, on 289. Lord Hartington to the duke of Devonshire, 17 Aug. 1754, Devon. Coll., no. 260.119.

[72] Charles Cavendish hosted a dinner at his house on 17 July 1754; the next time he dined with his friends was at Stanhope's house on 2 Dec. of that year. Thomas Birch Diary.

[73] Thomas Birch to Charles Cavendish, 17 Oct. 1754, BL Add Mss 4444, f. 180.

in 1733 left him her one quarter share of the duke of Kent's Steane estate. This was sold and converted into stock, which was placed in the hands of trustees. In 1772 the last surviving trustee, Lord William Manners, died, and his son declined the inherited trusteeship. This meant that Charles Cavendish had to choose new trustees, who would have to be persuaded of the legality of the way the trust had been used in the past. He wrote out a justification of his practice and submitted it for legal opinion. He had been receiving the profits from the Stean estate and after its sale the dividends from stock because "it was manifestly improper to pay the money" to Frederick during his minority. Frederick was then thirty-nine, and "even now," Cavendish said, "it appears to be doubtful whether it is prudent to do it." Cavendish had spent the earnings from the trust on the "maintenance & education" of Frederick, the "expense of which greatly exceeded the income of the estate, except in some of the first years of F's life." The legal opinion he solicited, however, held that the trustees had no power to permit him to receive that money for the purpose he gave, for it was a father's duty to support his child. In the eyes of the law, then, although it was not put this way, in skirting his duty Cavendish had been profiting from his disabled son, and he and his heirs, who would be Henry, were accountable to Frederick for the money taken from him. Despite this ruling, the new trustees chosen by Cavendish, all members of the family, agreed to let him continue to accept all dividends and interest from the funds in their name. Henry as well as Charles was a party to the new—but in fact the old—financial arrangements for Frederick's support. Several lawyers became involved, but in the documents we have seen there is no suggestion that Frederick himself was unhappy with the arrangements. What we have learned is that in Charles's judgment, his son Frederick was incompetent to take care of his affairs.[74]

Charles Cavendish took on responsibilities for his siblings. James, the brother with whom Charles had traveled abroad as a youth, was the older of the two, but he deferred to Charles in family matters, asking Charles to dispose of their mother's estate and giving him power of attorney in all matters of their joint executorship.[75] The reason was, at least in part, that as colonel of a foot regiment, he was away in Ireland or Cuba or elsewhere. In his final year, he served as a Member of Parliament for Malton, dying young, presumably of a tropical disease, in 1741.

William, Charles's eldest brother, was interested in art and also, to some extent, in science. Elected to the Royal Society in 1747, William subscribed to a number of scientific books to which Charles also subscribed; for example, books by De Moivre, Roger Long, and Colin Maclaurin.[76] Charles acted as a political go-between for William,[77] but in general William and Charles led very different lives, due in part to temperament and in part to their order of birth. They started out the same way, as Members of Parliament, but Charles left politics and William did not and realistically could not. After their father's death in 1729,

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[74] "Copy Case between Father and with Mr. Perryn," 30 Apr. 1773. Charles Cavendish to S. Seddon, 27 and 29 July 1772. "Discharge from the Right Honourable Lord Charles Cavendish to John Manners Esqr as to Trusts for his Lordship and the Honourable Henry Cavendish & Frederick Cavendish His Sons," Devon. Coll., L/14/32. The new trustees were Philip Yorke, earl of Hardwicke, and Charles's nephews Frederick and George Augustus Cavendish. In his will, Charles left his son Frederick £4000 for his having received profits from his mother Anne's estate and dividends from the stock bought with the money arising from the sale of that estate. Devon. Coll., L/69/12.

[75] James Cavendish to Charles Cavendish, 25 Mar. 1727 and 23 Aug. 1732, Devon. Coll., no. 34/2.

[76] Lists of subscribers to Abraham de Moivre, *Miscellanea analytica de seriebus et quadraturis* (London, 1730); Roger Long (1742, 1764, 1784, vol. 1); Colin Maclaurin (1748).

[77] In a dispute over appointments between the duke of Devonshire and the duke of Newcastle, for example. Duke of Devonshire to Lord Hartington, 8 and 20 May, 15 and 24 June 1755, Devon. Coll., nos. 163.51,52,60, 62.

William as third duke of Devonshire sat in the House of Lords, where he rarely spoke, and when he did it was with such a soft voice that no one could hear him. Not a leader of the party and not a fighter, William accepted high office without high ambition. Like his father, he was a friend of Walpole's, doing favors for Walpole in kind and helping to keep him in office. Walpole did favors in return, appointing William lord privy seal and then lord lieutenant of Ireland, a highly lucrative post because of its patronage. Local government was the basis of political power in the eighteenth century, and the lord lieutenant of a county was the highest local official, though the lord lieutenant of Ireland had a trace of derogation; in any event, William carried out his work competently for seven years.[78] William was a hard drinker, a gambler, not overly smart, and distinctly lazy. He was also cautious and duty-bound, family traits which were regarded as strengths of character. Johnson, who rarely saw anything he could admire in a Whig, saw in William a man who was "unconditional … in keeping his word," a man of "honor."[79] The record we have of Charles's relationship with his brother William has entirely to do with money or property. That is so even during the Jacobite rebellion of 1745, when an army led by the Stuart pretender advanced south as far as Derby, menacing Chatsworth. By subscription, William raised a regiment in Derbyshire to stop the invasion, while Charles served as William's surrogate banker and advisor in London. Unless William's medals at Chatsworth were "sent out of the Kingdom," Charles told him, he did not think they could be saved if the French landed to aid the pretender, since there would be a rising right there.[80] Nothing, it turned out, had to be done, since the invading army was forced to retreat.

William had confidence in his youngest brother. Two years after succeeding to the dukedom, he made out his will, in which he left to William Manners and others his horses but named twenty-seven-year-old Charles Cavendish, his wife, Anne, and Robert Walpole trustees for his seven children.[81] Of his four sons, three entered politics, all staunch Whigs and allies of Fox, the fourth entering the military, which by then was an uncommon career for a Cavendish. The youngest son, John, who was Henry Cavendish's age and went through school with Henry, was by far the most determined in politics, rising to cabinet positions. The oldest son, William, was the most determined in love, choosing for his wife the sixteen-year-old Charlotte Boyle, a distant relation of the seventeenth-century chemist Robert Boyle, knitting together the two great aristocratic families in science. From the point of view of the Cavendish fortune, she was a prize, the sole heir of the immensely rich Lord Burlington. As it happened, the Burlington family was talked about more for its scandals than for its wealth, which decided William's mother, herself a commoner before becoming duchess of Devonshire, against the match. William's father the duke supported it, the marriage took place, and the duke's own marriage came apart as a consequence. The practical result of this turmoil was that the already fabulous Cavendish estate nearly doubled in value.[82] To young

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[78] J.H. Plumb (1956–1960, 1:42–43, 235–236, 2:280).

[79] John Pearson (1983, 89–91); quotation from Johnson on 90.

[80] Lord Hartington to Dr. Newcome, 14 Dec. 1745; Charles Cavendish to duke of Devonshire, undated, Devon. Coll., nos. 260.58 and 211.3; John Whitaker to Dickenson Knight, undated [1745]; Ralph Knight to Dickenson Knight, undated [Dec. 1745]; John Holland to Ralph Knight, undated [1745], in Great Britain, Historical Manuscripts Commission (1893, 164–165). Duke of Devonshire to Robert Wilmot, 25 Oct. 1745, in Great Britain, Historical Manuscripts Commission (1925, 2:349). Richard Burden to [Viscount Irwin], 7 Dec. 1745, Great Britain, Historical Manuscripts Commission (1913, 138).

[81] Duke of Devonshire, "My Will," 1 Oct. 1731, Devon. Coll., no. 163.95.

[82] Pearson (1983, 93–103).

William's sorrow, his wife did not live long enough to become duchess, and he himself did not live many years after becoming the fourth duke. Charles Cavendish was the responsible family intermediary, meeting several times with the third duke's lawyer in connection with his son's marriage to Charlotte Boyle.[83] There is a legend that Henry Cavendish lived for several years in his youth in Burlington House in Piccadilly, but it seems rather improbable.[84]

Like his son William, the third duke's daughters made advantageous marriages. Rachel married Horatio Walpole, a relative of the well-known writer Horace Walpole. Caroline married William Ponsonby, second earl of Bessborough, who at the time was secretary to the third duke as lord lieutenant of Ireland; to their son, the third earl of Bessborough, Henry Cavendish would leave a sixth of his great fortune.[85] Elizabeth married John Ponsonby, of the same family; to make up her dowry the duke, who was rich in property but short of cash, borrowed from Charles Cavendish.[86] When the third duke died in 1755, Charles Cavendish found his will, which had been lost, written on a sheet of letter paper, almost worn out and very plain, in keeping with everything else about the third duke.[87]

Charles Cavendish assumed various obligations for the women of his family. Together with his uncle James, he served as executor of the estate of his aunt Elizabeth (Cavendish) Wentworth.[88] The second duke of Devonshire, after his daughter Diana died in childhood, set aside lands to raise dowries for each of his three surviving daughters, Rachel, Elizabeth, and Anne. When Rachel and Elizabeth were about to be married, their brother Charles was named representative for Anne, who was without prospect and in the event never did marry. To keep the lands within the Cavendish estate, the women were paid off in cash with interest, requiring Charles to talk hard to bring Anne around to the logic of it, she being "extremely jealous, & fearful of being injured."[89] Rachel, who married Sir William Morgan of Tredegar,

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[83] Charles Cavendish's involvement is reflected in the statement of expenses presented to the third duke by Hutton Perkins, the duke's lawyer, on 13 May 1748. Devon. Collection, no. 313.1.

[84] "The scientist, Henry Cavendish, lived there [in Burlington House] for several years in his youth." D.A. Arnold, Royal Society of Chemistry, "The History of Burlington House" (http://www.rsc.org/AboutUs/History/bhhist.asp). Royal Society of London (1940, 65). The owner of Burlington House, Richard Boyle, third earl of Burlington, is said to have had an interest in natural philosophy, but he is known for his interest in the arts and especially for his talent as an architect, being instrumental in introducing the Palladian style in Britain and Ireland. Horace Walpole called him "the Apollo of the arts." When his daughter and heir Charlotte Elizabeth Boyle married William Cavendish, Henry Cavendish was about to begin his university studies. When the earl died in 1753 and Burlington House passed to his daughter, Henry Cavendish had completed his university education. It is unclear what connection Henry could have had with Burlington House. We know that Henry's heir George Augustus Henry Cavendish used the house for at least two spells.

[85] Entries for the second and third earls of Bessborough, in Brydges (1812, 7:266–267). Francis Bickley (1911, 207).

[86] "Bond from His Grace the Duke of Devonshire to the Rt Hon^{ble} Lord Charles Cavendish," 22 Sep. 1743, Devon. Coll., L/44/12.

[87] R. Landaff to duke of Devonshire, 6 Dec. 1755; Thomas Heaton to duke of Devonshire, 6 Dec. 1755, Devon. Coll., nos. 356.5 and 432.0. Theophilus Lindsey to earl of Huntington, 24 Dec. 1755. Great Britain, Historical Manuscripts Commission (1928–47, 3:111–114, on 113).

[88] "Probate of the Will of Ly Eliz. Wentworth 1741," Devon. Coll., L/43/13. Lady Elizabeth was the widow of Sir John Wentworth of Northempsall. Seven years later, Charles and James Cavendish were released from any further claim on them as executors by another Lady Wentworth, Dame Bridget of York: "Ly Wentworths Release to Lady Betty Wentworths Executors March 5 1748." Charles kept a notebook for Lady Betty's personal estate for twenty years, from 1741 to 1761. After 1748 Charles and James regularly received a small dividend from 200 shares of South Sea stock. After James's death, his part went to Richard (Chandler) Cavendish and, eventually, to Charles Cavendish.

[89] "Deed to Exonerate the Estate of the Duke of Devonshire from the Several Portions of Six Thousand Pounds … to be Directed to be Raised for Lady Rachel Morgan, Lady Elizabeth Lowther and Anne Cavendish the Three

had four children;[90] Charles kept in touch with her family, and when her daughter Elizabeth married William Jones of Llanarthy, Charles was a party to the settlement.[91] In 1723, his sister Elizabeth married the Member of Parliament for Lanchester Sir Thomas Lowther; his long and consequential involvement with her family we take up in the next section.

Through another family member, a younger first cousin, Charles Cavendish came into a large inheritance. Elizabeth (Cavendish) Chandler's father was Lord James Cavendish, Charles's uncle (not his brother of the same name), a fellow of the Royal Society, with interests in mathematics and natural philosophy.[92] Her mother was Anne Yale, daughter of Elihu, a rich diamond merchant and governor of Fort St. George in Madras, after whom Yale University is named. In 1732 Elizabeth married the politician Richard Chandler, son of Edward Chandler, bishop of Durham, the year after her brother William had married another Chandler, Barbara. In 1751 Elizabeth's father and brother both died, and as she had no children and her mother had died earlier, she and Richard Chandler alone constituted that branch of the family. Her father left his real estate to Richard Chandler provided that he took his wife's surname.[93] When Richard (Chandler) Cavendish died, Elizabeth became sole owner of a house in Piccadilly, a good deal more real estate, and a large sum in securities and mortgages. In her will, other than for her real property, she left her estate after payment of legacies, debts, and funeral expenses to Charles Cavendish, her executor and only living male first cousin on the Cavendish side. Shortly before her death, she added a codicil to her will, naming as co-executor with Charles the prominent lawyer and politician Lord Charles Camden. The two executors were to hold the Piccadilly house in trust, but otherwise as far as Charles Cavendish was concerned, the will was practically the same.[94] Charles Cavendish took upon himself the task of executing it. Three and a half years after Elizabeth, Charles Cavendish died, having fully completed the executorship but before the residue had been deposited in his account. It was left to Charles Camden, the surviving executor, to transfer Charles Cavendish's inheritance, £97,000 in bank annuities and £47,000 in mortgages, from Elizabeth to his heir, his oldest son. In this way, at the end of 1783, a considerable fortune became the property of Henry Cavendish,[95] on his way to becoming the "richest of the wise."

Holker Hall

Holker Hall is a grand manor on the northwest coast of England, in the county of Cumbria, formerly in Lancashire (Fig. 5.3). It is situated among splendid gardens on hilly park-like

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Surviving Daughters of William Second Duke of Devonshire," 28 July 1775, Devon. Coll., L/19/67. Charles Cavendish to John Heaton, 28 Aug. 1775, draft, and "Account of Deeds to Be Executed by Lord Charles Cavendish," Devon. Coll., 86/comp. 1.

[90] Brydges (1812, 1:356). Page (1971, 2:190). Geoffrey Holmes (1967, 222).

[91] Articles on the marriage of William Jones and Elizabeth Morgan, daughter of Lady Rachel Morgan, to which Charles Cavendish was a party, 4 July 1767, Devon. Coll., L/43/16.

[92] James Cavendish and Charles Cavendish together recommended Gowin Knight for fellowship in the Royal Society for his "mathematical and Philosophical knowledge," 24 Jan. 1745, Certificates, Royal Society 1:14, f. 297.

[93] "The Surname of Cavendish Witnessed by W. Goostrey All Proved by Mr Chandler 20th December 1751," Devon. Coll.

[94] Elizabeth Cavendish's will, 26 Feb. 1778, Devon. Coll., L/ 31/37. In a codicil of 31 Jan. 1779, among other changes, she left her land to Dudley Long instead of to the duke of Devonshire, and she left her house in Picadilly to Charles Cavendish and Charles Camden to hold in trust for members of the Long family, especially Dudley.

[95] "Lord Camden and the Honourable Henry Cavendish Assignment and Deed of Indemnity," 31 Dec. 1783, Devon. Coll., L/31/37. "Copy of Mr. Pickerings Letter to Mr. Wilmot," 26 Apr. 1780, ibid., L/86/comp. 1.

grounds with woodlands overlooking Morecambe Bay (Fig. 5.4). Built in the sixteenth century, it was altered in the 1780s and again in the next century. Today it belongs to the Cavendish family and is open to the public. Its library contains many books from Henry Cavendish's library.

Late in life, Henry Cavendish had a conversation with a colleague John Barrow about Holker Hall. Barrow thought that it belonged to Lord George [Augustus] Cavendish. Cavendish corrected him: "It did belong to him, Sir; but he left it to my father, from whom it descended to me, and will next go to another Lord George [Augustus Henry Cavendish]."[96] Barrow's recollection of the conversation is detailed and plausible, but it raises questions.

It is at odds with published sources, which agree on a succession of ownership of Holker Hall, in which Charles and Henry Cavendish do not enter. According to this version, Holker Hall came into the Cavendish family in 1756, when Lord George Augustus Cavendish acquired it from a Lowther cousin. When Lord George Augustus died in 1794, it passed to his brother Lord Frederick. When Lord Frederick died in 1803, it passed to Lord George Augustus Henry Cavendish, who held it until his death in 1834. We will look at the tangled affairs of the Cavendish and Lowther families, which may shed light on the confusion over Holker Hall and how it came about. The episode shows the effort Charles Cavendish was willing to make to help his family.

The relevant history begins with the last Lowther to live at Holker Hall, Sir Thomas Lowther, the son and heir of Sir William Lowther, a large landholder in Lancashire and Yorkshire, who had been raised to a baronetcy at the end of the seventeenth century. Known as an independent country Whig, Thomas Lowther was a Member of Parliament for Lancashire, spending part of his time in London. The rest of his time he spent mainly at his country house and family seat Holker Hall, near the village of Cartmel. The rectory and manor of Cartmel also belonged to his estate, as did an abbey and considerable land in Furness, at some distance from Holker. His Yorkshire estate at Marske contained another large tract. He received returns from crops, timber, and minerals and rents from his many thousands of acres, but he was nevertheless constantly in debt and in the habit of borrowing money from his estate steward, a telling dependency.[97] The settlement shows that Lady Elizabeth Cavendish brought £6000 to the marriage, a welcome addition to Thomas's precarious finances.

Charles often saw his sister Elizabeth, who named him godfather to her second child, a daughter who lived only a short time.[98] In a report on their daughter's death, Thomas wrote that Elizabeth was "in very great concern & trouble,"[99] and in letters beginning around this time, Thomas included regards from his sisters but no longer regularly from his wife, as he had in the past. The spunky Elizabeth, who wished she had been a boy so she could have gone abroad with her brothers Charles and James, was placed in the hands of physicians "to try what effect it will have upon her to make her of better behaviour." She was considered insane by the time she died. Her husband, Thomas, a sportsman who was fond of horse racing, a kind but improvident man, lapsed into heavy drinking and more debt.[100] In 1745 he died without a will. In the month after his death, at his surviving child William's request,

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[96] John Barrow (1849, 146).

[97] The first survey of the Lancashire estate in 1775, thirty years after Sir Thomas's death, listed Cartmel-Holker at 2,860 acres and Furness at 3,559 acres. J.V. Beckett (1977b, 47–51).

[98] Thomas Lowther to James Lowther, 8 Aug. 1728, Cumbria Record Office, Carlisle, D/Lons/W/ 39.

[99] Thomas Lowther to James Lowther, 26 Sep. 1728, ibid.

[100] His debt was £4880 at his death. Beckett (1977b, 51).

Charles Cavendish together with the duke of Devonshire and another relative Lord Lonsdale agreed to serve as guardians during William's minority.[101]

Elizabeth declined the executorship, and William asked Charles Cavendish to be administrator of the estate for his benefit.[102] To carry out his responsibility, Cavendish corresponded with the steward at Marske in Yorkshire and with the steward at Holker in Lancashire, John Fletcher, requesting full information about the estate, which included a variety of properties in addition to buildings and land such as iron pits and a fishery. In his many letters and lengthy notebooks dealing to his administration, Cavendish considered a range of issues, including debts, arrears, rents, bonds, interest, dividends, furniture, pictures, books, household expenses, repairs, taxes, corn, hay, pigs, asses, cattle, and horses. Having learned that the "proper method" for an administrator was to publish a sale, he pressed Fletcher for valuations of everything that was to be sold, overlooking nothing. "As to the dogs you [Fletcher] say that people are more inclined to beg than to buy, but my business is to sell & not to give & therefore I desire you will inquire whether you can get any thing for them." He supposed there would be no point in selling the dogs to the guardians for William to use, for by the time he came of age, "they will most of them being worn out."[103] Cavendish was both administrator and one of the guardians, which added a level of complexity as he wished to avoid any dispute between the two. On various points, he obtained an opinion from the attorney general. As the executor, he was well-organized, thorough, and insistent on adhering to the methods he set out.

Problems naturally came up, the first of which was Fletcher, who was slow to understand and made mistakes in his accounts, causing Cavendish "a great deal of trouble."[104] He was told to prepare as soon as possible a "perfect state of all the effects whatsoever belonging to Sir Thomas at his death & all of the sums due from him at that time."[105] Cavendish was dissatisfied with the result: "I can't suppose you think it [what Fletcher sent him] such an account as I asked for, nor such as is necessary for me to have in order to know the true state of Sr Thomas's affairs." The next month he wrote again, explaining how to make up his accounts. "I think this method necessary for the regularity of my own accounts in which I must enter a state of all moneys due to the personal Estate of Sr Th. Lowther at the time of his death & of all debts then due out of it."[106] Cavendish repeated his instructions over and over. Fletcher was old and ill, and in the spring of 1746, he died, succeeded by his capable son-in-law, William Richardson, easing Cavendish's work. Cavendish told the new steward that in dealing with Sir Thomas's creditors, "I have laid it down for a rule to pay every body in proportion as every creditor has an equal right & I suppose is equally desirous to receive his money, & if I depart from that rule in one case there will be no end of solicitations, so that though I am very sorry any person that wants his money should be kept out of it I see no help

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[101] Edward Butler to John Fletcher, 16 May 1745, Lancashire Record Office, DDCa 22/3/1.

[102] Charles Cavendish to John Fletcher, 18 July 1745, draft, Devon. Coll., L/43/14. Charles was sworn in as administrator on 30 July 1745. Charles Cavendish to John Fletcher, draft, 30 July 1745, Devon. Coll., box 43/14. This bundle contains one notebook of Cavendish's guardian account for William and two notebooks of his administrator's accounts and correspondence for Thomas Lowther's estate. Drafts of his letters to the estate stewards and copies (probably incomplete) of their letters to him are contained in this correspondence, 1745–48. Administrator appointment, 17 Aug. 1745, Devon. Coll., box 31/11.

[103] Charles Cavendish to John Fletcher, 27 July 1745, Lancashire Record Office, DDCa 22/5.

[104] Charles Cavendish to William Richardson, 13 Mar. 1746, draft, Devon. Coll., box 43/14.

[105] Charles Cavendish to John Fletcher, 20 July [1745], Lancashire Record Office, DDCa 22/5.

[106] Charles Cavendish to John Fletcher, 13 Aug. 1745, ibid.

for it." In the case of creditors who refused to accept only part of the principal, "unless they will agree each of them to take a part of their debt I must offer the whole to some of them & I should chuse to do it to those who make the most difficulty & I desire you will acquaint them with it."[107] In the case of tenants who were in arrears and who would not immediately pay what was due from them, Cavendish directed the steward to distrain their effects. Where this method was not legally allowed, he would recover arrears by legal action; Cavendish told the steward to send him the names of persons calling for that action. A year and a half after he had taken charge of the estate, Cavendish could write to the steward, "I can now be pretty certain that when Sir William comes of Age there will be money enough to pay all the debts, & it will save some trouble."[108] In his decisions, Cavendish was firm and clear, and he usually got the results he wanted.

Cavendish's sister and now widow Elizabeth needed care. He paid sums to "Dr Mead," likely the London physician Richard Mead, the head of his profession, "Dr Wilmot," "Dr Monroe" who received an "allowance," and an apothecary.[109] Elizabeth did not long outlive her husband, dying in 1747, while Cavendish was still active as administrator. In the same year, another Lowther, John, died, leaving most of his estate to William on the death of, or in jointure with, his mother, and Cavendish had to sort out the details of this property as well.[110]

Cavendish kept on friendly terms with his ward. When Sir William—after his father he was baronet—was at the university, Cavendish sent him books he asked for. He introduced William to his society, inviting him to dinner at his house with scientific friends.[111] In 1753 William was appointed lord lieutenant of Westmoreland, and in 1755 he succeeded his relative Sir James Lowther in the Cumberland seat, a promising start on what looked to be a fine career.

Sir James Lowther was born in London and educated at Oxford and Middle Temple. Through inheritance, he became owner of valuable collieries and other properties around Whitehaven in Cumberland, on the northwest coast of England. He expanded his estate, lived frugally, and in time grew immensely rich, reputed to be the richest commoner in England. He made important improvements in the extraction and trading of coal, encouraged the production of iron in Cumberland, improved the harbor at Whitehaven, making it a major port for shipping coal, adopted technical improvements at his collieries, and was the first to install a Newcomen steam engine in Cumberland. After a visit to Whitehaven, Richardson said that he "did not imagine to have found so many new contrivances."[112] Lowther's colliery steward Carlisle Spedding dug the second undersea coal mine in England, Saltom Pit. Thomas Lowther reported to James after a shipment of coal from Saltom had arrived that everyone said that these were the "finest coals that ever came into this country." William

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[107] Charles Cavendish to William Richardson, 13 Mar. 1746, draft; Charles Cavendish to William Richardson, 20 May 1746, draft; Charles Cavendish to William Richardson, 20 May 1746, draft, box 43/14. William Richardson to Charles Cavendish, 2 May 1746, copy, ibid.

[108] Charles Cavendish to William Richardson, 21 June 1746; Charles Cavendish to William Richardson, 27 Dec. 1746, Lancashire Record Office, DDCa 22/7.

[109] On his sister's behalf he also paid "Mr. Duffield," who received regular pavements up to £180 each time, and "Mrs. Potter." Various dates in "Guardians Account" and in an untitled notebook containing six pages of accounts, 1745–48, Devon. Coll., box 43/14.

[110] On 9 Jan. 1747, the steward, Danby, for the Yorkshire estate informed Charles that John Lowther had died. "Sr W. Lowther's Estate," Devon. Coll., box 43/14.

[111] 5 June 1753, Thomas Birch Diary.

[112] Thomas Lowther to James Lowther, 6 June 1734, Cumbria Record Office, Carlisle, D/Lons/W.

Brownrigg, a physician in Whitehaven who took a medical interest in the firedamp that miners breathed, was "earnestly solicited" by Lowther to study the problem.[113] In 1736, Lowther was elected to the Royal Society, with Charles Cavendish's support.[114] Progressive and scientifically minded—a friend mentioned Lowther's "old Acquaintance Sr Isaac Newton"[115]—Lowther was the kind of industrialist Charles and Henry Cavendish shared interests with.

Thomas had been close to James; they corresponded regularly, and Thomas paid visits to Whitehaven.[116] James died in January 1755, and having no children of his own, he left his collieries and extensive lands in Cumberland to Thomas's son William. James was not related to the Cavendishes, but William of course was, and his inheritance was viewed as a coup for the family. Lord Hartington, soon to be fourth duke of Devonshire, was congratulated, "I must wish yr Lordship Joy of the very great Acquisition made by your near Relation Sr W. Lowther, which I am credibly informed, is 4000 £ a year in Land, Coal Mines bringing in 11,000 £ a year, & not less than 400,000 £ in Money. Sr James Lowther has 100,000 £ & an Estate in Middlesex."[117]

In the spring of the following year, 1756, William Lowther contracted scarlet fever. Katherine, wife of the recently deceased third duke of Devonshire, wrote to the fourth duke William that "every body is in great pain for Sr Wm Lowther." He had been ill for a week or ten days, attended by "Shaw & Heberden." The day she wrote, William had had "a very bad night," and his doctors had called in "Willmot," who ordered more blisters. She wrote a postscript to the letter, saying that Charles Cavendish was just there to tell her that Sir William had died.[118] On the same day, the duke received a consoling letter saying that persons who knew William thought he had "left the Chief part of His fortune to Your Brothers."[119] The "Chief part of His fortune" referred to Holker Hall, which we return to below.

A second Sir James Lowther was remembered in the will of his relative Sir James Lowther of Whitehaven. When William Lowther died, he was twenty-eight and unmarried, and because he had no children, the Cumberland estates, which he had recently inherited, reverted by Sir James Lowther's will to young James Lowther, then age nineteen.[120] Commenting on this inheritance, the Reverend Theophilus Lindsey wrote to the earl of Huntington of the "immense accession to young Sir James Lowther's own fortunes by the death of Sir William, and the distribution of the unentailed fortunes of the latter among the Cavendishes, Lords John, George and Frederick, his relations."[121] The fortune of young James Lowther caused Horace Walpole to fear that England was becoming the "property of six or seven people."[122]

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[113] Joshua Dixon (1801, 5).

[114] Cavendish signed Lowther's certificate. 20 May 1736, JB, Royal Society 15:331.

[115] Henry Newman to James Lowther, 26 Aug. 1732, Cumbria Record Office, Carlisle, D/Lons/L1/1/53.

[116] Thomas Lowther to James Lowther, 11 July 1734, ibid., D/Lons/W/37. There are many letters from Thomas to James Lowther in the Carlisle archive. Charles Cavendish also visited Whitehaven.

[117] H. Fox to Lord Hartington, 4 Jan. 1755, Devon. Coll., no. 330.30.

[118] K. Devonshire to duke of Devonshire, 15 Apr. 1756, ibid., no. 344.8. We assume the letter writer is Katherine, wife of the recently deceased 3d duke of Devonshire.

[119] Ducannon to duke of Devonshire, 15 Apr. 1756, ibid., no. 294.46.

[120] Beckett (1977b, 52). Also William left all of the buildings at Cockermouth, near Whitehaven in Cumberland, to Charles Cavendish to hold in trust for young James Lowther.

[121] Theophilus Lindsey to Francis Hastings, 10th earl of Huntington, 25 May 1756, in Great Britain, Historical Manuscripts Commission (1928–47, 3:117).

[122] Horace Walpole to George Montagu, 20 Apr. 1756, in Walpole (1937–1983, 9:183–187, on 185).

In his will, William named his former guardian Charles Cavendish as his executor.[123] He left his money, stock, goods, chattels, and personal estate not otherwise specified to Cavendish in trust to pay for his funeral expenses and his legacies and to pay off his debts. What remained of the personal estate after these payments he left to Cavendish as his executor. Because he lived in London, Cavendish depended on the steward at Holker, Richardson, to provide him with information he needed from William's estate at both Holker and Whitehaven. His letters to Richardson tell us about his actions and problems. Other than for the pictures, which were to remain in Holker Hall, none of the furnishings in any of William's houses was specifically given in his will, so "the whole" belonged to Cavendish. That was the easy part. He needed to know what particulars belonged to William's personal estate and what their values were and which of them young Sir James wanted to buy. Because much of William's estate was in Cumberland, he depended on John Spedding, steward to the late James Lowther and after him to the late William at Whitehaven. To keep the money coming in, Cavendish allowed Spedding to continue to use what he needed from the personal estate to carry on the coal trade. He told Richardson to go to Whitehaven and talk to Spedding to learn what at the collieries belonged to William's personal estate. He sent him off with a list of particulars that he thought belonged.[124] Cavendish set about with evident total confidence to settle the affairs of this complex estate.

There was a difference of opinion on who owned the steam engines at the pits, and on the value of the ships and of the leasehold collieries and estates. Cavendish confided to Richardson his concern about having to depend on Spedding for valuations, asking how much trust he could place on the accounts he received from him. He understood that Spedding would be partial to the owner of that estate, who was then young James, but he was "intitled to a full discovery [of all Sir Williams personal estate] by Law as well as from the principles of justice." In all disputes of interest, he told Richardson, it was his "desire to act with perfect openness & candour," having "not in the least desire to get anything which I am not justly intitled to." He suspected that measurements of the quantities of some stores "may not have done me strict justice," but he did not know what to do about it other than to insist that Spedding give him strong assurances of the "truth" of the inventory before signing an agreement with him. Richardson thought that some of the prices Cavendish demanded were too high. Cavendish told him that he had no objection to lowering them if he saw fit, explaining that he did "not desire to have a farthing more than I have a right to."[125] Charles Cavendish spoke of "principles of justice," "strict justice," "openness," candor," and "truth." We meet these words again in his son Henry's business affairs.

From letters to his steward, we see the estate from Cavendish's point of view. We have another point of view from Catherine Lowther, who told her son, young James, that "Lord C – is determined to give you all the trouble in his power; you must therefore make the best of it."[126] Having "great calls for money,"[127] she was "very pressing to have the affairs at Whitehaven settled," but Cavendish would not settle until he knew what the personal

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[123] Will of William Lowther, dated 7 Apr. 1755, probated 22 Apr. 1756, Devon. Coll., L/31/47.

[124] Charles Cavendish to William Richardson, 27 Apr., 13, 27 May 1756, Lancashire Record Office, DDCa 22/7. Cavendish's list: arrears of rent; bonds, notes, etc.; furniture, plate, etc.; coal debts; coals raised; wagons, carts, etc.; horses; tools; corn, hay, etc.; timber in yard; timber felled; material for buildings not used; ships; engines; leasehold estates & collieries.

[125] Charles Cavendish to William Richardson, 26, 29 June and 27 July 1756, Lancashire Record Office, DDCa 22/7.

[126] Catherine Lowther to James Lowther, 11 July 1756, Cumbria Record Office, Carlisle, D/Lons/L1/61.

[127] Catherine Lowther to James Lowther, 8 July 1756, ibid.

estate consisted of and what parts of it her son wanted to buy.[128] Cavendish was not without sympathy, but he would not bend his principles. He accommodated her immediate needs by advancing her any money she asked for from William's legacy to her of £6000, in discharge of all demands of the estate.[129]

We come to a major disagreement, which had to do with £30,000 in New South Sea Annuities that were put in trust to finance the transfer of William's estate to young James. Cavendish thought that the annuities were his because the transfer could not take place in the specified time, James not being of age. In July 1756, Cavendish and James agreed that the latter would bring a bill in the Court of Chancery against Cavendish to "have the right relative to the 30,000" and also the right relative to the leasehold estates and the steam engines and other equipment that went with them. Cavendish and James agreed on two other points: Richardson and Spedding between them would decide the values of the collieries and the furniture in the house at Whitehaven; and the legacies would be paid and the personal estate and the stock would be given to James when he came of age, while in the meantime he would receive dividends.[130] Upon reading the agreement, Catherine wrote to her son, "I think most of it very unreasonable," in keeping with "His Lords conduct."[131]

We will look at Cavendish's claims, for they show his hardheaded determination to acquire what he believed he was entitled to, even if only because of a legal technicality. Cavendish agreed that by Sir James's will, young James was entitled to the properties in Cumberland (with the exception of houses and land in Cockermouth) and to all of the stocks except the £30,000 in New South Sea Annuities. The main issue was whether this sum fell back into the stock from which it was taken (James's case) or whether it was separated and fell into the residue (Cavendish's case). Cavendish insisted that the £30,000 belonged to him as part of the residue of William's estate, since William died before young James was twenty-one, making the exchange of estates impossible. Cavendish also insisted that Sir James's leasehold estates in Cumberland, consisting mainly of coal mines together with steam engines and other equipment affixed to the estates, passed to him as William's residuary legatee. The cases were debated, and council on both sides was heard. The court decided that the £30,000 in annuities and James's leasehold properties belonged to James, and that Cavendish had to pay over the interest from the annuities to James. Whether the steam engines and so forth stayed with the land or went to the Cavendish as executor was left to the opinion of the master of the rolls. Cavendish appealed the decision.[132]

Repeatedly in his letters to Richardson, Cavendish used the expression "what belongs to me," or its equivalent. His letters read as though he was furthering his own interests, and that is how we originally read them.[133] But this was his way of speaking: he meant by it, what belonged to him in trust for uses specified in the will, with anything left over going to him as specified in the will. He administered a very large estate, and he went about it with his customary conscientiousness. There is another consideration. William was generous—

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[128] Charles Cavendish to William Richardson, 8 May 1756, Lancashire Record Office, DDCa 22/7.
[129] Cavendish to Richardson, 27 Apr. 1756.
[130] "Heads of What Is Agreed on between Ld Charles Cavendish & Sr James Lowther," [before 19 July 1756], Cumbria Record Office, Carlisle, D/Lons/L1/62.
[131] Catherine Lowther to James Lowther, 19 July 1756, ibid., D/Lons/L1/61.
[132] Packet of papers labeled in Henry Cavendish's hand "Sr W. & Sr J. Lowther's Wills & Papers Relating to the Law Suit between L.C.C. & Sr J. Lowther." Devon. Coll., 31/17.
[133] Christa Jungnickel and Russell McCormmach (1999, 93–94).

he tripled Spedding's pay when he acquired James Lowther's estate[134]—and his will was generous, granting specific legacies totaling £79,000.[135] At the time he made out his will, this sum, large as it was, would have been realistic, owing to his recent inheritance. The elder James Lowther's annual income was well above £20,000 a year, a good portion of which would have gone to William,[136] and his income from his father's estates, of the order of £4000,[137] would have paid part and perhaps most of his living expenses.[138] He could not have foreseen that he would benefit from James Lowther's wealth for so short a time. To realize the intent of William's will, Cavendish would have wanted to claim everything possible as personal estate and turn it into money. In his letters to Richardson, he spoke of his appreciation of his former ward, "a benefactor whose great fortune enabled him to do what the generosity of his temper prompted him to."[139]

When Catherine Lowther informed her son about William's death, she gave him advice about the great wealth coming to him. The "acquisition of fortune, cannot be any recompense for the want of so worthy a friend [William] & will only make you more the subject of envy than you have already been, & can in no shape conduce to yr happiness, either in this world or another, unless you use it, as he did, in doing good, otherwise will only draw upon you, misery in both."[140] Six months later, she reminded him that "it is a debt due to that Great Being, who has made you accountable for so large a portion of this worlds goods; whch if properly managed, will not only make you happy here, but eternally so." The world at first would look on him favorably "as a person endow'd by providence wth the power of relieving the distress'd, & making happy his fellow creatures," a power denied to a poor man, who can offer only prayer and hope.[141] James disregarded the advice, using his money for a different kind of power. He did some good for Whitehaven, for example, by setting up a manufactory for copper and stockings, but he grew into one of the "profligate wicked wretches" and "villains" his mother warned him against. He became known throughout the region as the "bad earl," distinguished equally by his unenviable character as by his immense wealth. James Boswell called him a "brutal fellow." Horace Walpole said he was "equally un-amiable in public and private." The Reverend Alexander Carlyle, a leader of the Church of Scotland, said that he was "more detested than any man alive." Through lavish expenditure, he kept mistresses and controlled nine members of Parliament known as "Sir James's Ninepins," who were required to vote as he ordered.[142] Otherwise, he was miserly, showing his contempt for common people by traveling in a rundown carriage pulled by ungroomed horses.[143] In his attitude toward money, James could hardly be more different

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[134] Beckett (1977b, 52).

[135] Plus several small annuities.

[136] Beckett (1977b, 64). Not all of James's income would have gone to William. For example, he left his South Sea annuities to young James, who would have received the dividends. Sir James Lowther's will, 1754, Devon. Coll., L/31/17.

[137] Beckett (1977b, 52).

[138] Because of his very short life as a very wealthy man, not much can be learned. His income from 5 July 1755 to 25 May 1756 (the month after his death) was £11,640. His expenses were £8251, which included large payments to Girolamo Belloni, the head of a family bank in Rome. "Sr William Lowther Bart His Account with Robt Snow & Willm Denne 1755," 5 July 1755 to 25 May 1756, Devon. Coll., box 43/14.

[139] Cavendish to Richardson, 8 May 1756. Cavendish directed his steward to continue William's generosity by distributing £50 to persons in the neighborhood who were most in need, as William would have done were he alive.

[140] Catherine Lowther to James Lowther, Apr. 1756, Cumbria Record Office, Carlisle, D/Lons/L1/61.

[141] Catherine Lowther to James Lowther, 28 Oct. 1756, ibid.

[142] "Lowther, James, Earl of Lonsdale (1736–1802)," *DNB*, 1st ed. 12:217–220, on 219.

[143] William Donaldson (2002, 409).

than William, his benefactor. Horace Walpole wrote to Montague five days after Williams death making the comparison: “Sir William Lowther has made a charming will, and been as generous at his death, as he was in his short life … but what do you think of young Sir James Lowther, who, not of age, becomes master of one or two and forty thousand pounds a year.”[144] We do not know what Cavendish thought. Through his execution of William’s will, he helped make possible this outcome, but he had no responsibility for it. That rested with Sir James’s character and the forces that shaped it.

To this point, we have not looked at what William placed at the head of his will and gave most attention to, Holker Hall. William left this house along with other manors, buildings, and lands to William Cavendish third duke of Devonshire and his eldest son “to the several uses upon the trusts.” Holker Hall was to go first to his own male offspring, of which he had none, in which event it was to go to his aunt Catherine Lowther for her “use” over the course of her life; and upon her death, the estate was to pass to George Augustus Cavendish for his use during his life; after his death, it was to pass to his younger brother Frederick Cavendish for his use during his life; and after his death, it was to pass to the youngest brother John Cavendish for his use during his life.[145] The three brothers were the younger sons of the third duke of Devonshire, nephews of Charles Cavendish’s, and first cousins of Henry Cavendish’s. None of the three brothers married.

Not long after William died, Cavendish heard from friends of Catherine Lowther “that she has thoughts of making over the estate to Lord George Augustus Cavendish for a proper consideration.”[146] This evidently was soon done. Lord George became the first male Cavendish to live at Holker Hall, making it his home for nearly forty years, until his death in 1794. In his final will he spoke of “the person or persons who shall upon my decease succeed and become entitled to the said House [Holker Hall]and Estate at Holker,”[147] wording which might suggest that there was uncertainty about his successor, but as directed by William Lowther’s will Holker Hall went next to Frederick Cavendish, who held it until his death in 1803.

Nowhere in William’s will is Charles Cavendish said to be entitled to Holker Hall, nor is he in George Augustus Cavendish’s and Frederick Cavendish’s wills. If what Henry Cavendish told John Barrow is correct, that Holker Hall was left to his father and his father left it to him, it is unlikely that his father acquired it from George Augustus Cavendish as Henry said it did; for by Sir Williams’s will, Frederick Cavendish was next in line. When Frederick died, his younger brother John, who was next in line, was already dead, and the beneficiaries named in Sir William’s will came to an end. If there was uncertainty, it may have come at this juncture, but so far as we can judge from his will, Frederick did not think there was any uncertainty, treating Holker Hall no differently than the rest of his property. With the exception of special legacies, he left “the Capital messuage or mansion house of Holker Hall with the park lands and hereditamenti” in the parish of Cartmel, Lancashire, together with his other properties to his nephew George Augustus Henry Cavendish and his

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[144] Horace Walpole to George Montagu, 20 Apr. 1756, in Walpole (1937–1983, 9:184–185).

[145] William Lowther’s will, 7 Apr. 1755, probated 22 Apr. 1756, Devon. Coll., L/36/47. He died on 15 Apr. 1756.

[146] Charles Cavendish to William Richardson, 28 Dec. 1756, Lancashire Record Office, DDCa 22/7.

[147] George Augustus Cavendish’s will, signed 9 Mar. 1792, probated 12 July 1794, Public Record Office, National Archives, Prob 12/1247. He died on 2 May 1794. He used the same expression for his estates in the county of Huntington: “at the time of my decease unto the person or persons who shall upon my death succeed or become entitled to those estates.”

heirs and assigns.[148] This George was also Henry Cavendish's principal heir and the Lord George that Henry told Barrow he was going to leave Holker Hall to. The land tax returns for Lower Holker, which includes Holker Hall, list Frederick Cavendish through 1803, the year he died, in 1804 the name changing to George [Augustus Henry] Cavendish.[149] Henry Cavendish's name does not appear. If he was entitled to Holker Hall, he did not occupy it and he did not pay land taxes on it. By the time Frederick died, Charles had been dead for ten years, and Henry had seven years to live. Henry Cavendish's conversation with Barrow was unlikely to have taken place before Barrow was elected to the Royal Society in 1805, at which time Henry had five years to live. Other than in contemplation, he had no occasion to enjoy the splendor of the mansion overlooking Morecambe Bay.[150]

There are three possible reasons why Henry Cavendish's ties to Holker Hall remain elusive. One is that we have missed something, either a document that has not yet been found or a right that a legal scholar would understand. Another is that Barrow's recollection is wrong, though it seems unlikely that he would remember Cavendish having said that he owned the manor if he did not say it. Third, Cavendish was confused about the ownership. He was normally very accurate, and we do not consider this possibility lightly. But let us see. To begin with, he certainly knew about his father's involvement with the Lowthers. When Charles Cavendish was appointed administrator of Thomas Lowther's estate in 1745, when he was Sir William's guardian in 1745–48, and when he became executor of Sir William's estate in 1756, Henry was fourteen to seventeen, and twenty-five. He was away at school for part of the time, but at other times he was home, and he would have known that his father made journeys to the Lowther properties and why. Later he himself was involved: Charles Cavendish and after him Henry were trustees of Cartmel Rectory, part of the Lowther estate: the bishop of Chester leased Cartmel Rectory to Henry Cavendish in trust for the persons entitled to it under Sir William Lowther's will, who were the persons entitled to Holker Hall, George Augustus Cavendish and Frederick Cavendish, followed by George Augustus Henry Cavendish.[151] After his father's death, Henry made an inventory of the contents of a walnut cabinet he kept in his own bedchamber, which included William Lowther's and James Lowther's wills and papers relating to the lawsuit between Charles Cavendish and James Lowther.[152] Henry made a list of his father's papers, which contained letters about William Lowther's estate,[153] and he made a list of keys, which included keys to William Lowther's chest of drawers and trunk.[154] Henry lived among the relics of his father's dealings with the Lowther family, including all the paperwork, but he may never have looked at it. It is written in legal language and is extensive, and the transfer of Lowther property was, as we have seen, complicated. It would have taken him time to master it, to no obvious purpose. In light of the history of the Lowther estate, if Henry made a mistake about it, he is forgiven.

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[148] Frederick Cavendish's will, signed 24 Jan. 1797, probated 29 Oct. 1803, Prerogative Court of Canterbury, PROB 112/1399/369.

[149] The 1803 land tax return was dated 7 July. The 1804 land tax return was dated 28 June. George Augustus Henry Cavendish's name is listed from 1804 through the year of Henry Cavendish's death, 1810, and beyond. Lancaster County Archives, QDL/LN/23.

[150] From his conversation with Barrow, it seems that Cavendish knew the manor and its setting. Possibly his father brought him there on one or more of his visits. In 1786, on a journey with Blagden, he passed into Cumbria, but there is no mention of Holker Hall. Blagden to Banks, 4 Sep. 1786.

[151] The documents are in Devon. Coll., L/36/62.

[152] Henry Cavendish, "Walnut Cabinet in Bed Chamber," Devon. Coll.

[153] Henry Cavendish, "List of Papers Classed," ibid.

[154] Henry Cavendish, "Keys at London," ibid.

He may have remembered incorrectly, or misinterpreted something his father once told him about Holker Hall, or was given an account by his father at variance with the record, part of which his father disputed. The interest of this episode is what it tells us about our subject: Henry Cavendish had the normal English aristocrat's desire to improve his country estate, recalling his maternal grandfather Henry de Grey's ambitions for Wrest Park.

Chapter 5
Public Activities

Public Life

Charles Cavendish's administrative skills were valued in arenas outside of family affairs, politics, and science, in the founding and working of several organizations. Each of the organizations had a technical dimension, and the people he worked with were often the same people he worked with in politics and science. In the first section of this chapter, we briefly consider the organizations, beginning with a hospital.

For twenty years Robert Walpole kept the country in peace and prosperity, during which time several hospitals were established, Westminster in 1720, Guy's in 1724, and others. These were hospitals in the usual sense of the word. In addition there was a new charitable hospice for unwanted children, the Foundling Hospital (Fig. 5.2). Inspired by foundations for this purpose in Amsterdam, Paris, and elsewhere, the Foundling Hospital was the culmination of an arduous and heartfelt campaign by Thomas Coram on behalf of "great numbers of Helpless Infants daily exposed to Destruction." The Hospital was incorporated by royal charter in 1739 in a ceremony attended by bankers and merchants from the city and by six dukes and eleven earls, who set the tone of the endeavor. The charter, which was received by the president of the Hospital, the duke of Bedford, a relative of Cavendish's, named Cavendish's brother, the duke of Devonshire, and his father-in-law, the duke of Kent, as original governors, and Cavendish himself was elected governor later that year.[1] The Hospital was first located in a leased house, but soon it acquired a new building set in the fields, the location of most of the other new institutions of eighteenth century London. The interior of the building was adorned with paintings; elegant concerts were held there.[2]

This fashionable charity needed administrators who were both able and hardened to the task, for conditions of life in an eighteenth century foundling home were depressing. During the first four years the Hospital admitted children indiscriminately, whether or not they were true foundlings—exposed and deserted children who would otherwise die—nearly 100 a week at times. Of the roughly 15,000 children received then, over 10,000 did die, a mortality rate of about seventy percent. From the provinces, infants were transported under desperate conditions to the Hospital, where they were dumped, sparing parish officials the trouble and expense of maintenance. To avoid the cost of burial, parents abandoned children there, more dead than alive. The administrators of the Hospital had to deal with the consequences of their policy and ultimately with the policy itself.

The Hospital could call upon the best medical opinion in London. Hans Sloane, president of the Royal Society, and Richard Mead, both of whom were named in the charter, were among the leading physicians who volunteered their expensive services. William Wat-

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[1] R.H. Nichols and F.A. Wray (1935, 16, 19). Roy Porter (1982, 302–303).

[2] John Summerson (1978, 119–120).

son, an expert on infectious childhood diseases, led the Hospital's crusade to prevent the devastations of smallpox, then a disease primarily of children under three.[3]

With the desire to put its children to work, the Foundling Hospital turned for help to the whitefish industry. The Society of Free British Fisheries recommended fitting up the rope yard for spinning twine and making net, agreeing "to take as much Yarmouth Shale as the children could braid." Cavendish was active at both ends of this arrangement, as a governor of the Foundling Hospital and as a member the Society.[4] The inconclusive end of the War of the Austrian Succession in 1748 was the setting of the start of the Society. There was a widespread feeling then that the nation needed to be strengthened, and a natural way of doing this was to encourage its fisheries, by then an old idea. When in 1749 the House of Commons formed a committee on the state of British fisheries, a group of traders and merchants responded by submitting a plan for a fishery company, which resulted in a Parliamentary act. In 1750 the Society was incorporated under a royal charter. Modeled after the great chartered trading companies, the Society was justified by the need for British fisheries to compete successfully with the Dutch, who then dominated the trade in herring. It had three main objectives: to strengthen British commercial power through incentives to build up the fishery, to secure Britain against hostile rivals especially France by ensuring a supply of seamen, and to provide employment for the laboring class. There were anticipated side benefits. It would improve the moral character of the nation by eliminating the uncivilized practice of impressing seamen; rebuild the economy in depressed regions, especially the Highlands, indirectly reconciling the Scottish clans to a United Kingdom; lower the poor rate by putting the unemployed to work; and discourage crime, drink, gaming, irreligion, and other forms of social disorder. The Society was permitted to own ships, build warehouses and wharfs, carry naval staples, regulate trade, and raise capital for these purposes in the form of joint stock paying three percent semi-annually. It was popular at the beginning, fueled by anti-Dutch sentiment and a perceived threat from France, but expectations for it were soon disappointed. By the mid-1750s the Society was in trouble for a number of reasons: the start of the Seven Years War, the rise of the Swedish fishery, the movement of herring away from the west coast of Scotland, poorly thought-out regulations on the conduct of fishing and curing of fish, and more.[5] We need not go into this any further since we do not know what part Cavendish played.

We do know that Cavendish took an active interest in the Society of Free British Fisheries, as we would expect, repeatedly serving on its Council. The industrialist James Lowther of Cumberland, a distant in-law whose estate he would later take charge of, was a moving force behind the fishery from the start. Its first governor was Frederick, prince of Wales whose gentleman of the bedchamber he had once been. His fellow Member of Parliament from Derbyshire Nathaniel Curzon was on the Council of the Society, as was his close friend and colleague William Watson. Possibly Cavendish's interest began with the Society's contract with charities and parishes including the Foundling Hospital to make nets. He must

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[3] Ruth K. McClure (1981, 205–218). William Watson (1768). Charles Creighton (1965, 500, 514).

[4] Nichols and Wray (1935, 131, 182).

[5] Francis Grant (1750, 37). Anonym (1750a, 13, 46). Anonym (1750b). Mr. Horsley (1750). Bob Harris (1999, 285, 291, 293, 296, 298, 304, 307).

have thought that the fishery would be good for the country, and in any event it was a venue where he could perform a duty of service.[6]

Closer to Cavendish's scientific and scholarly interests was the British Museum. Readers of books lacked a proper public library in London. The Universities of Oxford and Cambridge had libraries, cathedrals had them, and there were a few specialized libraries, such as the one for law at the Inns of Court and the Royal Society's own library; even a few small public libraries had been established in London, but most readers could not readily lay their hands on a given book. By this yardstick of civilized society, England was decidedly backward. Italy had had important public libraries since the fifteenth century; in Prussia, Berlin had had a great public library since the late seventeenth century; in France the royal library in Paris had been open to the public since 1735, and the Mazarin library was nearly as large; and other great European cities such as Vienna and Munich had major public libraries.[7] With the assistance of Cavendish, London belatedly acquired an important public library as part of a new institution, the British Museum.

In the usual British way of addressing social needs, a public library in London came about through private rather than government initiative. When Hans Sloane stepped down as president of the Royal Society in 1741, the secretary Cromwell Mortimer, in the dedication of a volume of the Society's *Philosophical Transactions,* referred to his "noble and immense Collection" in natural history and to his large library of books on natural history and medicine, inflated to the "most complete in the Universe."[8] When Sloane died in 1753, he left to the nation his natural history collection and his library, for a price. Parliament accepted the offer, raising the necessary money by means of a lottery. Sloane's trustees bought Montagu House to hold his collection and library, to which were added the Cottonian Collection and the Harleian Manuscripts. Open and free to "all studious and curious Persons,"[9] Montagu House was occasionally referred to as Sloane's Museum, but it would be known as the British Museum.

Sloane's will did not name Cavendish as one of the trustees, but it included him in a long list of "visitors," starting with the king and the prince of Wales, who were charged with watching over Sloane's possessions.[10] To get from the dignitaries to the working staff—the librarian and underlibrarians—Parliament approved a complicated plan. A manageable but still large number of persons were selected from the trustees and visitors and given the responsibility of electing fifteen persons. These so-called "elected trustees" were to appoint a standing committee to meet regularly with the staff and take charge of the management of the Museum. Cavendish became a trustee in the first election, in 1753, and he was appointed to the standing committee in its first year, in 1759. The latter included Cavendish's relative Philip Yorke and his close friends and colleagues Watson, Birch, and Macclesfield.

From the start, the British Museum was warmly welcomed by fellows of the Royal Society, who volunteered their services. Most of the first trustees were fellows of the Royal Society or of the Royal Society of Antiquaries or both; eleven of the first elected trustees

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[6] Harris (1999, 286, 291, 305–306, 308, 312). Cavendish was a member of the Council of the Society in 1756, 1763, 1764, and likely in other years too. "The Monthly Chronicler," 30 November, *The London Magazine. For November, 1756.*

[7] Edward Miller (1974, 25).

[8] Dedication on 31 Dec. 1741, a month after Sloane's resignation: *PT*, vol. 41, for 1739–40, published in 1744.

[9] Arundell Esdaile (1946, 18).

[10] Sloane's printed will: BL Add Mss 36269, ff. 39–54. A handwritten list in 1753 of additional trustees includes Cavendish, f. 57.

were fellows of the Royal Society, and of the thirty-one trustees who were elected from the beginning of the British Museum until the year Cavendish died, twenty-three were fellows of the Royal Society. Of the thirty-seven trustees who were named to of the standing committee during Cavendish's lifetime, twenty-eight were fellows of the Royal Society, including four of its presidents.[11]

Cavendish was involved in every stage of preparation for the opening of the Museum in 1759. As a member of the standing committee, he examined Sloane's insects, birds, and other animals, finding some in good condition and others in a predictable state of decay. He helped to inspect Sloane's books and to compare the contents of Sloane's cabinets with catalogs in forty-nine volumes. By 1755 Cavendish's name sometimes headed the list of trustees at the general meetings, despite the number of peers who could come and whose names would have preceded his if they had. In time attendance at the weekly committee meetings dropped to five or so, but Cavendish always came, and when Macclesfield did not come, Cavendish presided, or at least he headed the list of persons attending: in the six months from May to November 1755, Cavendish attended thirty-four meetings of the standing committee, at twenty of which he presided.[12] Cavendish was a man of public affairs with broad interests and administrative skills, who could be counted on absolutely, not the least of the reasons why his services were valued in the British Museum and generally in the affairs of the learned world of London.

Places of Public Service

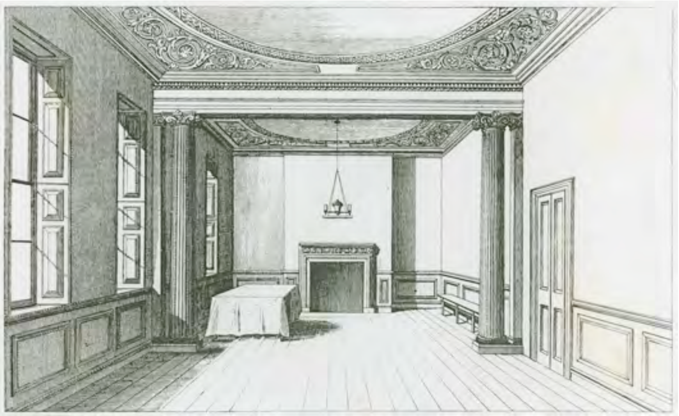

Figure 5.1: Royal Society. Through Charles Cavendish's time, the Royal Society met in this room at Crane Court. It had long departed when this print was made in 1848. Frontispiece to the first volume of Charles Richard Weld, *A History of the Royal Society*, 2 vols. (London, 1848).

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[11] Esdaile (1946, 30, 323). A.E. Gunther (1979, 209–210, 214–215).

[12] Thomas Birch's minutes of the meetings of the trustees of the British Museum: BL Add Mss 4450, ff. 1 and following. "Minutes of the General Meetings and the Standing Committee Meetings of the Trustees of the British Museum," ibid., 4451, ff. 3 and following.

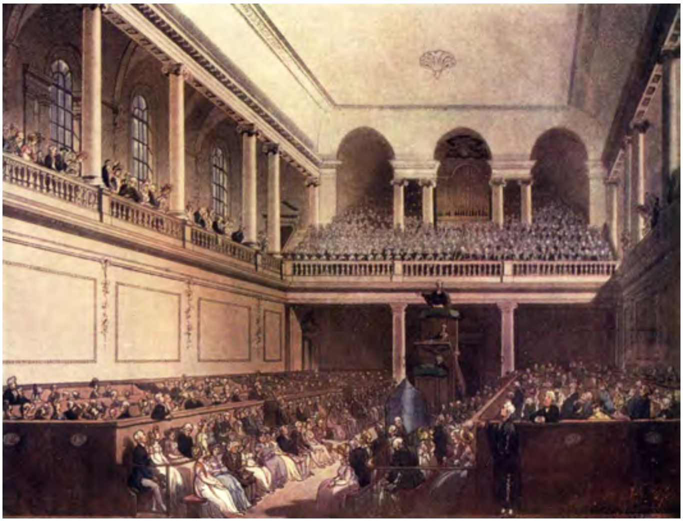

Figure 5.2: Foundling Hospital. The Chapel. By Thomas Rowlandson and Augustus Charles Pugin for Ackermann's *Microcosm of London* (1808–11). Demolished. Lord Charles Cavendish was a governor of this institution from the year of its charter, 1739. Wikimedia Commons.

Figure 5.3: British Museum. Entrance to the Old British Museum, Montagu House. Visitors are seen entering from the left; through the arched gateway on the right visitors are seen on the staircase. The statue inside the room is of Joseph Banks, former president of the Royal Society. Charles Cavendish became a trustee of the Museum at its first election in 1753. Henry Cavendish was elected a trustee in 1773.

Figure 5.4: British Museum. Staircase of the Old British Museum. Visitors are shown on the stairs and on the landing looking at stuffed animals. The giraffes look to be outgrowing the house. This was true in a sense, for by the time of this painting, most of the contents of the overcrowded and dilapidated Montagu House had been removed to the new home of the Museum. Watercolors by George Scharf, the elder, 1845. Reproduced by permission of the Trustees of the British Museum.

Figure 5.5: Building of Westminster Bridge. Painting by Samuel Scott, circa 1742. The bridge is shown in an early stage of construction. Lord Charles Cavendish was an active bridge commissioner from 1736 to 1749, the eve of its opening. Wikimedia Commons.

Figure 5.6: Westminster Bridge. By Giovanni Antonio Canaletto, 1747. The Lord Mayor's procession on the River Thames. This second bridge in London over the river is nearly finished in this painting; final construction can be seen at the far right. Courtesy of the Yale Center for British Art, Paul Mellon Collection.

Montagu House, which earlier had nearly been acquired by the Foundling Hospital, was located at the north end of town on Bloomsbury Square, the fashionable home to rich physicians such as Sloane and Mead (Figs. 5.3–5.4). Designed in the French style for Ralph, later first duke of Montagu, by Robert Hooke, the versatile curator of experiments of the Royal Society, the original house had burned down, replaced by a house resembling a contemporary Parisian *hôtel*. It had an imposing façade with colonnades, an entrance topped by a cupola, wings extending to the front to form a courtyard, an interior of spacious and lofty apartments with paintings on the walls, and, in general, the grandeur befitting a great library and scientific collection in the nation's capital. Given the great load it was to bear, of equal significance was the sober evaluation by the standing committee, to which Cavendish belonged, that the house was a "Substantial, well built Brick Building." Seven and a half acres of garden came with it, to which Cavendish's friend and fellow trustee William Watson devoted loving care.[13]

The collections of the British Museum were dedicated to the "Advancement and Improvement of Natural Philosophy and Other Branches of Speculative Knowledge."[14] The senior staff consisted of a "principal librarian" and three "under-librarians" or "keepers," each with an assistant, corresponding to the three departments: Printed Books, Manuscripts, and Natural Productions and Artificial Curiosities. There was in addition a keeper of the reading room, considered an assistant librarian.[15] The scientific ambition of the Museum was clear from the qualifications of the principal librarian, who was expected to be studious, learned, educated as a physician, versed in mathematics, a judge of inventions, able to carry on conversation with the learned in their fields, and competent to write and speak

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[13] Esdaile (1946, 39–40). Miller (1974, 50–54).

[14] "Rules proposed to be Observed in Making the Collections of Proper Use to the Public by Way of Resolutions in a General Meeting of the Trustees," BL Add Mss 4449, f. 115.

[15] Gunther (1979, 210). P.R. Harris (1998, 12).

French and Latin and correspond with foreigners.[16] There were disqualifying criteria too, which were not mentioned.[17] Gowin Knight, the choice for principal librarian, presented himself as a physician who had devoted the greatest part of his life to the "pursuit of natural Knowledge";[18] the evidence, his powerful artificial steel magnets, he brought with him to the British Museum, requesting a passage five feet wide to house them.[19] Matthew Maty, De Moivre's friend, who was appointed underlibrarian for Printed Books and would one day become principal librarian, had accomplishments equally impressive: he had received an M.D. under Boerhaave at the University of Leiden, he had studied natural philosophy and mathematics, he had wide-ranging foreign connections as editor of the *Journal Britannique*, and he spoke French and Dutch;[20] soon after joining the staff of the British Museum, Maty was elected secretary of the Royal Society. Another underlibrarian was Charles Morton, physician to the Middlesex and Foundling Hospitals, who like Maty had received an M.D. at the University of Leiden; he would become secretary of the Royal Society, and he too would one day become principal librarian.[21] A third underlibrarian was the naturalist James Empson, charged with overseeing Sloane's natural history collection. As each underlibrarian had an assistant, the staff was sizable and, in William Watson's opinion, "unexceptionable." Its "disposition," however, was a different matter, as librarians and assistants were not on speaking terms, and insubordination was rampant. The poet Thomas Gray, one of the first users of the library of the British Museum, said that "the whole society, trustees and all, are caught up in arms," and he compared the rebellious factions to "fellows of a college."[22] Watson analyzed the conflict in terms of turf and abilities.[23]

At first, a two-month reservation was required to secure a seat in a dark space in the basement used as the reading room, but after the novelty wore off the room proved ample; a few months after the Museum had opened, Thomas Gray found himself one of only five readers, the others being the antiquarian William Stukeley and three hacks copying manuscripts for hire.[24] In its first year, alongside Gray, several scientific readers visited the

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[16] "Qualifications and Duty Required in the Principal Librarian," BL Add Mss 4449, f. 108. "Rules Proposed to be Observed in Making the Collections of Proper Use to the Publick by Way of Resolutions in a General Meeting of the Trustees," ibid., f. 115.

[17] Emanuel Mendes da Costa applied to be an underlibrarian at the British Museum with these credentials: he was a longtime fellow of the Royal Society, an expert on fossils, and fluent in the all of the main languages. Letter to Lord Hardwicke, 4 Feb. 1756, BL Add Mss 36269, ff. 100–101. William Watson considered Da Costa to be eminently qualified, but his "religion is an unsurmountable object." Letters to the archbishop of Canterbury, 21 June 1756, and Lord Hardwicke, 22 June 1756, BL 36269, ff. 139–142, 144–145. A few years later Da Costa asked Thomas Birch if it was "obnoxious to the Society that I (as by Profession a Jew) can put up for Hawksbee's place" in the Royal Society. Letter of 17 Jan. 1763, BL Add Mss 4317, f. 113.

[18] Gowin Knight to Lord Hardwicke, and 22 Sep. 1754, BL 36269, ff. 29–30.

[19] BL Add Mss 36269, f. 134.

[20] J. Jortin to Lord Hardwicke, n.d. and 12 Feb. 1756, BL Add Mss 36269, ff. 104–106.

[21] "Morton, Charles," *DNB,* 1st ed. 13:1047–48,

[22] Edmond William Gosse (1906, 142).

[23] The underlibrarians were naturalists, and their assistants were antiquarians, an unworkable combination, it turned out. The different parts of the British Museum required different talents, which had to be properly assigned, Watson explained: "We have an extensive collection of the productions of nature & of art; a very large medical & philosophical library; as well as one relating to antiquities, & a vast collection of coins." The friction among the staff was rooted in this fact: "it must require a great length of time for any person to have a competent knowledge of any one branch of the Museum & unless he be acquainted with it, he will be but little qualified to instruct others." The proper persons had to be matched up with the proper subjects. Typical good sense from William Watson to the archbishop of Canterbury, 21 June 1756.

[24] Gosse (1906, 141–142).

room, Watson, Heberden, and John Hadley among them.[25] Readers were admitted for six months at a time upon recommendation; members of the Royal Society and other learned bodies were admitted without recommendation. From its modest beginnings, the library eventually became the national library, and the natural history collection grew into a major research center. This successful institution had no more assiduous early administrator than Charles Cavendish.

Cavendish's own researches were directed to questions of basic science, but he was interested in the uses of science too. On 8 June 1757, he was elected a member of the Society of Arts, founded three years earlier to encourage hopeful applications of knowledge by awarding prizes from money donated by public-spirited supporters of progress. Given the aims of the new society, its membership naturally overlapped that of the Royal Society: of the eleven founding members of the Society of Arts, four were fellows of the Royal Society, and twenty years later the president and all ten vice presidents of the Society were fellows of the Royal Society. Macclesfield, Franklin, Knight, Heberden, and Watson, to name several of Cavendish's friends, were members; it was Watson who proposed Cavendish. The Society attracted a strong aristocratic patronage as well; relatives of Cavendish's belonging to it included the dukes of Devonshire and Bedford, the earls of Bessborough and Ashburnham, Viscount Royston, and Lord George Cavendish. Cavendish was not active in the Society of Arts as he was in the Royal Society and the British Museum, but it is indicative of the breadth of his public interests that in 1760 he was appointed to special committees for judging competitions in the fine arts, technology, and agriculture.[26] He kept up his membership to the end of his life.

The bridging of the River Thames at Westminster was a highly visible application of knowledge of materials, structures, and machines. The early eighteenth century saw both the rapid improvement of roads through turnpiking and the beginning of bridge building on a large scale. A major impetus was the growth of London, by then the largest city in the world, the demands of which on the still largely agricultural nation were vast and insatiable. Herds of cattle were driven down turnpikes and over bridges to feed the concentrated mass of humanity on the banks of the Thames. Here and there streets of the city led to stairs down to the river, where cursing boatman ferried paying passengers to the opposite bank. London Bridge, the only bridge in the city, was medieval, dangerous, congested, and built up with houses. Ideas for improving transportation by a second bridge, discussed since Elizabethan times, had been successfully resisted by impecunious monarchs, water men defending their livelihood from ruin, and parties expressing fears such as commercial competition, armed rebellion, and the falling down of London Bridge once it was neglected for a rival.[27]

Renewed interest in a new bridge took the form of two petitions to Parliament in 1721, leading to a committee and a bridge bill. The House of Commons did not act, probably for political reasons, since Walpole, who favored the bridge and was on the committee, was well hated by then.[28] When in 1736 another petition for a bridge was submitted to the Commons, the resulting committee, which could hear testimony of any kind, chose to hear technical testimony, undoubtedly hoping in this way to avoid the commercial controversy that had

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[25] "Persons Admitted to Reading Room Jan. 12. 1759 to May 11. 1763," BL Add Mss 45867.

[26] 26 Mar., 9 and 30 Apr. 1760, Minutes of the Society, Royal Society of Arts, 5. Derek Hudson and Kenneth W. Luckhurst (1954, 6). Royal Society of Arts (1768). Henry Trueman Wood (1913, 28–46).

[27] R.J.B. Walker (1979, 12–32).

[28] Walker (1979, 44–49).

upset bridge plans in the past. J.T. Desaguliers, the curator of experiments for the Royal Society, addressed the committee on the "proper Instruments for boring the Soil under the River Thames."[29]

The Westminster Bridge Bill of 1736 set up a commission, with about 175 members, a good proportion of whom were members of Parliament. They also included such an obviously useful person as the director of the Bank of England as well as dukes, bishops, and admirals, who were useful in other, more or less obvious ways. The first meeting was held in June 1736, at which time the commissioners viewed models of the bridge that had been exhibited in the House of Commons, and they set up a lottery with the Bank of England to finance the construction.[30]

A good many of the commissioners were fellows of the Royal Society, Charles Cavendish one of them, and the Royal Society was kept informed on the project. Thomas Innys exhibited before them a model of a machine he invented for laying the foundation of the piers of the bridge. To decide on technical matters of this sort, in June 1737 the bridge commissioners formed a committee of thirteen, the so-called committee of works. Cavendish was appointed to it, as were several other fellows of the Royal Society, though William Kent, a well-known architect, was perhaps the only member of the committee with obvious qualifications.[31] Now both a commissioner and a committeeman for the bridge, Cavendish took his duties with his usual seriousness.

Although at the beginning, the committee of works resolved to consider only wooden bridges for reasons of cost,[32] nevertheless it and the commissioners heard the stone-bridge advocate Charles Labelye, whose method of laying the foundations of the piers worked for either a stone or a timber superstructure. Labelye's credentials differed from those of his competitors, the best-known of whom came from a background in architecture and seem to have had no engineering experience. Not an architect, he was evidently experienced in surveying and construction, for the House of Commons treated him as an expert "engineer," calling on him to testify on the bridge before its own petition committee. Like Desaguliers, who claimed him as his "disciple" and "assistant,"[33] Labelye was of Huguenot origins. Educated in Geneva, he settled in England, where he became involved in such projects as draining the fens and improving harbors.[34] In due course, the "foreigner" Labelye was hired by the commissioners to build stone foundations for a bridge that still could be made of wood or stone.[35] Eventually the commissioners decided that a bridge made partly of wood was unequal to the dignity of Westminster and London and ordered it to be built entirely of stone.

Labelye was not a fellow of the Royal Society, but he was friends with a good number of men who were. In the middle of building the bridge, he sent the president of the Royal Society Folkes a calculation about the card game whist.[36] The prospect of a gambling bridge-

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[29] 16 Feb. 1735/36, Great Britain, Parliament, *House of Commons Journals* 22:569. Hereafter *HCJ*.

[30] Walker (1979, 63–67).

[31] Besides Cavendish, three other members of the committee had been fellows of the Royal Society since the 1720s: the chairman of the committee, Joseph Danvers, M.P., a lawyer by training and now a landowner; David Papillon, M.P., practicing lawyer; Thomas Viscount Gage, M.P., from 1743 master of the household to the prince of Wales. Walker (1979, 79, 86 n.7.)

[32] 5 Aug. 1737, Minutes of the Committee of Works, vol. 1: Aug. 1737–Sept. 1744, Public Record Office, Kew, Work 6/39.

[33] 16 Feb. 1735/36, *HCJ* 22:569. J.T. Desaguliers (1744, 2:506).

[34] Walker (1979, 83–86).

[35] Walker (1979, 82).

[36] Charles Labelye to Martin Folkes, 22 Mar. 1741/42, Folkes Correspondence, Royal Society.

builder could be unnerving, but Labelye was only carrying out an exercise in De Moivre's subject, the doctrine of chances. Labelye was a good enough mathematician for Desaguliers to publish his investigation of the vis viva controversy in mechanics.[37]

At a meeting of the commissioners in August 1738, Cavendish heard a report about a violent opposition to the bridge. Angered by the threat of losing their trade to the bridge, watermen ran their barges into the boats moored beside the pile-driving engine. After the commissioners decided to advertise that part of the bridge act that legislated the death penalty for anyone found guilty of sabotaging the bridge works, the engine was tried without incident. The designer of the engine brought a model to a meeting of the Royal Society, and Desaguliers published a description and drawing of it in his *Course of Experimental Philosophy.* When in January 1739, the foundation for the first pier was finished, the earl of Pembroke laid the first stone "with great Formality, Guns firing, Flags displaying."[38]

Technical problems dogged construction all the way, the most damaging of which was the gradual sinking of the bridge. It was supposed to bear 1200 tons, but when it was loaded with 250 tons of cannon as a test, it began to fail. "Westminster-Bridge continues in a most declining Way," Thomas Birch wrote to Philip Yorke. People stayed up late to be able to say "What kind of a Night the Bridge has had." The formerly unhappy watermen burst into cheers as they watched the bridge settle as much as four inches in a night.[39] Possibly it was sabotaged, but whatever the cause the subsiding pier had to be rebuilt, requiring extra years. The wait was worth it. Spanning 1200 feet, built of Portland and Purbeck stone, Westminster Bridge was a monument to engineering and architectural grace (Figs. 5.5–5.6).[40]

The first Westminster Bridge lasted only about a century, a brief life compared with the six hundred years of London Bridge, but that was not owing to faulty construction. Once Westminster Bridge was built, the rickety condition of London Bridge gave rise to alarm. On Labelye's advice, some of its piers were removed, but the piers had acted as a dam, and when they were removed the tide eroded the riverbed and ground away at the piers of Westminster Bridge. Labelye's beautiful bridge had to be replaced.[41]

Halfway into the construction, Labelye wrote that the bridge commissioners "have nothing, and can expect nothing, but Trouble for their Pains," and that he admired their selfless "publick Spirit" and "Patience."[42] Labelye was right about Cavendish, who devoted a large effort to the bridge while at the same time carrying out his parliamentary duties. In 1739, in the third year of the bridge, for example, Cavendish served on twenty-four committees of Parliament, and he also went to nineteen meetings of the Westminster Bridge commissioners. In the middle years of the construction, he rarely missed a meeting of the commissioners or of the works committee. In addition he came fairly regularly to a third kind of meeting, that of a small commitee of accounts for the bridge, often chairing the meeting.[43] In 1744, he attended twenty-five out of twenty-six meetings of the commissioners and eighteen out of nineteen meetings of the works committee. He was involved in much of the quiet work in the building of Westminster Bridge, exhibiting the combination of political,

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[37] Charles Labelye to J.T. Desaguliers, 15 Apr. 1735, published in Desaguliers (1744, 2:77, 89–91).

[38] Walker (1979, 91–95). Desaguliers (1744, 2:417–418).

[39] Thomas Birch to Philip Yorke, 12, 19 Sep. 1747, 11, 18, June 1748, BL Add Mss 35397, ff. 72–76, 114–116.

[40] Summerson (1978, 113–116).

[41] Samuel Smiles (1874, 70–71, 140–142).

[42] Charles Labelye (1743, 24–25).

[43] Minutes of the Committee of Accounts, vol. 1:1738–1744, Public Record Office, Kew, Work 6/41.

administrative, technical, and accounting skills he brought to his organizational work for the Royal Society.

Scientific Administration

We begin this discussion by recalling some basic facts about the running of the Royal Society. By a royal charter of 1663, the Society was constituted a self-governing corporation. Every St. Andrew's Day, November 30, the members elected a Council of twenty-one and a number of officers: president, treasurer, and two secretaries. The president chose one or more vice presidents to sit in for him when he was absent. To ensure that the Council did not become fixed and at the same time to ensure a measure of continuity, ten of its members were newly elected each year while eleven were kept on from the old Council. The government of the Society was invested in the Council and president, who were assisted by a person responsible for foreign correspondence and translations of foreign papers. The election of officers was by simple majority.[44]

After being a member for eight years, Cavendish was elected to its Council for the first time in November 1735. He was elected again in November 1741, and for the next twenty-one years he was on the Council every year with the exception of 1753, when family business called him away. He served four more nonconsecutive terms on the Council, his last in 1769, when he served together with his son Henry. Henry would have an even longer record of service; combined, their membership on the Council would span seventy-three years, with some interruptions. For many years, Charles was also a vice president.

The Royal Society was now in its third home, a quiet, central location in Crane Court (Fig. 5.1). The front of the house faced a garden, the back a long, narrow court. Up one flight of stairs and fronting the garden was the small room where the Society as a whole met weekly, except during Christmas and Easter and the long recess in late summer, about thirty times a year in all. How often the Council met depended on how busy the Society was and on the energy of the current officers. Ordinarily it met six or fewer times a year toward the end of Folkes's presidency in the late 1740s, and eight to ten times under Macclesfield's in the 1750s, but it met twenty-two times in 1760 during preparations for observing the transit of Venus the following year. Presidents before Newton rarely came to Council, but Newton came all the time, changing the day of the meetings of the Council to accommodate his schedule. His precedent was followed, with decreasing rigor, by his successors: Sloane missed only eight out of 105 Council meetings in his fifteen years as president; his successor, Folkes, missed one quarter of his; and Folkes's successor, Macclesfield, missed about one third of his. Cavendish's first term on the Council was under Sloane's presidency, and he missed a good many meetings, perhaps because he found that the Council conflicted with his political duties. His attendance picked up in the year he returned to the Council, which was the year he stepped down from Parliament; for the next six years he came to two out of three meetings, and after that he was almost never to miss a meeting. Frequently only a half dozen members attended, a meager number considering that it included the two secretaries and usually the president, and ten or so constituted a fair turnout. To give an idea of his steadfastness, in the five years from January 1748 through November 1752, he attended all twenty-seven meetings, and in the eight-years from December 1753 through November

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[44] 20 Aug. 1730, Minutes of Council, Royal Society 3:50–61.

1761, out of eighty-seven meetings, he attended seventy-eight. Only two fellows came oftener, the secretaries of the Society, who had no choice short of neglecting their duties, Peter Davall from 1747 and Thomas Birch from 1752. One other councilor came regularly over a long period, the barrister James Burrow, who like Charles Cavendish sometimes acted as temporary president of the Society during a vacancy.[45]

The minutes of the Council listed Lord Charles Cavendish first after the president, except when Lord Macclesfield (before he was president) was there, and later Lord Morton; this protocol ceased after 1760 when councilors were listed alphabetically. At this time about one seventh of the membership of the Royal Society was aristocratic,[46] a proportion which was increasing.[47] As an aristocrat who supported science, Cavendish was not unusual. What set him apart from most was his solicitous attention to the affairs of the Society.

Meetings of the Council typically dealt with money: payment of bills from printers, bookbinders, solicitors, and instrument makers; payment of debts; payment of insurance on the houses owned by the Society; and payment of salaries. Besides handling these matters routinely as they came up in Council, Cavendish usually went over them all again, since nearly every year he was appointed to a committee of auditors of the treasurer's account. Cavendish was an all-purpose, responsible, and accurate servant of the Society, as his son Henry would be after him.

Recently the *Philosophical Transactions* had been criticized for publishing thin material. The critic John Hill, a writer on natural history and on various subjects outside of science, stepped up his criticisms after having failed in his bid to become a member of the Royal Society. Singling out for ridicule papers on natural history appearing in the journal, he proposed that the Society form a committee to decide on papers to be read or published. There were influential members of the Society such as William Watson who agreed with Hill that the standard of papers could be improved.[48] Early in 1752 Macclesfield asked the Council to consider the way papers were chosen for publication in the *Philosophical Transactions*. One of the secretaries had run the journal, making decisions on his own though probably taking into consideration requests by individual members. At this time the secretary was the physician Cromwell Mortimer, under whose oversight the journal emphasized antiquarian interests.[49] For the "credit and honour of this society," Macclesfield said, from now on, decisions about publication would be made by a committee. The president, the vice presidents, and the two secretaries were to be included in the committee, and no decisions on papers could be made without a quorum of five. For advice on particular papers, authorities from outside the committee could be brought in by a request of a majority of the committee. At meetings of the committee, at the request of a member, a paper would be read in full without "debate or altercation." Then a vote would be taken by ballot, so as to "leave every member more at liberty to fully declare his opinion." Since the decision to publish a paper was a recognition not every author received, the new committee had a sensitive assignment. Macclesfield (correcting himself) said that the Society in the past had not "usually meddled" in the selection of papers to be published. That it had meddled at various

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[45] Information from the Royal Society, Minutes of Council.

[46] Bound with the minutes of the committee of papers is a printed membership list for the Royal Society in 1749. The total British membership then was around 340, and of these around 45 were aristocrats, counting bishops and persons like Cavendish with the courtesy title "Lord."

[47] Richard Sorrenson (1996, 36).

[48] Kevin J. Fraser (1994, 44, 48–51). John Hill (1751).

[49] Charles Bazerman (1988, 137).

times in various ways he conceded; what was going to change was that it would meddle in a systematic and accountable way. Cavendish joined Macclesfield in proposing amendments, and on 26 March 1752 the new statutes were passed by the Council.[50] With Cavendish in the chair, Philip Yorke proposed that for the time being the Council would be the "committee of papers."[51] Readers of the journal were informed that the *Philosophical Transactions* was now for the "sole use and benefit of the society, and the Fellows thereof."[52] In the middle of the eighteenth century, in a variety of ways the Royal Society rationalized its procedures,[53] and the papers committee could be seen as an example.

Although a committee would decide on which papers were to be published, a secretary continued to screen papers presented to the Society. The role of a secretary in controlling access to the Society can be seen in the exchange of letters between Thomas Birch and Samuel Bamfield, who had written a paper on a theory of astronomy that disagreed with Newton's. Bamfield wanted to have it read to the Society; Birch refused. Bamfield suggested that another member might see the truth of his theory; Birch recommended that he read a standard book on Newtonian astronomy. Bamfield then tried to dedicate his work to Macclesfield and have Macclesfield look at it; Birch denied him.[54]

In April 1752, the committee of papers convened for the first time, Cavendish presiding. Macclesfield came to the first three meetings, but then dropped out, returning at the end of the year when he became the new president of the Royal Society. Cavendish chaired all of the meetings but one through November 1752. In 1753 Cavendish was not on the Council and the committee. When he returned to the Council in 1754, he attended every meeting of the committee, and this remained his habit in the years following; after him, Burrow came most often, Watson and Bradley came occasionally, and other members came and went. The committee met four to six times a year, usually attended by about four members in addition to the two secretaries, who were required to be there, and the president, when he came. Cavendish's attention to this important responsibility of the Society set a precedent for his son Henry, who would be a steady presence on this committee in his time.[55]

The work of the committee of papers was demanding. In the years before 1740, the number of papers reached a peak of well over 100 per annum on the average. After that, the number fell off, but slowly, and the load remained considerable through Cavendish's years on the committee. At the time the committee was formed, there was a backlog of papers, which the committee went through chronologically, beginning with January 1751, taking several meetings to get through that year: at its first meeting, the committee approved sixteen papers for publication, at its second meeting fifteen, and at its third twenty-four. Daniel Wray, who began coming at the second meeting, wrote to Philip Yorke of their "diligence, as members

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[50] 20 Feb., 19 and 26 Mar. 1752, Minutes of Council, Royal Society 4:55, 64, 71–75, 83.

[51] 27 Feb. 1752, ibid. 4:64–65.

[52] 19 Mar. 1752, ibid. 4:76.

[53] Measures were taken to eliminate unnecessary duplication of records, and to make progress in "methodizing" the orders of the Council "relative to the offices of Clerk, Librarian, Keeper of the Repository, Housekeeper, Macebearer and Porter." "Proposal Concerning the Papers of the Royal Society," presumably by Macclesfield, BL Add Mss 4441. It was found that papers presented before the Society ended up in two kinds of books, while only one, the minutes of ordinary meetings, was needed. 12 July 1742, Minutes of Council 3:285; 1 Feb. 1763, ibid. 5:1.

[54] Letters between Samuel Bamfield and Thomas Birch c. 1761–64, BL Add Mss 4300.

[55] Rough notes of the meetings of the committee of papers taken by Thomas Birch, one of the secretaries, in "Minutes of the Royal Society," vols. 1 and 2, Birch Collection, BL Add Mss 4445–46.

of the Committee of Papers."[56] Over time, the number of papers was an inadequate measure of the committee's work, since papers became longer.[57]

To evaluate critically every paper that came before the Royal Society was a good way to keep abreast of what went on in science, though we think that Cavendish's primary motivation was service to the Society. Procedures applying to the *Philosophical Transactions* were considered important, since its contents were the public record of the Society, on which its external authority largely rested. Decisions arrived at by men who were active in science and in the Society were likely to be competent and fair. Cavendish helped get the committee off to a conscientious start in its first year.

Cavendish was also active in the administration of the Royal Observatory. In 1765, by warrant from the king, the president together with other fellows of the Royal Society was charged with making tours of inspection of the instruments of the Observatory. Cavendish was one of several fellows who regularly made these tours, or "visitations," to Greenwich to determine what repairs were needed and to estimate the expense. In 1781, two years before his death, Charles Cavendish was still discharging the Royal Society's obligations, reminding the president that the publication of the Greenwich observations was long overdue.[58] In this capacity again, his son Henry would follow his precedent.

As in the British Museum, Cavendish's interest in books and manuscripts together with his accounting skills was put to use in the Royal Society, where he served as one of the inspectors of the library. The clerk of the Society said that "at present the books weigh less than the filth that covers them" a measure of the neglect of the library at the time. Cavendish and his fellow inspectors delivered a damning report on it: the catalog of the the great Norfolk collection of books and manuscripts is faulty in titles and dates, "there is a deficiency of several whole centuries of numbers" in the catalog, numbers on books do not agree with numbers in the catalog, "different volumes of the same work stand on different shelves, and have very different numbers," "different books have the same number," "many of the books are so ill arranged, as to the sizes of them, that they cannot be placed upright on the shelves," many have spoiled bindings or broken wooden covers, and many more are "very much worm-eaten." As for the rest of the books in the library, their cataloging had stopped over twenty-five years before, whereas since that time nearly 1000 books and pamphlets had been donated to the library, the record of which was found in the journals of the Society. The problems were so severe that the inspectors recommended making an entirely new catalog for the Norfolk collection, updating the catalog of the rest by going through the journals, altering the shelves or rearranging the books, and rebinding those books that were not so far deteriorated as to be beyond repair. Owing to the inspections, some of the defects were corrected. The library was worth the attention and the expense. In size

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[56] Daniel Wray to Philip Yorke, 5 July 1752, Hardwicke Papers, BL Add Mss 35401, f. 157.

[57] Raymond Phineas Stearns (1970, 97–98). Bazerman (1988, 81).

[58] Upon the death of the astronomer royal James Bradley in 1762, his executors removed his observation books from the Royal Observatory, claiming them as private property. In 1763, Maskelyne addressed the Royal Society on the subject of their recovery. To reimpose its authority, the Royal Society requested a new warrant from the king, which he granted in 1765, appointing the president and Council of the Society to be visitors of the Royal Observatory. "Visitations of Greenwich Observatory, 1763 to 1815," Royal Society, Ms. 600, XIV.d.11, ff. 6 passim. Cavendish to Banks, 19 May 1781.

it compared with an excellent private library, around 10,000 volumes, roughly the size of Henry Cavendish's private library later in the century.[59]

Elected during Sloane's presidency, Cavendish served through Folke's, Macclesfield's, and Morton's. In 1768, while the Council was absorbed in preparations for a second transit of Venus the following year, Morton died. Ten days latger, Daniel Wray wrote to Philip Yorke that "*Lord Charles* is deaf to all our prayers; and will not *preside* over us."[60] Cavendish was in his early sixties, in good health, and on the Council, but he did not want to be president; his feelings on the subject were the same as when Folkes had stepped down nearly fifteen years earlier.

Science

We begin with Charles Cavendish's earliest recorded scientific observations, which took place soon after his election to the Royal Society. In June 1728 at James Bradley's observatory at Wansted, Cavendish made observations at using a zenith telescope for detecting the parallax of the fixed stars (Fig. 5.7).[61] The instrument had been in place for less than a year, and after Bradley and Halley, Cavendish was the next person to observe with it. Later that year, in the course of looking for parallax, Bradley discovered the aberration of light from the stars, which greatly improved the accuracy of observational astronomy.

With his new instrument Bradley observed small motions of stars passing nearly through the zenith, motions which he knew were too large and in the wrong direction to be caused by the parallax of the fixed stars. His explanation was that the motion of the zenith stars was the resultant of two motions, the orbital motion of the Earth and the motion of light. In his announcement of Bradley's discovery of the aberration of light to the Royal Society, Halley observed that the "three Grand Doctrines in Modern Astronomy do receive a Great Light and Confirmation from this one Single Motion of the Stars Vizt. The Motion of the Earth, The Motion of Light and the immense distance of the Stars."[62] Bradley had, in fact, provided the first direct evidence of the Copernican theory, and the twenty-four-year-old Charles Cavendish had had a brush with this grand work of observation and reasoning in astronomy.

We assume that Cavendish learned about instruments from Bradley. Cavendish was able to return the favor several years later after Bradley had moved from Wansted to Oxford, a few miles from Macclesfield's Shirburne Castle, where Bradley regularly made observations. When Bradley became a candidate to succeed Halley as astronomer royal, Macclesfield exerted his influence, but because his voting had put him out of favor at court, he had

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[59] Andrew Coltee Ducarel to Thomas Birch, 13 Oct. 1763, Birch Correspondence, BL Add Mss 4305, 4:57. "I compute about 1000 vol to whit the Norfork 500 MSS & 3000 printed. The Society Library about 6000 printed books only." Emanuel Mendes da Costa to William Borlase, 9 July 1763, E. da Costa Correspondence, BL Add Mss 28535, 2:150. Reports of the inspectors of the libraries of the Royal Society, 6 June 1768, 6 April 1769, and 25 July 1770, Minutes of Council, Royal Society 5:308, 6:25–26, 62–65.

[60] Daniel Wray to Lord Hardwicke, 22 Oct. 1768, in George Hardinge (1815, 137). Next month, James West presided over them.

[61] S.P. Rigaud (1832, 237).

[62] 14 Nov. 1728, JB, Royal Society 13:260–262, on 261–262. Together with Samuel Molyneux, Bradley looked for the parallax of the star Gamma Draconis, which would appear as a small annual cyclical motion of the apparent position of the star. They observed a small annual cyclical motion, but not the one they expected, for which they had no explanation. After Molyneux died in 1728, Bradley found the explanation in the "aberration of light."

to proceed indirectly; to build scientific support for Bradley, he wrote to William Jones to ask him to enlist Folkes and Charles Cavendish.[63]

We learn of Cavendish's next recorded observations from a passing remark by his friend William Watson: in the severe cold of 1739, the thermometer in Cavendish's room sank to twenty-five degrees; Cavendish, Watson said, then placed his thermometer outside the window and some distance from it, observing a low one night of thirteen degrees.[64] It is possible that in 1739 Cavendish had a self-registering thermometer for low temperatures, though he did not make public such an instrument until nearly twenty years later.

Figure 5.7: James Bradley. Painting by Thomas Hudson, around 1742–47. Wikimedia Commons.

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[63] Lord Macclesfield to William Jones, 13 Jan. 1741/42; Lord Macclesfield to Lord Hardwicke, 13 Jan. 1741/42, in Rigaud (1832, xlvi).

[64] William Watson (1767, 444).

Unlike his own work, which he kept to himself or communicated privately or, at most, allowed a colleague to mention publicly, Cavendish's work for the Royal Society was public. His first scientific assignment concerned longitude at sea. The Greenwich Observatory was founded in 1675 to perfect astronomical tables for finding longitude, but the tables did not work for ships. To secure their safety and to promote trade, in 1714 Parliament passed an act that provided rewards for improvements in taking longitude at sea proportional to their accuracy, the ultimate award, £20,000, to be paid to the discoverer of a method that on a six-week journey to the West Indies gave the longitude upon arrival within an accuracy of thirty miles. To evaluate proposals, the Board of Longitude was established, a body of twenty-two members, who were quickly inundated with proposals; before a parliamentary committee, Newton, a member of the Board, rejected them all. A well-known alternative to the lunar method of finding longitude at sea was a seaworthy and accurate clock. John Harrison, at first with his brother James, built a series of clocks, the first one proving capable of overcoming variations of heat, moisture, friction, and fluidity of oil so perfectly that its error was less than one second a month for ten years running, only this wonderfully accurate machine was a delicate pendulum unsuited for taking to sea. The second clock was practical, keeping good time while undergoing violent motions simulating storms at sea. The Board of Longitude rewarded Harrison with modest sums of money, and in 1741 Cavendish was one of committee of twelve fellows of the Royal Society called in as a source of expert opinion, who recommended that Harrison continue to be encouraged.[65] In 1763, on the eve of a second trial run of Harrison's latest clock, Cavendish was appointed to another committee on the project. From what had become a life work and prolonged legal battle, and with the support of Cavendish and other fellows of the Royal Society, in the end Harrison received most of the money he deserved, and in addition he was awarded a Copley Medal. British ships in return received a reliable instrument for determining longitude; Captain Cook used Harrison's clock on his voyage to the South Seas in 1772, justifying the claims of precision made for it.[66]

In 1742, Cavendish accepted another assignment having to do with accuracy of measurement. The project was to compare the Royal Society's weights and measures with those kept by the Academy of Sciences in Paris and also with other standards in England. Measurements were decisive in some experimental work, and depending upon the country in which they were made, they were expressed in the English foot or the French toise, lengths marked off on metal standards and deposited in various archives. The project was expanded to include a comparison of the Royal Society's standards with other standards in England. The instrument-maker George Graham carried out the necessary experiments in the presence of a delegation of witnesses from the Royal Society, who other than being fewer were

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[65] The persons Cavendish came together with on the committee were known for their accuracy: mathematicians De Moivre and his circle, Folkes, Jones, and Macclesfield; astronomers Bradley and Halley (and Macclesfield); instrument makers John Hadley and George Graham; the versatile James Jurin; and Cambridge professors of natural philosophy and mathematics Robert Smith and John Colson.

[66] The act of 1763 altered the original act of 1714. The other members of the new committee were Lord Morton, Lord Willoughby, George Lewis Scott, James Short, John Michell, Alexander Cumming, Thomas Mudge, William Frodsham, and James Green. Only the instrument maker Short and the watchmakers Frodsham and Green were satisfied with Harrison's explanation of his clock. Cavendish was appointed by the Board of Longitude to another committee; John Bird deputized for him this time. E.G.R. Taylor (1966, 126, 170, 172). "Some Account of Mr. Harrison's Invention for Determining the Longitude at Sea, and for Correcting the Charts of the Coasts. Delivered to the Commissioners of the Longitude, January 16th, 1741–2"; in John Harrison (1763, 7–8, 19, 21). Humphry Quill (1966, 5–6, 120–122, 139–146, 186, 221).

almost the same as the committee that had investigated Harrison's clock. In this company, Cavendish was in his element, accuracy.[67]

In 1747 William Watson invited members of the Royal Society to join him in an experiment on electrical conduction, the scale of which, miles literally, was a measure of his enthusiasm for the subject. The experiment was made possible by the recent discovery of the Leiden jar, the "explosion" of which could communicate shocks over considerable distances. Watson thought that a powerful Leiden jar might send a shock clear across the River Thames, and to test the idea Watson with "many others" assembled at the new Westminster Bridge (to which Cavendish had recently devoted so much work) across which they laid a wire connected to a Leiden jar, the river and the bodies of the experimenters completing an electrical circuit. Upon discharging the Leiden jar, Watson and his associates felt shocks in their wrists and elbows, confirming his hypothesis. The circuit was progressively lengthened until finally the experimenters moved from the river onto dry land, at Shooters' Hill, where using signals and watches they concluded that electrical conduction is "nearly instantaneous." In the experiments, which lasted for weeks, twenty-five fellows of the Royal Society took part, including Cavendish and other members of the De Moivre circle, Folkes, Stanhope, Davall, Jones, and Scott. Bradley was there, and so were many of the leading instrument makers. For this "Body of Philosophers," the outdoor experiments in the middle of summer were an outing as well as an inquiry into nature, Stanhope supplying venison pastry and French wine.[68] The experiments were financed by and "made by the order and for the service of the [Royal] Society."[69] Watson published an account of them in the *Philosophical Transactions*.[70]

More important was Cavendish's assistance to Watson in his private researches on electricity. To discover if the vacuum transmits electricity, Watson relied on the imperfect vacuum achieved by an air pump until Cavendish solved the problem with an ingenious and very simple apparatus, which achieved a Torricellian vacuum and an electrical circuit at once. Bending a narrow glass tube seven and a half feet long into a parabolic shape, Cavendish filled it with mercury and placed its ends in basins of mercury; the mercury in the two arms of the parabola descended until the level stood about thirty inches above the basins, leaving a vacuum at the top of the parabola. By bringing up a wire from an electrical machine, Cavendish caused electricity to pass through the vacuum in a "continued arch of lambent flame." "This noble Lord," Watson said in appreciation, joined a "very complete knowledge" of science with that of making apparatus; his "zeal for the promotion of true philosophy is exceeded by none."[71]

"It were to be wished, that this noble philosopher would communicate more of his experiments to the world, as he makes many, and with great accuracy," Benjamin Franklin

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[67] "An Account of the Proportions of the English and French Measures and Weights, from the Standards of the Same, Kept at the Royal Society," *PT* 42 (1742, 185–88). "An Account of the Comparison Lately Made by Some Gentlemen of the Royal Society, of the Standard of a Yard, and the Several Weights Lately Made for Their Use; with the Original Standards of Measures and Weights in the Exchequer, and Others Kept for Public Use, at Guild-Hall, the Tower, &c.," *PT* 42:541–556. H. Hall and F.J. Nicholas (1929, 40). Of the seven witnesses, five we have met in connection with De Moivre: Folkes, who was then president, Macclesfield, Jones, Peter Davall, and Cavendish. The other two were the instrument-maker Hadley and the secretary Cromwell Mortimer.

[68] Thomas Birch to Philip Yorke, 15 Aug. 1747, BL Add Mss 35397, ff. 70–71.

[69] 17 Oct. 1748, Minutes of Council, Royal Society 4:15.

[70] William Watson (1748a).

[71] William Watson (1752c, 370–371).

wrote in 1762, expressing his admiration for an experiment Cavendish made on the conduction of electricity by heated glass.[72] The study of electrical conduction had been advanced by the discovery of the Leiden jar, which delivered far greater quantities of electricity than did the unaided electrical machine. The Leiden jar was able to do this because the glass of the jar did not conduct electricity. By his experiment, Cavendish showed that when glass is heated to four hundred degrees or higher, it becomes a conductor of electricity.

From the summer of 1760 to early 1763, the Council of the Society was almost exclusively occupied with observations of the transit of Venus in 1761, energized by the complexity of this project. In anticipation of the transit, Halley had recommended observing it as a means of measuring the distance of the Earth from the Sun, the standard by which the distances of other bodies of the solar system were measured. To obtain the necessary observations of Venus crossing the solar disk, the Royal Society sent Nevil Maskelyne and Robert Waddington to St. Helena, and Charles Mason and Jeremia to Bencoolen, though they were forced to stop at the Cape of Good Hope. Sixty-two observing stations in a number of countries participated in this project of unprecedented size, and the Royal Society was to receive their reports of the transit and to publish them in its *Philosophical Transactions*.[73] Cavendish was involved in the scientific work at various levels, from the examination of a faulty instrument to the writing of a synopsis of the completed observations of the transit.[74] Soon after the transit of Venus, two of its observers Charles Mason and Jeremiah Dixon were commissioned by the Royal Society to measure a degree of latitude between Maryland and Pennsylvania, and Cavendish played a part in this too.[75] In general, there was little of scientific significance done officially at the Royal Society in the middle of the eighteenth century in which Cavendish was not involved.

The best-documented example of Charles Cavendish's scientific work at the Royal Society is his repetitions of experiments on the compressibility of water made by John Canton, a London schoolmaster. Canton's apparatus was simple, a glass tube with a very small bore two feet long, open at one end and closed at the other by a hollow glass ball an inch and a quarter across. In a preliminary experiment, the ball and a few inches of the tube were filled with mercury and placed in a water bath, which was heated until the mercury rose to the top of the tube, at which time the tube was hermetically sealed. When the mercury had cooled to its original temperature, it stood 32/100th of an inch higher than it had originally, before the mercury had been heated and the tube sealed. The only difference before and after the expansion of the mercury was that the pressure of the atmosphere over it had been removed. Canton found the same when water was used in place of mercury, only the water rose a little higher than the mercury, 43/100th of an inch. The only difference before and after the expansion of the water again was that the pressure of the atmosphere over it had been removed. In a paper in the *Philosophical Transactions* in 1762, Canton concluded that water is compressible. Two years later he published a sequel in which he extended his experiments to other liquids.[76]

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[72] Benjamin Franklin to Ebenezer Kinnersley, 20 Feb. 1762, ed. L.W. Larabee (1966, 10:42).

[73] Weld (1848, 2:11–19). A. Pannekoek (1961, 284–287). J.D. North (1995, 352–354).

[74] 27 May 1762, Minutes of Council, Royal Society 4:333–34. Thomas Birch to Philip Yorke, 6 Sep. 1760, 20 June 1761, BL Add Mss 35399, ff. 153, 207.

[75] 25 June 1761, 25 Oct. 1764, Minutes of Council, Royal Society 4:45.

[76] John Canton (1762; 1764). John Canton to Benjamin Franklin, 29 June 1764, in ed. L.W. Larabee (1967, 11:245).

Doubts were raised about Canton's experiments in the *Monthly Review*, which although it was not a scientific journal nevertheless reviewed critically the contents of the *Philosophical Transactions*. When the Royal Society decided to honor Canton with his second Copley Medal—his first was for experiments on magnetism—for his proof of the compressibility of water, the journal hinted that it was to the Society's dishonor.[77] The new president of the Royal Society, Lord Morton, asked the secretary Thomas Birch if it was "necessary every year to give the Medal," and he also asked for an account of the "Experiment, by the [Florentine] Accademia del Cimento which pretends to establish the opinion that water is incompressible."[78] Because in conversation, some fellows made objections, in concern for the "honour of the Society" the Council appointed a committee to repeat Canton's experiments at the Society's expense and to report back to the Council.[79] Any objections to Canton's experiments had to be submitted in writing if they were to be considered by the committee. In June 1765 the Council ordered instruments for the committee, who were assisted in its experiments by several instrument-makers.[80] The Society was in recess for the summer, and some of the committee members were out of town. Those who remained—Cavendish, Franklin, Watson, Heberden, and Ellicott—met four times in July to perform experiments in the Museum of the Society. At the beginning of August, the clerk of the Society informed the president that the attending members of the committee were convinced of Canton's conclusion, but since they were "all friends to the experiments," he anticipated a "contest," especially since the experiments were of such "nicety." In November, after the Society had resumed its meetings, certain experiments were performed a second time before a larger committee.

The larger committee contained a principal skeptic of Canton's claims. Francis Blake, an Oxford mathematician who was active in the Society, raised various questions about Canton's experiments, but his main concern was what seemed to be a violation of common sense: in the Florentine experiment, water was subjected to great pressure without, evidently, causing any change in its bulk, whereas in Canton's experiment, an observable change was alleged to have resulted from a very slight pressure. Which account was Blake to credit? As requested, he put his questions to the Council in writing.[81]

In a paper drawn up for the Council, Cavendish stated and answered the objections to Canton's experiments.[82] The first objection went to the heart of the matter, the conflict with the Florentine experiment: experiment is authority, Cavendish said, and experiment can overrule experiment. In response to Blake's objections, Cavendish wrote a separate paper, which he began by making the same point: "The authority of the most able experimenters is of no weight, when it appears that their experiments were made in such a way, as could not

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[77] *The Monthly Review* 29 (1763): 142–144, and 33 (1765): 455–456, on 456.

[78] Lord Morton to Thomas Birch, 6 and 17 Nov. 1764, BL Add Mss 4315, ff. 13, 16.

[79] Besides Cavendish, the committee consisted of the president Lord Morton, Matthew Raper, John Ellicott, James Short, William Watson, Israel Mauduit, and Charles Morton. 28 Nov. 1764, Minutes of Council, Royal Society 5:57. Francis Blake, Edward Delaval, Benjamin Franklin, and George Lewis Scott were added to the committee: 21 Feb., 17 June 1765, ibid. 5:62–63, 109.

[80] They were John Bird, James Ferguson, and Edward Nairne. John Bird is referred to in Cavendish's memoranda on the experiments. James Ferguson was paid for his work: 10 July 1766, Royal Society, Minutes of Council 5:161. Edward Nairne was also appointed according to Lord Morton: 30 Nov. 1765, JB, Royal Society 25:655.

[81] Francis Blake, "Remarks and Queries Recommended to the Consideration of the Right Honourable the Earl of Morton," Canton Papers, Royal Society, 3.

[82] Canton Papers, Royal Society, 3. These objections are contained also in a much longer (11-page) paper, which would also seem to have been written by Cavendish, though the copy in the Canton Papers is not in his handwriting.

possibly show so small a degree of compressibility as Mr. Canton has discovered."[83] There had been progress in the art of experiment in the century since the Florentine experiments, Cavendish said, evidence of which was Canton's skillful demonstration of "so small a degree of compressibility."

We are indebted to the Canton controversy for the only surviving direct record of Cavendish's experimental work, preserved in Canton's papers at the Royal Society. Cavendish sent his measurements and computations to Canton to review, having annotated them throughout with "by my measure" and signing the bottom of every sheet. This example of his practice shows thoroughness and attention to accuracy, characteristics equally of his son Henry's work, of which we have ampler record.

In November 1765, the Council resolved that the hypothesis of the compressibility of water accounts for Canton's experiments and that no other appears to do so as satisfactorily, on which basis it voted to award Canton the Copley Medal for 1764.[84] Two days later, at the anniversary meeting of the Society when the award was announced, the president Morton referred to the work on Canton's experiments by that "Noble Member of the Society," Lord Charles Cavendish, who was "eminent for his great Abilities, and deep knowledge in all the branches of science that come before him."[85] He did not describe the ensuing experiments carried out at the Society, since Cavendish had written a "full and accurate Account" of them and of the "Theory deducible from them."[86] Cavendish's paper was read at the next general meeting of the Society.[87]

Cavendish described several self-registering thermometers he had contrived in his one publication in the *Philosophical Transactions*, in 1757, by which time he had been active in science for thirty years. The idea of maximum and minimum thermometers goes back to the end of the seventeenth century, but Cavendish's were the first maximum and minimum liquid thermometers.[88] Fig. 5.8 shows them: two maximum thermometers, one using mercury and the other alcohol, and one minimum thermometer. Macclesfield, who was then president of the Royal Society, proposed Cavendish as the Copley Medalist for that year, a choice which the Council unanimously approved. In his address to the Society on the occasion, Macclesfield brought together the Copley Medalist's scientific and social eminences: Lord Charles Cavendish was as conspicuous for "his earnest desire to promote natural Knowledge, and his Skill and abilities together with his continual Study and endeavor to accomplish … his desire" as he was for his "high Birth and eminent Station in life." The Medal was a small part of the recognition that was due him, Macclesfield said; because of his "excess of Modesty," the public had been deprived "of many important discoveries as well as considerable improvements made and contrived by his Lordship, in Several Instruments and Machines necessary for trying Experiments and deducing proper consequences from the Same; and

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[83] Charles Cavendish "Observations on Mr. Blake's Objections to Mr Canton's Experiments," Canton Papers, Royal Society.

[84] 21 and 28 Nov. 1765, Minutes of Council, Royal Society 5:131–132.

[85] 30 Nov. 1765, JB, Royal Society 25:656.

[86] Morton's address, 30 Nov. 1765, JB, Royal Society 25:647–664, on 656. The award of the Copley Medal did not bring the work of the committee to an end; two and a half weeks later, the Council resolved that an experiment on the compressibility of water proposed by Morton be resumed. 19 Dec. 1765, Minutes of Council, Royal Society 5:148.

[87] Charles Cavendish, "A Paper Delivered to Mr da Costa for the Use of the Committee on Mr Canton's Experiments," 21 Oct. 1765, and "Appendix to the Paper on Mr Canton's Experiments," 5 Dec. 1765, JB, Royal Society 25:668–679. The material is also in the Canton Papers, Royal Society, 3.

[88] William E. Knowles Middleton (1966, 150).

also of the results of various usefull and instructive Experiments that he has been pleased to make in private, with that accuracy and exactness which are peculiar to his Lordship, and which few besides himself have a just right to boast of."[89]

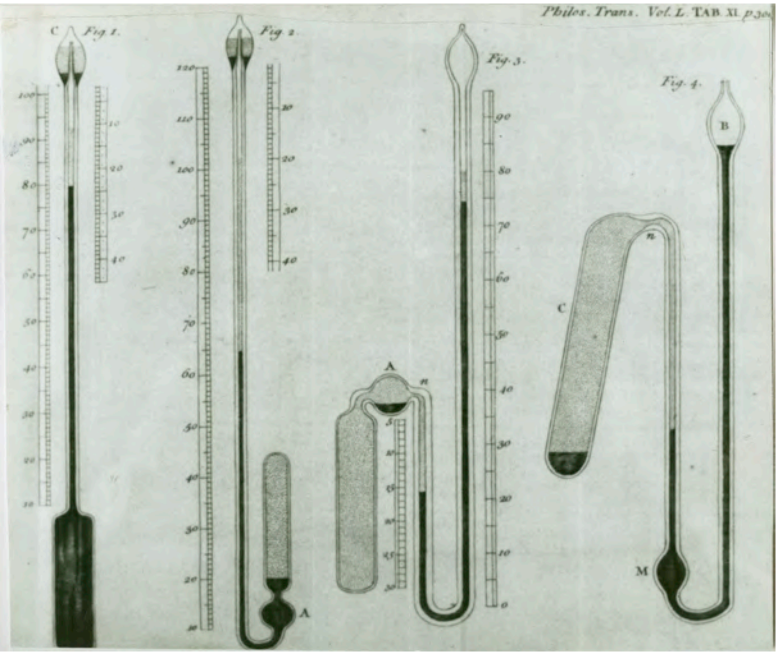

Figure 5.8: Charles Cavendish's Thermometers. The thermometer in Figure 1 shows the greatest degree of heat. It differs from ordinary thermometers in that the top of the stem is drawn into a capillary tube, which ends in a glass ball C. The cylinder at the bottom and part of the stem are filled with mercury (dark part of the figure), showing the ordinary degree of heat. Above the mercury is spirit of wine (alcohol, dotted part of the figure), which also fills the ball C almost to the top of the capillary tube. When the mercury rises with temperature, some spirit of wine is forced out of the capillary tube into the ball. When the mercury falls with a falling temperature, a space at the top of the capillary tube is emptied of spirit of wine. A scale laid beside the capillary tube measures the empty length, which is proportional to the greatest degree of heat that has been registered. Figure 2 is an alternative construction. Figure 3 shows a thermometer for giving the greatest degree of cold. Figure 4 shows how the instrument can be made more compact, an advantage if it is sunk to the bottom of the sea or raised to the upper atmosphere by a kite. The drawing is from a paper that Cavendish communicated to the Royal Society, for which he was awarded the Copley Medal that year. Charles Cavendish (1757).

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[89] The Copley Medal was awarded to Cavendish "on account of his very curious and useful invention of making Thermometers shewing the greatest degrees of heat and cold during the absence of the observer." 17 and 31 Mar. 1757, JB, Royal Society 22:506, 520; 30 Nov. 1757, ibid. 23:638–648, on 638–639. It has been suggested that the Royal Society may have been influenced by Cavendish's social standing as well as by the scientific merits of his work. It could be, though it is not clear that in the year 1757 a more deserving work was passed over. William Lewis continued to bring his important experiments on platinum the before the Society, but he had received the Copley Medal in 1754 for earlier experiments in this series. Yakup Bektas and Maurice Crosland (1992, 52).

Apart from their lofty phrasing, Macclesfield's observations were factual. Cavendish made experiments in various branches of natural philosophy, with careful regard for "accuracy and exactness." What Macclesfield called Cavendish's "modesty" could with equal rights be called his "confidence." Given his rank and his competence, he did not need to (anymore than Macclesfield needed to) publish his researches to gain recognition; indeed if he had published them, he might have betrayed an *im*modesty. It was enough that at times he made his results available to his colleagues in the Royal Society. With a naturalness not easily attained by those who had to advance themselves, Cavendish could live an approximation to the cooperative scientific life envisioned by the utopians of the previous century.

Information about Cavendish's researches away from the Royal Society is fragmentary. His electrical experiments were brought up earlier, referred to by Watson and Franklin. His son Henry's manuscripts record his measurements of the pressure of water vapor over a wide range of temperatures.[90] From the same source, we know that he performed experiments on the bulk of water over a range of temperatures,[91] measured the depression of mercury in glass tubes of different sizes,[92] measured the expansion of mercury with heat,[93] probably did chemical experiments,[94] and made astronomical observations together with Henry.[95] From other sources, we know that he computed tables of errors of time for William Ludlam, an astronomer at Cambridge,[96] made meteorological observations with Heberden,[97] kept a meteorological journal,[98] and took Earth-magnetic readings in his garden.[99]

Cavendish converted water to vapor and back with an ingenious and very simple apparatus, similar to Canton's. He filled a barometer enlarged into a ball on top with mercury

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[90] Charles Cavendish's values for aqueous vapor tension, given in inches of mercury, are reproduced in an editor's note, in *Sci. Pap.* 2:355.

[91] In connection with government taxes on spirits, Henry Cavendish supplied a table of the bulk of water at degrees of heat from 25 to 210°. "From the Experiments of Lord Charles Cavendish, Communicated by Mr. Henry Cavendish. March 1790," Blagden Collection, Royal Society, Misc. Notes. In the same connection, he communicated the weight of a cubic foot of water, "the result of my father's experiment." Henry Cavendish to Charles Blagden, [probably 1790]; in Jungnickel and McCormmach (1999, 673–674).

[92] Henry Cavendish included his father's table of the depression of mercury in his report on the meteorological instruments of the Royal Society in 1776. Cavendish, (1776b, 116). The table was cited for a long time. Pierre Simon Laplace (1839, 1004).

[93] Thomas Young (1807, 2:391).

[94] Henry Cavendish referred to his father's chemicals. He mixed dephlogisticated air in a bottle with "a bit of my father's phosphorus." 16 June 1781, "Experiments on Air," Cavendish Mss II, 5:56.

[95] Packet of astronomical observations from 1774, in Charles Cavendish's hand, with Henry Cavendish's observations added. Cavendish Mss Misc. We know of Charles Cavendish's interest in astronomy from other sources; for example, William Ponsonby to duke of Devonshire, 24 Jan. 1744/43, Devon. Coll.: "I have not had an opportunity lately of seeing Lord Charles, but I make no doubt of his Lordship having made proper observations on the Comet, which appears here in great Splendor."

[96] Charles Cavendish, "Difference to Be Subtracted from Sidereal Time to Reduce It to Mean Time." This and two other tables of calculations on errors of time by him, in William Ludlam (1769, 145–148).

[97] In 1769 Charles Cavendish's good friend the physician William Heberden published a paper in the *Philosophical Transactions* comparing the rainfall at the bottom of a tall building with that at the top. Benjamin Franklin had an explanation, which he put in a letter where he referred to the experiments of Heberden and Charles Cavendish, both "very accurate experimenters." Benjamin Franklin to Thomas Percival, [probably June 1771], in ed. W.B. Wilcox (1969/1974, 155).

[98] Letters from William Borlase to Thomas Hornsby in 1766 and to Charles Lyttleton in 1767, quoted in J. Oliver (1969, 293). William Heberden included Charles Cavendish's readings of the greatest cold at night for twenty years, as he recorded them at his house on Great Marlborough Street, in Heberden (1788, 66).

[99] In his report on the Royal Society's meteorological instruments, Henry Cavendish said that the variation compass had a contrivance "taken from an instrument of Lord Charles Cavendish." Henry Cavendish (1776b, 120).

and then introduced a small quantity of water above the mercury. The level of the mercury immediately lowered because of the pressure of the water vapor above it, the degree of lowering depending on the temperature. In a memorandum, Henry Cavendish wrote, "My father's experiments [with the apparatus] on which what I said concerning the turning of water into vapour are founded seem so convincing as to leave no doubt of the truth of it."[100] With this tribute to Charles Cavendish by Henry Cavendish, we conclude Part I. In Part II, we move from the life of the father to the life of the son.

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[100] This two-sheet memorandum concerns the simple additivity of air pressure and the pressure of water vapor. Cavendish Mss IV, 4.

Part II: The Honorable Henry Cavendish

Chapter 6
Education of Henry Cavendish

A few weeks after Henry Cavendish's death, a neighbor on Bedford Square, the physician John Walker, wrote to the botanist James Edward Smith that Cavendish had been "educated and trained by his father from very early youth to scientific pursuits."[1] Of Cavendish's private education and training in science by his father we know only the outcome, but of his formal education we can say something about goals and methods.

Hackney Academy

It was from tutors, no doubt, that Henry Cavendish received his early general education. We know that the tutor to one of his first cousins was paid one hundred pounds a year,[2] and we assume that a comparable investment was made in Henry's education. With respect to his further education, his father had a choice of a "public" and a private school. Since he himself had gone to a public school, he might be expected to have sent his son to one, especially since that was increasingly the practice among the aristocracy, who regarded public schools as the proper training ground for "public life." Most of the English peerage was educated at one of two public schools, either Eton, which is where Lord Charles had gone, or Westminster, which acquired a reputation as a "nursery of statesmen." Perhaps his sons, Henry and Frederick, did not look to him like future statesmen, or perhaps he did not have good memories of his own schooling, though we note that on at least one occasion, he returned to Eton to attend the public exercises. Or, more likely, he belonged to a trend in eighteenth-century England of fathers taking greater interest in their children, one indication of which was their selection of private schools, whose masters served as surrogate fathers. Whatever his reasoning, he sent his sons to a private school.[3]

There were a good many private schools to choose from, most of them conveniently located in the suburbs of London.[4] The school selected by Charles Cavendish was one of the so-called "academies," Hackney Academy, which emphasized modern subjects (Fig. 6.1). It was the largest of the academies, with an enrollment of about one hundred. Founded

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[1] John Walker to James Edward Smith, 16 Mar. 1810, ed. Smith (1832, 170–171). We assume that this John Walker was the physician who published on geography, natural history, and physiology, and was known for his promotion of vaccination. "Walker, John (1759–1830)," *DNB* 20:533.

[2] Henry Cavendish's aunt Rachel Cavendish married Sir William Morgan of Tredgar. They had two sons, William and Edward, born a few years before Henry Cavendish, and one of these "Master Morgans" had a tutor who received one hundred pounds per annum. This is according to Charles Cavendish in an account for his widowed sister, undated [1740], Devon. Coll., 167.1.

[3] Of the peers about the same age as Charles Cavendish, 46 attended Eton and 31 Westminster; of those about the age of Henry Cavendish, 53 attended Eton and 78 Westminster. From John Cannon (1984, 40, 43–44). H.C. Maxwell Lyte (1911, 287). Randolph Trumbach (1978, 292).

[4] Trumbach (1978, 265).

around 1685, it was also the oldest, and the most fashionable, academy in eighteenth-century England.[5]

Located two miles northeast of London, the village of Hackney was best known as a place where rich Londoners had their country seats. Between London and Hackney the traffic was so heavy that “hackney” became the general word for coaches of the type used there. With its magnificent playing fields and clean air, Hackney Academy enjoyed a reputation for healthy living, and like other private schools it was thought to answer the standard complaints about the public schools, their rampant sexuality.[6] The school to which Charles Cavendish sent his sons was seen as respectable, up-to-date, healthy, and safe.

Figure 6.1: Hackney. William Thornton (1784, facing 488).

There was another consideration, too; Hackney attracted students of a certain kind, not day students from the lower middle class or the crafts, as some academies did, but strictly boarding students, who came from the upper middle and upper classes, in particular, from wealthy Whig families. Ten years before Charles Cavendish entered Henry at Hackney, the hardheaded Lord Hardwicke had sent his son Philip Yorke there to get a useful education. Other Whig peers who sent their sons to Hackney included the duke of Grafton, the earl of Essex, the earl of Grey, and the duke of Devonshire, who sent his son John there at the same time that his brother Charles sent Henry. Evidently the first Cavendishes to attend Hackney, John and Henry were soon joined by Henry’s brother, Frederick. They in turn were followed

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[5] Nicolas Hans (1951, 63–66, 70).

[6] William Thornton (1784, 481). Daniel Lysons (1795, 450–451). Trumbach (1978, 266).

by the sons of the next, the fourth, duke of Devonshire, Richard and George Augustus Henry, as Hackney settled in as a Cavendish tradition.[7]

Hackney Academy was run by the Newcomes, a family of teachers, Anglican clergy, and Cambridge graduates with an interest in science. Henry Newcome, the first of the Hackney Newcomes, a good classical scholar and strict disciplinarian, was still headmaster when Henry Cavendish was there. He and his son Peter, who later became headmaster, were friends of the duke of Kent's family, dining with them at St. James Square.[8] They were friends of the Cavendishes too. Just as his son Henry arrived at Hackney Academy Charles Cavendish recommended Peter Newcome for membership in the Royal Society, as one skilled in mathematics and polite literature. Cosigners of the certificate included the Hackney graduate Yorke, Thomas Birch, and Daniel Wray, suggesting that Peter Newcome was one of Cavendish's circle.[9] While Henry Cavendish was at Hackney, Newcome joined Charles Cavendish and other fellows of the Royal Society in Watson's experiment on the conduction of electricity across the River Thames, and a year after Henry left the school Newcome published his observations on an earthquake felt at Hackney in the *Philosophical Transactions*.[10] The contact between the Cavendishes and the Newcomes was ongoing: years after he had finished at Hackney, and shortly before he was elected fellow, Henry Cavendish was invited by Peter Newcome to a meeting of the Royal Society as his guest.[11] This Newcome was well regarded in the Royal Society, serving on its Council in 1763 and 1764.[12] There were connections between the scientific interests of the Cavendishes and Hackney.

Normally students were admitted to Hackney at age seven, but Henry Cavendish did not enter until he was eleven. He began with the advanced course, instructed in subjects that would apply to his later studies and work: mathematics, natural sciences, French, and Latin. At the usual leaving age, Henry, like the other Cavendishes and like most of the other students at Hackney, proceeded directly to the university, which in his case was Cambridge.

Peterhouse, Cambridge

From the fourteenth century to the time Henry Cavendish entered Cambridge, twenty Cavendishes had graduated from the University.[13] The first duke of Devonshire to get a university education was Charles Cavendish's brother William, who went to Oxford (briefly) not to Cambridge, but he sent his two sons to Cambridge. Charles's oldest son, Henry, having just turned eighteen, entered St. Peter's College, or Peterhouse, Cambridge, on 24 November 1749.[14] He was the first Cavendish to go to that college, where he remained in regular attendance for three years and three months (Fig. 6.2).

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[7] Hans (1951, 72, 243–244).

[8] Thomas Birch Diary, BL Add Mss 4478C. Frequent entries beginning in 1740.

[9] 25 Nov. 1742, Certificates, Royal Society 1:260. The other signers were James Jurin, Benjamin Hoadley, John Ward, and Thomas Walker. Newcome was elected on 24 Feb. 1743.

[10] William Watson (1748a, 62). Newcome reported the earthquake felt by persons at his house in Hackney. Newcome (1750); read 29 Mar. 1750.

[11] 10 Jan. 1760, JB, Royal Society 23:711.

[12] Minutes of Council, Royal Society 5.

[13] John and J.A. Venn (1922, vol. 1).

[14] George Wilson (1851, 17).

The chancellor of the University was the duke of Newcastle, a minister of state, and a distant relative of our Cavendishes. When the master of Peterhouse died, Newcastle lobbied hard for Edmund Keene, a Whig and fellow of Peterhouse. A close overseer of his sons' education, Charles Cavendish was on familiar terms with Keene as he was with the Newcomes at Hackney. During the time Henry was a student at Peterhouse, Keene dined with Cavendish's friends, Birch, Heberden, Wray, Mann, and Squire, and on at least one occasion with Birch and Cavendish.[15] Although Peterhouse was not identified with the nobility, for a time in the middle of the eighteenth century it was fashionable with the upper classes.[16] Henry Cavendish, his brother Frederick, and his cousin John all went to Peterhouse.

Figure 6.2: Peterhouse, Cambridge. From David Logan, *Cantabrigia Illustrata* (Cambridge, 1688).

The attendance at the University when Henry Cavendish entered was small and declining, but the proportion of students who were, like Cavendish, aristocratic was rising.[17] Classed roughly by their station in life, in ascending order students entering Cambridge were sizars, petitioners, fellow commoners, and nobleman. Sizars, who were the poorest and were charged the lowest fees, and who were essentially a college charity, were sons of poor clergy, small farmers, petty tradesmen, and artisans. The majority of students were pensioners, who were better off, commonly sons of more prosperous clergy and professional men, but without distinction of birth. Nobleman paid the highest fees, and since they did not have substantial privileges beyond those of fellow commoners, they often settled to be fellow commoners.[18] Henry Cavendish entered Cambridge as a fellow commoner.

Fellow commoners were occasionally older men who simply liked university life, but most of them were young men of independent means, often sons of country gentleman and commercial magnates if not of nobility. Accounting for just over ten percent of the student population in the eighteenth century, they were a conspicuous minority, inclined to fine dress,

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[15] 6 June 1747, 17 May 1751, 18 and 22 February 1752, Thomas Birch Diary, BL Add Mss 4478C.

[16] D.A. Winstanley (1935, 193). Winstanley says that at midcentury, Peterhouse was "much patronized" by the aristocracy, but it should be noted that of peers born in 1711–40, Henry Cavendish's period, only 3 went to Peterhouse. By contrast, 9 went to Clare College, 8 to King's College, 7 to Trinity College, and 6 to St. John's College. In attendance at Cambridge in 1740–59, while Henry Cavendish was there, out of 27 peers' sons, again only 3 were at Peterhouse. Cannon (1984, 48–51).

[17] Cannon (1984, 45).

[18] Thomas Alfred Walker (1935, 76–78). Edmund Carter (1753, 5, 29).

sometimes accompanied by their own servants, and in any case able to afford to pay poor students to wait on them. They were admitted to the fellows' table, common room, and cellar, where they could smoke clay pipes and drink Spanish and French wine, in which respects they were equivalent to fellows of the college. They were usually excused from performing the college exercises required of humbler undergraduates and of attending lectures by the college tutors.[19] From what we know of Cavendish's later habits, the extravagances of some of the other fellow commoners did not happen to be his, but his privileges were the same as theirs, including freedom to spend most of his time as he wished. The advantages of rank were significant and obvious in the University, reinforcing the generally accepted notion of hierarchy in the society of Cavendish's time.[20]

In the absence of accounts of Cavendish at Cambridge, we fall back on the usual life of Peterhouse undergraduates to give some idea of his. Their service was spare, they dined off pewter, and their diet was monotonous. If the fare remained as it had been in the previous century, they ate mutton five times a week and drank ale and beer, which was brewed at a profit by the college butler. Service was adapted to rank: for fellows and fellow commoners, the butler set four tablecloths, and for the rest, pensioners and sizers, he set two.[21] Prayers were given at six in the morning and again at six at night, supper was at eight, and the college closed at ten. During the day, students could attend college lectures, meet with their tutors, study in their rooms, or seek diversion, for which they had a range of options that included sports, games, and music. College rooms could be chilly, dark, and dreary for everyone. In the year Cavendish arrived, it was ruled that a fire was to be made in the combination room from noon to two o'clock. When students ventured outside of the college, they found themselves in a very small town, Cambridge, with shops that made money off them by selling wine, candles, menswear, books on law and medicine, and pens, pencils, and paper. Coffee houses enjoyed a brisk business, different ones frequented by fellows and by students, where for the price of a coffee they could smoke, read journals, and visit for hours. Fellow commoners usually had extra money, which helped or hindered their progress depending on how they used it.[22]

When Cavendish arrived, Peterhouse had between thirty and forty students, not all of them in residence. During the years he was there, 1749 through 1752, over fifty students were admitted; thirteen of these were fellow commoners, most of whom later went into politics; the rest were sizars and pensioners, most of whom became clerics.[23] No one but Cavendish became notable for any scientific achievement.

The fraction of eminent British scientists in Cavendish's time who had a Cambridge or Oxford education was small and steadily falling.[24] Still there were several young men of future scientific accomplishment in Cambridge while he was there. One year younger than he, Nevil Maskelyne of Trinity College would go on to a distinguished career in astronomy,

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[19] Winstanley (1935, 198). Walker (1935, 78). Cannon (1984, 58).

[20] Cannon (1984, 54–55).

[21] Walker (1935, 79–80).

[22] Ibid., 79–85.

[23] The numbers given here are based on Thomas Alfred Walker (1912). They are less precise but more accurate than those given in our *Cavendish* (1996).

[24] Hans estimates that the proportion of Oxford and Cambridge graduates among eminent British men of science dropped from sixty-seven percent in the seventeenth century to twenty percent at the end of the eighteenth century. His figures are based on rather arbitrary definitions, but the large percentage of scientific practitioners in Henry Cavendish's time who were not Oxford or Cambridge graduates is significant. Hans (1951, 34).

first as assistant to James Bradley and then in Bradley's post of astronomer royal; he was to become one of Cavendish's most valued colleagues. Of about the same age as Cavendish were the promising but short-lived chemist John Hadley, the very capable astronomer Francis Wollaston, and the "excellent mathematician" Francis Maseres.[25] Hadley, who was a guest in the Cavendish home, recommended Henry Cavendish for membership in the Royal Society, and Cavendish was first to sign the certificates recommending both Wollaston and Maseres for membership.[26] Of eventual importance to Cavendish's work was John Michell. Having graduated the year before Cavendish entered Cambridge, Michell was a fellow of Queens' College, where he gave lectures and did experimental work on his own.

Very few eminent British men of science came from the upper class. Nicholas Hans, a historian of eighteenth-century education, groups Cavendish with Robert Boyle and Edward Delaval as the three eminent scientists out of 680 British scientists who were "sons of peers." Cavendish was not, of course, the son of peer, but the point is made: in this company, aristocrats were rare.[27] Boyle the seventeenth-century chemist was a distant relative of the Cavendishes'. Delaval, a younger brother of a peer from an ancient Northumberland family, was another chemist. Because of Delaval's scientific interest, his station in society, his residence (his college, Pembroke, was across the street from Peterhouse), and his voice (which was resounding, a family trait, earning him the local name of "Delaval the loud"), Cavendish could not have failed to know him or about him; he was to receive his Copley Medal in the same year as Cavendish.[28]

The poet Thomas Gray, who resided at Peterhouse not long before Cavendish, described Cambridge fellows as sleepy and drunken and fellow commoners as their imitators, and in his letters from Cambridge he constantly referred to the stupor of the place. There was a measure of truth in his observations, but fellows also had an excuse, since they had little to occupy them officially. At an earlier time, they had given lectures, but by the middle of the eighteenth century their teaching duties had largely fallen away, while their fellowships were becoming sinecures. College lecturers still performed when Cavendish was there, but the practice was on the way out. The motivation to do any work had to come from within, and while there were fellows who had a love of learning and teaching, even a few who were great scholars, most of them contributed little or nothing of significance.[29] The exceptions were fellows who were also tutors, who did serious, regular teaching. Peterhouse had two official tutors, both formerly hard-working sizars at the college who became clerics, neither leaving a mark as a scholar.[30] Assigned to the same pair of tutors as Henry, John Cavendish brought his own private tutor, and Henry might have brought his own too. The University had a small number of professors, whose teaching was increasingly marginal, as the tutors of the colleges took over their subjects.

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[25] On Maseres: William Ludlam (1785, 7).

[26] Certificates, Royal Society 3:65 (Francis Wollaston's announced candidacy, 3 Jan. 1769) and 3:104 (Francis Maseres's announced candidacy, 31 Jan. 1771).

[27] Hans (1951, 34).

[28] The name was given to Delaval by his friend Thomas Gray. Robert Ketton-Cremer (1955, 142–143). Two years older than Cavendish, Delaval became a fellow of Pembroke. "Delaval, Edward Hussey," *DNB*, 1st ed. 5:766–767.

[29] Winstanley (1935, 256–261). Thomas Gray to Horace Walpole, 31 Oct. 1734, in Walpole (1937–1983, vol. 13, pt. 1, 58–59).

[30] Charles Stuart and Chapel Cox.

If fellow commoners wanted to leave with a degree they had to fulfill the requirements, though in form only.[31] Because a degree was unlikely to make a difference in their lives, fellow commoners usually left without one, as Henry Cavendish did on 23 February 1753. The suggestion has been made that he objected to the religious tests, which were stringent,[32] but if that was his reason for not graduating, he left no record of it, then or later. The most likely reason he left without the degree was that he did not consider taking one but simply followed tradition, as did most of the thirteen fellow commoners at Peterhouse during Cavendish's stay, only five of whom took degrees, three of which were Masters of Arts only.[33]

The examination that Cavendish did not take was then on its way to becoming the renowned Cambridge mathematical tripos. Examination results were published beginning in the late 1740s, and beginning in the year Cavendish would have taken it, 1753, the list of examinees was divided into wranglers (top performers) and senior and junior optimes, reflecting the lively competition for a high rank. Because the examination was almost completely mathematical, no doubt Cavendish would have done well: John Green, bishop of Lincoln, writing in 1750 while Cavendish was a student, observed that at Cambridge, "Mathematics and natural philosophy are so generally and exactly understood, that more than twenty in every year of the Candidates for a Bachelor of Arts Degree, are able to demonstrate the principal Propositions in [Newton's] *Principia*; and most other Books of the first Character on those subjects."[34] This would surely have described Cavendish.

With the emphasis on mathematics at Cambridge, there were naturally some very able mathematics teachers, such as John Lawson of Sidney Sussex College, who was mathematical lecturer and then tutor when Cavendish was a student.[35] If Cavendish had taken a degree, his competition in the examinations of 1753 would have included William Disney and Thomas Postlethwaite, both of whom became writers on religion and stayed on in the University. Disney, who graduated first wrangler and later became regius professor of Hebrew, published against Gibbon's history of the Roman Empire and for the superiority of religious duties over worldly considerations.[36] Postlethwaite, third wrangler and later master of Trinity College, published a discourse on Isaiah, while retaining his reputation as one of the best mathematicians in the University.[37] In the previous year, the second wrangler was Henry Boult Cay, who for a time was a fellow of Clare College before becoming a barrister in the Middle Temple; Cavendish probably knew this wrangler as a student, for later he brought him as his guest to the Royal Society Club.[38] Mathematical distinction at Cambridge was not an indicator of future scientific interest; none of the three wranglers, Disney, Postlethwaite, or Cay, became a member of the Royal Society. Under Dr. Law, Keene's successor, Peterhouse produced its first senior wrangler, Robert Thorp, who became coeditor with John Jebb and George Wollaston of a selection from Newton's *Principia*, which was

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[31] They had "to keep the statutory two acts and opponencies and to sit for the Senate House Examination," though in reality they were exempted from the examination and allowed to "huddle" the acts by parroting a few set sentences in Latin. Winstanley (1935, 199).

[32] Wilson (1851, 17, 181). There was no religious test at matriculation, but to graduate with a bachelor's degree, the candidates had to "sign the 36th Canon, the Articles, and the Liturgy of the Church of England."

[33] Walker (1912, 292–306).

[34] John Green, *Academic*, 1750, 23, quoted in Christopher Wordsworth (1968, 73).

[35] "Lawson, John," *DNB*, 1st ed. 11:736–737.

[36] John Nichols, ed. (1817–1858, 6:737). Gibbons attributed the decline of Rome to Christianity.

[37] "Postlethwaite, Thomas," *DNB*, 1st ed. 42:204–205.

[38] 5 Mar. 1767 and 30 June 1768, Minute Book of the Royal Society Club, Royal Society 5. Henry Boult Cay is under his father John Cay's entry in the *Dictionary of National Biography*.

used as a standard text in the University, *Excerpta quaedam e Newtoni Principiis* ….[39] In the next century a number of physicists of the first rank, William Thomson, Peter Guthrie Tait, and James Clerk Maxwell, studied at Peterhouse, known for its excellent coaches William Hopkins and E.J. Routh. There was no hint of this future in the Peterhouse Cavendish knew.

Whereas we think that Charles Cavendish learned mathematics by private lessons from mathematicians who were Newton's associates, Henry Cavendish learned his at Cambridge, if not also elsewhere. At the very least, we can say that whether or not he had a mathematically adept tutor or attended lectures on mathematics, for over three years he was exposed to the mathematical tradition of Cambridge and to the books on mathematics and natural philosophy recommended in a student guide at Cambridge.[40]

In the introduction, we discussed Charles and Henry Cavendish in relation to two revolutions, one political and one scientific. The education that Henry received at the University of Cambridge was related to both. One consequence of the political Revolution of 1688–89 was a change in the Church of England, with Cambridge becoming a stronghold of low-Church latitudinarians and Whigs, who were sympathetic to the Revolution and to Newtonian natural philosophy for the support it gave to the argument from design for the existence of a Creator.[41] Newton's main influence in Cambridge was exerted through his physical theories, the route to which was his mathematics, then the dominant study in the University.[42] Cavendish was indoctrinated in a mathematical and scientific orthodoxy originating in the Scientific Revolution in an institution that favored the poliitical settlement of the Glorious Revolution of 1688–89; for some three odd years he studied Newtonian philosophy in a Whig environment.

Cavendish was not the only major English experimentalist of the second half of the eighteenth century who was exposed to Newtonian philosophy at Cambridge—in addition to Delaval there was the chemist William Hyde Wollaston at the end of the century, for example—but there were very few of them. From his earliest researches, Cavendish demonstrated his mastery of mathematics, in which respect his work differed markedly from that of most of his fellow experimentalists. Although there were additional reasons for the direction he took in science, it bore the imprint of his Cambridge education.

We have only one record of Cavendish's thinking while he was at the University. Frederick, prince of Wales, after holding court in opposition to his father, George II, for nearly fifteen years, died while still waiting for his chance. In the meantime, he had wanted to become chancellor of Cambridge University in 1748, but his father opposed him, and the University took the safe course. As if to compensate Frederick for what it had denied him in life, the University honored his memory by publishing a deluxe edition of academic exercises in 1751 (Oxford did the same). Written in Latin, the laments met the standards of the day, which were not particularly high, inspiring Horace Walpole to make a play on words: "We have been overwhelmed with lamentable Cambridge and Oxford dirges on the Prince's death."[43] Henry Cavendish contributed a poem to the volume, "Lament on the Death of Most Eminent Frederick, Prince of Wales." The premature death of a prince was a fitting occasion to reflect on the fragility of life, and Cavendish dutifully wrote that tears are fruit-

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[39] Walker (1935, 95; 1912, 73, 119).

[40] Daniel Waterland (1740), reported in Wordsworth (1968, 78–81, 248–249, 330–337).

[41] John Gascoigne (1989, 145, 147).

[42] W.W. Rouse Ball (1889, 68, 74–76).

[43] Horace Walpole to Horace Mann, 18 June 1751, in Walpole (1937–1983, vol. 20, pt. 4, 260–261).

less, the thistle and the lily alike flourish, and death plays no favorites. But the middle stanza is not conventional. Here we hear the voice of the future scientific investigator: while nature may mock us, it "does lay bare hidden causes, and the wandering paths of the stars."[44] Such were the circumstances of Cavendish's first publication and probably his last poem, for his preferred way of speaking of the hidden causes of nature would be in the unadorned language of science.

Learning Science

As the University was dominated by its colleges, so its teaching was dominated by the many tutors in the colleges. The much smaller number of university professors tend to be discounted in historical accounts of Cambridge in the eighteenth century. The criticism is often deserved, but their teaching was increasingly irrelevant to most students. Deprived of the usual incentive to lecture, some of them nevertheless took this form of teaching seriously, and almost all of the scientific professors brought out textbooks. From the standpoint of a student who would become a scientific researcher, the professors hold our interest. They alone among the teachers at Cambridge represented the specialized sciences.

William Heberden recalled that in his student days at Cambridge, around 1730, some professors made a difference. The professor of mathematics Nicholas Saunderson lectured on Newton's work when the college lecturers largely ignored the subject, and the text on optics by the professor of astronomy and experimental philosophy Robert Smith, and the text on natural philosophy by Thomas Rutherforth, future professor of divinity, drew attention to their subjects and spread the teaching of them in the university.[45] Whether or not Cavendish heard Cambridge professors lecture, he most certainly knew their texts. In this section, we look at texts written by professors for use in Cambridge, in which way we learn, as interested students in Cavendish's day learned, the approved ways of studying nature.

The education Cavendish received in Cambridge rested on major achievements of the Scientific Revolution: mathematics replaced logic in the curriculum, and natural philosophy was regarded as the most important branch of philosophy. The power of mathematics to describe Cavendish's "wandering paths of the stars" was impressively demonstrated by Newton in his *Principia*. First published in 1687, the book appeared in three editions in Newton's lifetime, the last in 1726.[46] The complementary power of experiments was demonstrated by Newton in his *Opticks*, which too appeared in three editions in his lifetime, the first in 1704 and the last in 1717/18. The treatise concluded with a series of questions and speculations, which were expanded in each edition, their object being to stimulate others to carry forward the investigation of nature, and many readers regarded them as the most important part.[47] Cavendish's library contained all editions of the *Principia* and *Opticks*.

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[44] Henry Cavendish (1751).

[45] Heberden quoted in Wordsworth (1968, 66–67). Gascoigne (1989, 175).

[46] The editors of the three editions of Newton's *Principia*, were Halley in 1687, Roger Cotes in 1713, and Henry Pemberton in 1726. In 1729 an English translation was brought out by Andrew Motte, a later edition of which is *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World*, Newton (1962). I.B. Cohen (1971, vii, 7).

[47] The editions in his lifetime were: in 1704 in English; in 1706 in Latin; and in 1717/18 in English again. Isaac Newton (1952). I.B. Cohen (1974, 59).

Figure 6.3: Sir Isaac Newton. Portrait by Godfrey Kneller, 1702. Wikimedia Commons.

Newton's principal physical writings were widely accessible, but his published mathematical writings at the time of his death consisted of a few scattered tracts, which by no means revealed the extent of his researches. In the *Principia*, he introduced the mathematical ideas his readers needed to understand what followed, and in the first edition of *Opticks* he appended two Latin treatises on curves and their quadrature, which later came out in English translations. It was left to his followers to publish other mathematical writings, the existence of which was known since he lent out his manuscripts.

In the *Principia* Newton laid down the laws of matter and motion and the law of universal gravitation, from which he deduced the motions of the planets, comets, moon, and tides. The sweeping deductive power of the *Principia* was the basis of its appeal:[48] the laws of motion were presumed to contain all of the relations between matter, motion, and force in the sense that all of the theorems of geometry are contained in the axioms of that subject. Other forces besides gravitation were known to exist, but they had not yet been experimentally determined and mathematically described. The "whole burden of philosophy," Newton wrote in the *Principia*, was to observe the motions of bodies and from them to deduce the forces acting and then to deduce from these forces the other phenomena of nature.[49] Cavendish's electrical researches exemplified this objective.

Like the *Principia*, *Opticks* begins with definitions and axioms or laws, but a glance at its pages reveals that it contains an orderly progression of experiments. It argues for a new understanding of light: the white light of the Sun is compounded of heterogeneous colored

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[48] C. Truesdell (1960, 6).

[49] Newton (1962, 1:xvii–xviii).

rays, which are original and immutable qualities of light, quantitatively distinguishable by their different degrees of bending, or refrangibility, upon passing through transparent substances. For the explanation of the bending and reflecting of light by bodies, Newton looked to the subject of his *Principia*, forces and motions. Between the rays of light and bodies, a force acts, and although for some results it is unnecessary to know "what kind of Force," the exact description of the force was an important question.[50] The problem of light was more difficult than the problem of gravitating bodies; the bodies of the solar system move in ellipses and parabolas, but light passing near bodies has a "motion like that of an Eel."[51] Newton did not complete a "Theory of Light," but only began one. The sixteen "queries" in the first edition of *Opticks* suggest how at the time he expected the enlarged science of optics to appear when completed. Cavendish accepted Newton's description there of light as particles that interact with the particles of ordinary bodies through forces.

Heat is the subject of nearly half of the first set of queries in *Opticks*. By the law of action and reaction, the third of Newton's laws of motion, the reflection, refraction, inflection, and emission of light by bodies induce an internal vibration in the bodies, which constitutes heat.[52] Cavendish accepted and developed the identification of heat with the internal vibrations of bodies, which he called "Newton's theory of heat."

In the second edition of *Opticks*, Newton added several queries that give the fullest statement of his expectation for the mechanics of the interaction of light and ordinary bodies. To the third edition, he added a final set of queries on the ether presumed to fill space. Backed by Newton's authority, the queries of the *Opticks* proved to be a source of new paths (and a few dead ends) for readers throughout much of the eighteenth century.

At whatever level Cavendish studied the *Principia* at Cambridge, in his later scientific work he revealed his command of the main subjects of that book, mechanics, mathematics, and mathematical astronomy. In addition, his manuscripts contain studies of dispersion, refraction, and lenses, which connect his work with Newton's other treatise, *Opticks*.

One of the first to lecture on Newtonian science in Cambridge was William Whiston, who wrote several texts still in use in the University when Cavendish was there. In his *Memoirs*, Whiston recalled returning to Cambridge after he had taken holy orders, to join what he called the "poor wretches" who were still studying Descartes' fictions. Having heard Newton lecture without understanding a word, it was only after reading a paper by the astronomer David Gregory that he realized that the *Principia* was the work of a "*Divine Genius*." With "immense pains" and "utmost zeal," he struggled with the book on his own. Later he published *A New Theory of the Earth*, which he submitted and dedicated to Newton, "on whose principles it depended, and who well approved of it." From Newton's explanation of comets, Whiston demonstrated the book of Genesis: the Earth, originally a Sun-bound comet, was struck by another comet, causing the Deluge and giving the Earth its elliptical path and diurnal rotation. These cosmic events expressed God's will, but the agency was Newton's universal gravitation.[53] When Newton left Cambridge for his post at the Mint in London, he arranged for Whiston to succeed him as Lucasian Professor of Mathematics in Cambridge.

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[50] Newton (1952, 82).

[51] Newton (1952, 339). Query 3.

[52] Ibid. Query 5.

[53] William Whiston (1749, 37, 43; 1737); *A New Theory of the Earth*…, 5th ed. (London, 1737). Jacques Roger (1976).

An ambitious man of wide interests and strong commitments, Whiston published his lectures in Cambridge on astronomy and on natural philosophy, the latter as the first extensive commentary on the *Principia*, and with the author's approval he published Newton's lectures on universal arithmetic, or algebra. He eventually fell out of favor with Newton (and Cambridge), but Newton had done much for him, placing him in Cambridge and showing him his favor for many years. Whiston reciprocated by helping implement Newtonian studies at Cambridge.[54]

While he was professor of mathematics, Whiston let the young scholar Nicholas Saunderson lecture to large audiences on the same material, Newton's universal arithmetic and his *Principia* and *Opticks*. Blind virtually from birth, Saunderson demonstrated, according to his publisher, how far the faculties of the imagination and memory could compensate for the want of a sense. His fellow mathematician Roger Cotes thought that his "want of sight" was an advantage as well as a disadvantage. He definitely was a source of local wonder, being able to distinguish a fifth part of a musical note, estimate the size of a room from sounds in it, tell the difference between genuine and false medals by touch, and, most important, gain proficiency in higher mathematics. Elected Whiston's successor as Lucasian Professor of Mathematics, Saunderson had good relations with persons associated with Newton: Cotes, Jones, De Moivre, Machin, John Keill, and others. His "reverence for Newton was extreme," as he made Newton's work the center of his teaching. Like Whiston, Saunderson's importance was not as an original mathematician—the historian of mathematics at Cambridge says that Whiston and Saunderson "barely escape mediocrity"—but as an industrious teacher of the new mathematics and natural philosophy in Cambridge. Saunderson published no books himself, but the year after his death in 1739 his lectures on algebra were brought out, *Elements of Algebra for Students*. His lectures on Newton's form of the calculus, *The Method of Fluxions Applied to a Select Number of Useful Problems [...] and an Explanation of the Principal Propositions of Sir Isaac Newton's Philosophy*,were published in 1756, four years after Cavendish had left Cambridge, but manuscripts of the lectures had long circulated there and are thought to give a good idea of how the material was taught in Cambridge at the time. The Advertisement in Saunderon's book says that it was "reckoned the best for students in the universities, of any yet published," and that any defects in the presentation could be overcome with the help of the student's tutor. *The Method of Fluxions* begins abruptly with a proposition about triangles, the sides of which are identified with Newtonian forces. Here and there in the book experiments are mentioned and empirical numbers are used in problems, but the subject is the mathematical parts of natural philosophy. Students learned the mathematical representation of nature and mathematical analysis at the same time, with fluxions, fluents, algebra, geometry, and mechanics forming a seemingly inseparable subject. In his teaching, Saunderson conveyed a way of thinking about nature, the lesson a Cambridge student in the middle of the eighteenth century would have come away with.[55]

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[54] Whiston published his astronomical lectures in 1707 in Latin; translated in 1715, they appeared as *Astronomical Lectures, Read in the Publick Schools of Cambridge....* These lectures include "attraction" and Newton's theory of the moon; they are an astronomical preparation for Newton's philosophy, which Whiston promised to give next term. In 1710 he published his lectures on natural philosophy, which were translated in 1716, *Sir Isaac Newton's Mathematical Philosophy More Easily Demonstrated.* Maureen Farrell (1981, 200). Rouse Ball (1889, 83–85, 94–95). "Whiston, William," *DNB*, 1st ed. 21:10–14. D.T. Whiteside, in Newton (1967, 1:xvi).

[55] Rouse Ball (1889, 86, 88). "Saunderson or Sanderson, Nicholas," *DNB*, 1st ed. 17:821–822. Roger Cotes to William Jones, 25 Nov. 1711, and Nicholas Saunderson to William Jones, 4 Feb. 1714/13, in Rigaud (1965, 1:261;

Upon Saunderson's death, the ageing De Moivre, who looked to one observer as if he were "fit for his coffin," was passed over, and Whiston, who wanted to return, was not taken seriously. The new Lucasian Professor of Mathematics was John Colson,[56] a mathematical schoolmaster who had taken a modestly active part in the science of his day. As an original mathematician, he deserves no more than passing notice. His principal scientific claim to the Lucasian chair was his publication three years earlier of a tract that Newton had wanted to publish but for which there had been no market. Long circulated in Cambridge, Newton's manuscript was translated from its original Latin into English by Colson as *The Method of Fluxions and Infinite Series*, with a dedicatation to William Jones. The Cambridge diarist and antiquarian William Cole described Colson as a "plain honest man of great industry and assiduity," but who disappointed the university "in its expectations of a professor that was to give credit to it by his lectures."[57] He disappointed because of his teaching, not because of his research, of which there was none to speak of. Colson was Lucasian Professor when Cavendish was a student at Cambridge.

If Colson's accomplishments as a mathematician were minor, his enthusiasm for fluxions and its inventor cannot be faulted. His praise in the annotated edition of Newton's *Method of Fluxions* stands out among Newtonian panegyrics: Newton was the "greatest master in mathematical and philosophical knowledge, that ever appear'd in the world," and his doctrine of fluxions was the "noblest effort that ever was made by the human mind." Unlike Newton's other mathematical writings, which were "accidental and occasional," his *Method* was intended as a text for "novices and learners," a goal with which the teacher Colson could identify. Colson made clear the distinction between textbook and original work, between a teacher like himself and an inventor like Newton. The teacher and textbook had their modest place: with their aid, the beginner could comprehend the work of the greatest thinker of all time. Colson's edition was at once a textbook, an indoctrination in mathematical Newtonianism, and a polemic in defense of Newton.[58]

For the learner of fluxions and infinite series, there was Newton's own presentation, and there was Colson's. If Newton's was terse, Colson's was prolix; Newton's treatment of infinite series occupied twenty pages, Colson's "perpetual comment" ninety-eight.[59] Colson assumed little of his reader, patiently explaining what he regarded as the greatest difficulty

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265). Nicolas Saunderson (1756, ix–x, 79, 81), and Advertisement. "Saunderson or Sanderson, Nicholas," *DNB*, 1st ed. 17:821–822. Like Newton's lectures, Saunderson's consisted of a set of examples, as recalled by the Cambridge astronomer William Ludlam, who knew them firsthand. Ludlam had been one of Saunderson's pupils, who read sections of Newton's *Principia*. William Ludlam (1785, 6).

[56] Quotation about De Moivre's age and infirmity from William Cole's diary, quoted in "Colson, John," *DNB*, 1st ed. 4:801–802, on 801. From 1709 until he was named Lucasian Professor, John Colson taught at Sir Joseph Williamson's Mathematical School in Rochester. R.V. and P.J. Wallis (1986, 29).

[57] In 1738 Colson translated from the French a theoretical paper by Alexis Clairaut on the figure of the planets for the *Philosophical Transactions*. Before that, he published two mathematical papers of his own on algebra and another on spherical maps in the same journal. One of the papers on algebra was translated into Latin and appended to the 1732 Leiden edition of Newton's *Arithmetica Universalis*. "Colson, John," *DNB*, 1st ed. 4:801–802. Rouse Ball (1889, 100–101). Whiteside in Newton (1967, 1:xv; 8; 8:xxiii).

[58] Colson's comments in *The Method of Fluxions and Infinite Series.... By the Inventor Sir Isaac Newton....To Which Is Subjoined, a Perpetual Comment* ... (1736, ix–xii, xx, 335–336).

[59] Colson's commentary was considerably shorter than the commentary by John Stewart, professor of mathematics in the University of Aberdeen, to a translation of two mathematical tracks by Newton; the two tracks occupy 54 pages of Stewart's book, his commentary 497 pages plus introductory matter. *Sir Isaac Newton's Two Treatises: Of the Quadrature of Curves, and Analysis by Equations of an Infinite Number of Terms, Explained* ... (London, 1745).

for a beginner, the notion of a vanishing quantity, expanding freely on the text, giving copious examples, and writing not as a mathematician but as an eternally patient teacher. We cannot know if Cavendish read Colson's commentary, but if he did, he read two observations that might stimulate a beginning mathematical student. One is that Newton had not said the last word on the subject: improvements in the method of fluxions had been made since Newton, and the subject was capable of further perfection. The other observation has to do with Newton's method, that of analysis, which proceeds from the known to the unknown; analytics is the "art of invention," a method of discovery.[60]

The Newtonian school at Cambridge began soon after Newton left the University for London. Richard Bentley,[61] master of Trinity College, was not himself a man of science, but he was a good judge of men who were. Wanting to make his college a center of "Newtonian philosophy," he had a laboratory built for Newton's friend John Francis Vigani, who had lectured on chemistry at Queens' College. With Newton's and Whiston's help, he secured the new Plumian Professorship of Astronomy and Experimental Philosophy for Roger Cotes, who had shown mathematical talent while a student at Trinity. He raised a subscription for an astronomical observatory to be built over Trinity's entrance gate and for neighboring rooms to be assigned to Cotes ("Bentley's man") and to his assistant, his cousin Robert Smith. He arranged for Whiston, of Clare College, to have rooms in Trinity under Cotes's observatory.[62] Trinity set a precedent for other colleges. Bentley, more than any other, was responsible for the eventual dominance of the Newtonian school of science and mathematics at Cambridge.

Bentley bore the expense of a new edition of Newton's *Principia* in 1713 and was himself going to edit it, but sensibly assigned it to Cotes, whose preface to the edition became a cardinal document in the spread of Newtonian thought. Three years later, Cotes died suddenly. He had published only two papers, one of which Robert Smith included in a posthumous edition of Cotes's mathematical manuscripts, *Harmonia Mensurarum*,which contained in addition to writings on logarithms, fluxions, and mechanics the "earliest attempt to frame a theory of errors." Led to the theory by his interest in practical astronomy and its instruments, Cotes made mathematically rigorous the limits of errors arising from imperfections of the senses and of instruments.[63] With his help, observers could calculate which errors were negligible and which were not and take steps to minimize the latter. Cavendish showed a working knowledge of the theory of errors in his experimental work.

Cotes and Whiston gave experimental lectures in natural philosophy in the observatory at Trinity. When Whiston left Cambridge, Cotes continued the lectures by himself, and after Cotes's death, Robert Smith continued them, and he also published Cotes's lectures. Intended for a wide audience, Cotes's *Hydrostatical and Pneumatical Lectures*, Smith said, could be read by persons knowing little mathematics "with as much ease and pleasure, as in reading a piece of history." Unwilling to leave it at that, Smith added mathematical notes of

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[60] Colson (1736, 1, 144, 335).

[61] Rouse Ball (1889, 149, 155).

[62] "Bentley, Richard," *DNB*, 1st ed. 2:306–314, on 312. A. Rupert Hall (1976, 26–27). James Henry Monk (1833, 202–204). Whiston (1749, 133). Ronald Gowing (1983, 8, 14). Rouse Ball (1889, 89). The Plumian Professorship was endowed by Thomas Plume, archdeacon of Rochester, in 1704; Cotes was elected to the chair two years later.

[63] Gowing (1983, 91–93).

his own.[64] A second edition of Cotes's lectures was published in Cambridge in 1747, two years before Cavendish entered the University.

Cotes's lectures dealt mainly with pneumatics but also with hydrostatics, both subjects relying on that most precise of instruments, the balance. Gravity, the force to which the balance responds, Cotes wrote, "is a property of so universal an extent" that even "air, which as I shall afterwards shew, may be weighed in the ballance." Cotes drew on Newton's *Principia* to explain the physical properties of air, its weight and elasticity, and its role as the medium of sound. He concluded the four-week course with a lecture on "factitious airs," taken from Robert Boyle's *New Experiments Physico-Mechanical*. These were airs, or gases, contained in bodies, which could be freed by various means: fire, explosion, dissolution, putrefaction, and fermentation. Cotes presented factitious airs not as a completed subject for textbooks but as a new subject; at the time of his lectures, Boyle's were the "best and almost only trials which have yet been made concerning factitious airs." By introducing factitious airs, Cotes extended the exact science of pneumatics to a largely unknown field of gaseous phenomena attending chemical actions. He referred to Newton's *Opticks* to point to the future direction of science: "Who ever will read those few pages [the last query] of that excellent book [*Opticks*], may find there in my opinion, more solid foundations for the advancement of natural philosophy, than in all the volumes that have hitherto been published upon that subject."[65] We know that Cavendish read Cotes's lectures, since he cited them in his first publication, which was on factitious airs. Cavendish's physical approach to "pneumatic chemistry" was foreshadowed by Cotes's and perhaps stimulated by it.

In 1716, at age twenty-seven, Robert Smith succeeded Cotes as Plumian Professor in Cambridge, the position he held for the next forty-four years. He also succeeded Bentley as master of Trinity College, and like his predecessor he vigorously promoted science in Cambridge. To encourage the student Richard Watson, later professor of chemistry at Cambridge, Smith appointed him to a scholarship, urged him to read Saunderson's *Fluxions* and other mathematical books, and gave him, Watson said, "a spur to my industry, and wings to my ambition." Israel Lyons, who lived in Cambridge, showed such promise that Smith offered to put him through school; Lyons dedicated his *Treatise of Fluxions* in 1758 to Smith. Smith completed the Trinity observatory Cotes had begun, and he gave the college a bust of Cotes and money to erect a monument to him. He left large benefactions to the College, to the University, and to science, which included funds for his own Plumian Professorship and for annual Smith Prizes to go to the two commencing bachelors of art who had done the best work in mathematics and natural philosophy. Smith presented his college with a statue of Newton by Louis-François Roubilliac.[66] As a student, Cavendish would have known that the Plumian Professor was one of the founders of Newtonian science at Cambridge.

When Cavendish was a student, the most important Newtonian work by a Cambridge professor was Robert Smith's *A Compleat System of Opticks*.[67] Newton's *Opticks* was a scientific work: his account of experiments on the analysis of white light into colored rays

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[64] "The Editor's Preface" in Roger Cotes (1747). For his joint course of experiments with Cotes, Whiston gave half of the lectures, but he did not publish them. "Cotes," *DNB*, 1st ed. 4:1029.

[65] Cotes (1747, 5, 123, 187, 201–203).

[66] "Smith, Robert," *DNB* 1st ed. 18:517–519. Winstanley (1935, 150). R.W.T. Gunther (1937, 61). Rouse Ball (1889, 91). Monk (1833, 2:168). Robert Willis and John Willis Clark (1886, 600). Richard Watson (1818, 14). In 1758 Lyons dedicated to Smith his *Treatise on Fluxions*, which was used in teaching at Cambridge alongside texts on the subject by Newton, Saunderson, and others.

[67] Robert Smith (1738).

was accessible to learners, but the rest of his book addressed difficult problems of the interaction of light and matter, raising questions and in general lacking the conclusiveness of a textbook. Smith's *Opticks* was a textbook. His treatment of Newton's optics was selective, overlooking Newton's second thoughts and hesitations, and omitting what did not fit. He cited Newton's queries where they supported his "system," treating them as assertions not questions.

Because Smith presented optics as a system, he could not ignore the question of the nature of light. In his answer, he followed Newton, only he was more decisive. Newton inclined towards a corpuscular view of light, but he speculated freely on an ether. Smith acknowledged that Newton's ether could explain the phenomena of light equally well, but he preferred Newton's corpuscles of light, in line with the thinking of the time. Smith's *Opticks* became the main authority on Newtonian optics after Newton's own *Opticks*, in some respects supplanting it.[68] Cavendish accepted the corpuscular theory, and nowhere in his writings did he use the word that characterized the alternative theory, "ether."

In discussing how we come by our ideas of things by sight, Smith considered the question the astronomer Samuel Molyneux asked of the philosopher John Locke: would a blind man who suddenly regained his sight be able to distinguish a globe from a cube by sight alone? To this question the philosophers had given a negative answer, which was apparently confirmed by the recent experience of a man reported in the *Philosophical Transactions.* Unconvinced by the philosophers, Smith had a ready subject at hand, his colleague the blind Lucasian Professor. Saunderson agreed with Smith that by "reason," the blind man upon regaining his sight could tell the globe from the cube.[69] The answer, whether correct or not, was an inference from the experimental philosophy: in knowing the world, experience is reflected upon by reason, a lesson Cavendish took to heart.

Many of the topics in Smith's *Opticks* interested Cavendish. Smith included a history of astronomy, beginning with Galileo, from whom astronomy acquired its essential, modern instrument, the telescope. The Cavendish library contained the classic works of astronomy by Copernicus, Brahe, and Kepler. Smith described Huygens's long, highly magnifying refracting telescopes, which Cavendish borrowed from the Royal Society and mounted at his house. Smith commented on the importance of London's scientific instrument makers for the progress of astronomy; George Graham, an instrument maker of "extraordinary skill," helped him in writing his book. Cavendish associated with instrument makers as much as he did with scientific investigators. Smith developed the optics of lenses and mirrors, which Cavendish took up in a number of papers. Smith treated the human eye as an optical instrument, constructing a "tolerable eye" from two hemispheres filled with water,[70] and he appended an essay on indistinct vision by his friend and colleague at Trinity, the Bentley protégé James Jurin.[71] Cavendish experimented on the eye as an optical instrument, and he corresponded with the astronomer William Herschel on indistinct vision.

Smith brought out a second scientific book, concerned with the other most discriminating sense, hearing. *Harmonics, or the Philosophy of Musical Sounds*, was well received, recommended by George Lewis Scott, one of De Moivre's pupils, to Edward Gibbon as the

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[68] Henry John Steffens (1977, 48, 50, 53); G.N. Cantor (1983, 33–34).

[69] Smith (1738, 1:42–43), and "The Author's Remarks upon the Whole," at the end of the book, on 28–29.

[70] Ibid., 25, 332.

[71] James Jurin, "An Essay upon Distinct and Indistinct Vision," appended to Smith's *Opticks* (1738, 115–170).

"principal book of the kind."[72] Like natural philosophy, music had recently undergone major changes. The monodic idea had become well established, and with it so had the harmonic as opposed to the contrapuntal approach to musical composition. The emphasis had shifted to chords and the modern notion of key: by the use of a definite key and of modulation between keys, unity could be achieved in long expressive melodies. There was a problem with keys, however: modulation between closely related keys could be carried out satisfactorily but not modulation between remoter keys, as required for greater contrast.[73] Ancient musical theorists such as Ptolemy had considered only perfect consonances, and as a result their scales contained imperfect consonances, disagreeable to the ear. By distributing the largest imperfections in certain concords over the others, modern theorists tempered the ancient scales, making the imperfect concords less offensive, although there were more of them. Smith did not adopt the well-tempered scale, as promoted by Bach in the *Well-Tempered Clavichord*, but addressed the problem starting from the "first principles of the science." He redistributed the imperfections of the ancient scales in such a way as to make the imperfect consonances all equally "harmonious." For this "scientific solution" of the artistic problem, Smith constructed a theory of imperfect consonances, the first ever; it was his acoustical version of indistinct vision in optics.[74]

Smith lived in the Enlightenment, a word which referred to a felt need for clarity. Like musicians of "delicate ear," at performances Smith preferred to listen to a single string rather than to unisons, octaves, and multiple parts, in agreement with his preference for "distinctness and clearness, spirit and duration" over "beating and jarring" and "confused noise." He quoted from his other book, *System of Opticks*, from Jurin's account of what happens when a person comes out of a strong light into a closed room: at first the room appears dark, but in time the eye accommodates to the darkness and the room appears light. The discernment of clarity within a confusion of sound and the recovery of vision in darkness were analogous, symbolizing the natural philosopher's quest for order and understanding. Musicians at first disliked Smith's retuned organ despite its improved harmony, but musicians, like scientists, could be educated, Smith said, and in time they would no longer be able to stand the "course harmony" of organs tuned in the old way. Smith's aesthetics was an aesthetics supported by mathematics, experiment, and theory.[75]

The study of harmonics underscored the value of theory in the science of music. In the ancient world musicians followed their ear rather than the "theories of philosophers," Smith said; they arrived at temperament "before the reason of it was discovered, and the method and measure of it was reduced to regular theory." To the moderns, the ear was no longer sufficient. Smith, an expert performer on the violin-cello, had a musical ear but he did not need one. In harmonics, he needed only scientific theory, as he explained: a person without a musical ear could tune an organ to any temperament and to "any desired degree of exactness, far beyond what the finest ear unassisted by theory can possibly attain to." It was the same in optics as in music: Smith's colleague the blind mathematician Saunderson taught Newton's theory of colors.[76]

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[72] Robert Smith (1759). First edition in 1749. "Smith," *DSB* 12:477. "Smith," *DNB* 18:519.

[73] Donald A. Ferguson (1935, 272–278).

[74] Smith (1759, v–vii).

[75] Ibid., 171–172, 210.

[76] Ibid., viii-ix, 33–35.

Because he approached music as an experimental philosopher, Smith confirmed his mathematical theory by practice. At his request, experiments were performed by the Cambridge organist and by the clockmaker John Harrison, who played the bass-viol. In the modification of musical instruments required by his theory, he was helped by "two of the most ingenious and learned gentleman in this University," John Michell, a skilled violinist, who became a colleague of Henry Cavendish's, and William Ludlam, to whom Charles Cavendish supplied astronomical calculations. Smith and his collaborators belonged to a tradition of scientists with an interest in music going back to Pythagoras and coming down to Huygens and Newton. His system was an improvement over other systems of temperament, but in the end the modification of instruments made it impractical.[77]

Modifier of instruments, experimenter, and mathematical theorist, Robert Smith was the complete natural philosopher in the fields he worked in. Of persons teaching scientific subjects at Cambridge, with the possible exception of John Michell, Smith was closest to Cavendish in his interests and skills. We would like to think that Cavendish became acquainted with Smith at Cambridge, but that seems unlikely. They were not in the same college, and Smith probably did not lecture any longer, and in any case, by then he was ill, irascible, and reclusive.[78] It is, however, virtually certain that Cavendish knew Smith through his books on optics and harmonics. We know that Charles Cavendish owned *A System of Opticks*, since he was one of its subscribers.[79] As we will see later in this chapter, Henry Cavendish was probably drawn to music, in which case he would certainly have known about Smith's *Harmonics*.

The Plumian Professorship was designated for astronomy as well as for experimental philosophy; during the time Cavendish was at Cambridge, the astronomy half was taken over by a new professorship, which combined astronomy with mathematics. In 1750 the master of Pembroke Hall Roger Long was named the first Lowndean Professor of Astronomy and Geometry, a position he would hold until his death twenty years later. Conspicuous as a Tory in predominantly Whig Cambridge and a contrarian, Long constantly feuded with the fellows of his college, especially over the right of veto, which he exercised with willful frequency. Like his Plumian colleague Smith, Long was a skilled musician, who presented the king and queen with a musical instrument of his own invention, the "lyrichord." In his field of astronomy, he was known for his models of the heavens, two of which are described in his *Astronomy*, a standard textbook in the University when Cavendish arrived. The frontispiece illustrates an early construction that Long used for demonstration, a glass celestial sphere known to a "great number of people" and imperfectly copied by several. The book describes a second construction, a narrow ring twenty feet across on which the constellations of the zodiac and the ecliptic were inscribed, treating viewers seated in the middle to

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[77] Ibid., ix–xiv. Edgar W. Morse (1975, 477).

[78] "Smith," *DNB* 18:518.

[79] As the subtitle suggests—*A Popular, a Mathematical, a Mechanical, and a Philosophical Treatise*—Smith's book contains material of interest to a wide variety of readers. The 340 subscribers included members of De Moivre's mathematical circle such as Macclesfield, De Moivre, and Folkes (who subscribed for twelve copies); Cambridge mathematicians and physical scientists such as John Colson, Roger Long, Nicolas Saunderson, Charles Mason, John Rowning, and Richard Davies; Scottish professors of mathematics and physical science such as Colin Maclaurin, Robert Simpson, John Stewart, and Robert Dick; and London instrument makers such as George Graham, James Short, and Jonathan Sissons. Ten years before its publication, in 1728, Smith first advertised for subscribers, and if that was when Cavendish subscribed, it was just as he entered the Royal Society. He paid thirty shillings for each of the two volumes. Alice Nell Walters (1992, 7).

a panoramic view of this part of the heavens. Long expressed the wish to build a planetarium that would rotate around a platform of spectators. He later built and installed in a court at Pembroke Hall the revolving "great sphere" on which the zodiac and the ecliptic and planetary orbits were inscribed, measuring eighteen feet across and capable of holding thirty people. This consummate lecturer's planetarium provided the frontispiece of the second volume of *Astronomy*. Long was assisted in the construction of the revolving globe by Richard Dunthorne, formerly his footboy, who held the butlership at Pembroke; in that unlikely arrangment, Dunthorne published a number of valuable works on the motions of the moon, comets, and satellites of Jupiter, and after Long's death he assisted in completing his *Astronomy*.[80] Like his planetarium, Long's perspective was expansive. In contrast to the usual perfunctory single chapter on the fixed stars, his *Astronomy* devotes many chapters to their immense distances and other cosmic properties. Drawn to the great questions of astronomy, Long concluded after "long and careful scrutiny," incorrectly as it happened, that stars do not move. Long's main contribution to astronomy in Cambridge was his teaching, and his textbook was his main publication.[81]

Long regarded astronomy as part of natural philosophy, the study of the bodies that comprise the universe. Newton's *Principia*, he said, raised astronomy "at once, to a greater degree of perfection than could have been hoped for from the united labours of the most learned men, for many ages," the accomplishment of "the amazing genius of one man—the immortal *Newton*!"[82] Because the force of gravity was known but the forces of light, magnetism, and electricity were not, astronomy was far more advanced than the other parts of natural philosophy. Instrument makers, especially the British, supplied the observers who kept astronomy advancing after Newton. Long used mathematics sparingly, but he began his lectures with the subject of quantity, making clear what kind of science astronomy was. Because Charles Cavendish was a subscriber to Long's *Astronomy*, Henry Cavendish is certain to have seen it, and he might have attended the lectures on which it was based. After Cambridge, he would acquire telescopes and make studies of comets' orbits and other astronomical objects.

In 1748, the year before Cavendish entered Cambridge, the future regius professor of divinity Thomas Rutherforth published lectures he gave at St. John's College, *A System of Natural Philosophy*.[83] Rutherforth's combination of interests, theology and natural philosophy, made sense in a university that prepared students for clerical careers and taught Newton's mathematics. He used geometrical arguments throughout his lectures, even managing to convey a notion of infinitesimal reasoning while at the same time not assuming a

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[80] Wordsworth (1968, 249). "Dunthorne, Richard," *DNB*, 1st ed. 6:235–236.

[81] The first volume of Long's *Astronomy, In Five Books* was published in Cambridge in 1742. The second volume did not appear until twenty-two years later, in 1764, for reasons of which, Long said, "it would be of no service to the public to be informed." The reasons had in part to do with his interest in music, as a letter from Cambridge noted: "Dr. Long advances, but slowly, in his astronomical work; tho' ye larger part of his 2d vol. is I believe printed. But he keeps amusing himself [...] with alterations in musical instruments, of wch he is very fond." J. Green to Thomas Birch, 29 Jan. 1760, BL Add Mss 4308, ff. 192–193. Only in 1784, after Long's death, was the remaining part of the book published. Long (1742, 1764, 1784, 1:ix–x, and 2:iii, 637–638). "Long, Roger," *DNB*, 1st ed. 12:109. Rouse Ball (1889, 105). Gunther (1937, 164–167). Ketton-Cremer (1955, 83–84). "Dunthorne, Richard," *DNB*, 1st ed. 6:235–236.

[82] Long (1742, 1764, 1784, 2:717–718).

[83] Thomas Rutherforth (1748). "Rutherforth, Thomas," *DNB*, 1st ed. 17:499–500. Rutherforth used his membership in the Royal Society to promote sales of his books. Thomas Rutherforth to Thomas Birch, 30 Jan. and 6 Feb. 1743/42, BL Add Mss 4317, ff. 305–306, 308.

rudimentary knowledge of quantity.[84] His book was competent at the level of its intended audience, and popular. Its list of subscribers numbered about 1000, of whom roughly a third were identified with Cambridge.[85] Charles Cavendish did not subscribe to it. When Cavendish was a student, the Jacksonian Professorship of Natural Philosophy had not yet been established.

The Woodwardian Professor of Geology Charles Mason was a good geologist, who had charge of an important collection of fossils in Cambridge. He had scientific interests outside of geology as well, described as "a man of curious knowledge in the philosophy of mechanics and a deep mathematician."[86] It is conceivable that he contributed to Cavendish's education, but it is unlikely. Cavendish did take up geology, but it was long after he left Cambridge. The professorship of chemistry was held by John Mickleburgh, who like his predecessor Vigani was an advocate of Newtonian chemistry. Mickleburgh took his teaching seriously, excusing his delay in answering letters on the grounds that because he was "now engaged in a course of Chemistry here, I can think of nothing but calcinations, sublimations, distillations, precipitations, etc.," but by Cavendish's time he evidently no longer lectured on the subject, and to our knowledge neither did anyone else until after Cavendish.[87]

Before leaving the subject of Cambridge's potential contribution to Cavendish's scientific education, we need to look at textbooks in use there that were written by authors who were not Cambridge professors. After becoming Lucasian Professor of Mathematics, John Colson translated into English several books from several languages, one of which was Petrus van Musschenbroek's *Elements of Natural Philosophy*, subtitled *Chiefly Intended for the Use of Students in Universities*.[88] Colson explained that there was need for a complete "system" of natural philosophy in English and that Musschenbroek's was the best. For his system, Musschenbroek drew on Continental sources such as writings by Descartes and Leibniz (concerning whose use of vis viva for force Colson disagreed with), but his principal source was the "very many and great discoveries of the illustrious *Newton* (the glory of *England*, to whom no age has produced an equal)." He thought that mathematics was the right preparation for natural philosophy, in agreement with Newton and the curriculum at Cambridge. Although physics had been placed on a "firm basis" through observation and experiment, there were always problems to solve, he said, and if we are unable to solve them, we can "excite other diligent inquiries into nature, that are to come after us." That most puzzling of fields electricity would grant "eternal fame" to its genius, whose name would be struck on public monuments; as if to confirm his prophecy, in the year after the publication of Colson's translation, Musschenbroek himself made an important discovery in electricity,

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[84] Rutherforth (1748, 23).

[85] Robert E. Schofield (1970, 97).

[86] Wordsworth (1968, 345). Indicative of Mason's range of interests are "hints" about melting iron and about a burning well in a letter he sent to the president of the Royal Society at about the time Cavendish was in Cambridge: Charles Mason to Martin Folkes, 22 Jan. 1747/46, Wellcome Institute, Martin Folkes Papers, Ms. 5403. Winstanley (1935, 168–169).

[87] John Mickleburgh to Dean Moss in 1725, in John Nichols (1817–1858, 4:520). Wordsworth (1968, 188–189). L.J.M. Coleby (1952b, 167, 169–170).

[88] John Colson translated Petrus van Musschenbroek, *Elements of Natural Philosophy* from the Latin in 1744; from the French he translated Jean Antoine Nollet, *Lectures in Experimental Philosophy* in 1748; from the Italian he translated Maria Gaetana Agnesi, *Analytical Institutions* in 1801. We have already discussed his translation from the Latin of Newton's *Method of Fluxions*.

the Leiden jar. Like Colson, he gave encouragement to aspiring students, assuring them that natural philosophy “can never be exhausted.”[89]

Colson would have recognized a kindred spirit in Musschenbroek, who at the time of Colson’s translation was professor of mathematics and astronomy at the University of Leiden, and whose main publications were extensions of his lectures in ever larger books. His predecessor at Leiden had been Willem Jacob ’sGravesande, another systematizer and author of textbooks, whose *Mathematical Elements of Natural Philosophy, Confirmed by Experiments: or, an Introduction to Sir Isaac Newton’s Philosophy* had been translated from the Latin into English by J.T. Desaguliers in 1720–21. His strength as a teacher lay in his use of experiments to support scientific truths, but like Musschenbroek he recognized the importance of mathematics for natural philosophy. The “comparing of motion” was the “continual theme of natural philosophy,” and anyone who went about that subject in “any other way, than by mathematical Demonstrations, will be sure to fall into Uncertainties at least, if not into Errors.” Newton had demonstrated in the *Principia* the “great use of mathematics in Physics, as no one before him ever penetrated so deeply into the Secrets of Nature.” Musschenbroek and ’sGravesande had studied at the University of Leiden when its most successful teacher Herman Boerhaave was lecturing; through their teaching, which included their textbooks, the three professors made Leiden the center of Newtonianism on the Continent. The experimental philosophy had replaced stable certainty with change, they said, and they encouraged their students to discover new truths using the experimental way aided by mathematical demonstration.[90]

Leiden was probably a better place to learn natural philosophy than Cambridge, but it was not necessary to be in Leiden to learn from it. Colson’s translation of Musschenbroek’s textbook and translations of ’sGravesande’s and Boerhaave’s textbooks were recommended reading in Cambridge, and they strongly influenced texts written by British writers, just as theirs were influenced by British texts. In presenting natural philosophy, ’sGravesande followed the “Example of the *English*,” by giving experiments that had “a kind of Connexion with one another”; Musschenbroek, in his presentation of optics, said that Robert Smith’s *Opticks* “has gone beyond all the rest in this science.”[91] At both universities the emphasis was on Newtonian philosophy, and at both universities the professors were primarily teachers not researchers. For a wide and perceptive reader like Cavendish, the experimental emphasis at Leiden would have supplemented the mathematical emphasis at Cambridge, and there would have been no contradiction.

Leiden’s authors would have exposed Cavendish to points of view not found in English texts on natural philosophy. If in his time as a student in Cambridge, Cavendish read Musschenbroek’s text or ’sGravesande’s text he saw how vis viva could be incorporated in otherwise largely familiar presentations of natural philosophy. It was in this particular that Cavendish’s use of mechanics differed from that of his British colleagues.[92]

In broad outline, we have sketched the scientific tradition at Cambridge insofar as it was represented by the texts of its early and mid-eighteenth-century professors. When Cavendish entered the ranks of scientific researchers, he was familiar with mathematical methods and

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[89] Musschenbroek (1744, 1:iii–v); Colson’s advertisement, xi, 6. The Leiden jar was discovered independently by the German experimenter E.G. von Kleist.

[90] Edward G. Ruestow (1973, 7–8, 115–121, 135–139).

[91] Struik (1974). Hall (1972). ’sGravesande (1747, 1:ix, xv). Musschenbroek (1744, 2:159).

[92] Musschenbroek (1744, 1:80–82). Willem Jacob ’sGravesande (1741, 76).

concepts of science within a certain Newtonian framework, and the connections with his Cambridge education are significant and unlikely to be only coincidence.

Giardini Academy

If there was an early musical influence on Henry Cavendish, it came from his mother's side of the family. The duke and first duchess of Kent had a love of music, the duke managing to combine music with his political career when as lord chamberlain he worked to bring Italian opera to London. Later, in 1719, he was one of the original subscribers to the Royal Academy of Music, and he (but not the duke of Devonshire) became one of its twenty directors. There is a painting showing the Kent family being musically entertained (Fig. 1.3). We know that the Greys and the Yorkes attended concerts at the Rotunda.[93] Had Henry shown a musical interest, he would have been encouraged.

Evidence of Henry Cavendish's interest in music is sketchy. There is a mathematical study by him, "On Musical Intervals."[94] There is a reference to a musical event in Cavendish's laboratory notes on chemistry: in 1782 he used his eudiometer, an instrument for measuring the "goodness" of air, to compare the good air of Hampstead, to which he had just moved, to the used "Air from Oratorio."[95] He began his lament on the death of the prince of Wales with music: "Melpomene [goddess of song], pour forth a gloomy anguish on our melodies/ Let the flute breathe out faint wailings/ And sing out a grievous tune in solemn funeral procession." More significant, a grand pianoforte is listed in the auction catalog of the contents of his house at Clapham Common at the time of his death.[96] Other than for servants, Cavendish was the only person who lived in the house, and the pianoforte would have been there only because he wanted it. According to a story, which on the face of it is unlikely but which may contain a core of truth, Cavendish came together with Michell, Herschel, Priestley, and others over musical entertainment.[97] We know that Michell, Herschel, and Priestley were accomplished in music.

We suspect that Cavendish's education included education in music. Given the limited evidence, in this discussion we proceed tentatively. The professional musician Charles Burney, Cavendish's contemporary and fellow of the Royal Society, said that music and other arts are "governed by laws," and in mastering them the individual approached nearer perfection by receiving help from others than by the "mere efforts of his own labour and genius."[98] The name Henry Cavendish appears on a list of subscribers to the musical academy of Felice Giardini.[99] The name does not prove he was our subject—Sir Henry Cavendish, a

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[93] Otto Erich Deutsch (1974, 91, 102). A.E. Gunther (1984, 62). Great Britain, Historical Manuscripts Commission (1920, 93, 227; 1923, 2:30).

[94] Henry Cavendish, "On Musical Intervals," Cavendish Mss VI(a), 28.

[95] This entry is unclear as to Cavendish's part. It begins with a comparison of "air caught by [the instrument maker Edward] Nairne in 2d gallery of Drury Lane playhouse Mar. 15, 1782 with air of Hampstead of Mar. 16." It follows with "Air from Oratorio about the same time." "Experiments on Air," Cavendish Mss II, 5:189.

[96] *A Catalog of an Assortment of Modern Household Furniture* […] *the Genuine Property of a Professional Gentleman; Which Will Be Sold by Auction by Mr. Squibb, at His Great Room Saville Passage, Saville Row, on Wednesday, December 5, 1810, and Two Following Days, at Twelve O'Clock.* Item 45 is a grand pianoforte, by Longman and Broderip, in a mahogany case.

[97] "Michell, John," *DNB* 1st ed. 13:333–334, on 333.

[98] Charles Burney (1799, 186, 205).

[99] Great Britain, Historical Manuscripts Commission (1913, 188–189).

distant Irish relative of the same age, was in London around this time[100]—but the evidence, sketchy as it is, points to him. Giardini arrived in London in 1751, while Cavendish was at Cambridge, and for ten years beginning in 1755 he adapted Italian operas for the King's Theatre in London. Later he composed concertos and other music for strings, several operas, and a successful English oratorio. Like Charles Cavendish, Giardini was a governor of the Foundling Hospital, where Handel gave concerts, and where Giardini proposed establishing a musical academy. He gave frequent solo performances under the auspices of his good friend J.C. Bach. By the time Cavendish was (if our supposition is right) in contact with him, Giardini was the preeminent violinist in London. Samuel Johnson sympathized with Giardini when he learned that the man did not make more than £700 a year despite his superior ability.[101] To do even this well, which to be sure made him modestly well off, Giardini had to combine activities, one being to run an academy by subscription. In 1758 or 1759, Henry Cavendish along with sixteen others agreed to continue to meet as an "academy" in the coming year as they had in the last, only under new terms, probably having to do with Giardini's finances. The members of the academy agreed to pay £8, half up front and the rest when the academy had met twenty times, the total number of meetings being sixty. It was left to the subscribers whether they would meet in the morning or the evening; if in the morning, as they had been meeting, breakfast would be provided; if in the evening, lighting. Thirteen of the seventeen, including Cavendish, had already paid their advance, and if all paid up, Giardini would have earned around £135, less out-of-pocket expenses, a good installment on his £700 for the year.

The subscribers were young and of both sexes, including husbands and wives and persons with various family connections; two of them, George Manners and Lady Granby, were related to Cavendish. Isabella Carlisle and Frances Pelham were talented singers, who arranged private concerts and may have been pupils of Giardini's. William Hamilton, a colleague of Cavendish's, who began taking lessons with Giardini in the year the Italian arrived in London, was an expert violinist, one of the rare amateur musical gentlemen who could compare in skill with amateur musical ladies.[102] Hamilton's first wife, Catherine, who performed with approval before Mozart, was also one of the subscribers to Giardini's academy.[103] Remembered as the husband of Lord Nelson's mistress Emma, Hamilton was known in his day as a solid diplomat, a learned antiquarian, and a good student of volcanoes. As envoy to the court of Naples, he leased a villa close to his favorite volcano, arranging a music room that Catherine described as "right facing Vesuvius, which now and then is kind enough to play whilst I too am playing." The other night, she said, it had sent up fiery red stones, "but we went right on playing, just as you would have done if you heard a pop-gun in the street." The president of the Royal Society Joseph Banks wrote to Hamilton in Naples to complement him on his description of a recent irruption of Vesuvius: "Cavendish in particular who you know is [given] not at all to flattery says it is a very valuable addition to the theory of volcanoes & that tho he does not on any account wish to derogate from the merit

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[100] He descended from an illegitimate branch of Henry Cavendish's family. An English and Irish politician, he is best known as a parliamentary diarist. Peter D.G. Thomas (2004).

[101] R.H. Nichols and F.A. Wray (1935, 247). Roger Fiske (1973, 284–286).

[102] Brian Fothergill (1969, 29). Horace Walpole to George Montagu, 17 May 1763, in Lewis and Brown (1941, 69–74, on 73). Horace Walpole to William Cole, 5 Feb. 1780, in Lewis and Wallace (1937, 186–189, on 187).

[103] Hamilton has helped us date the agreement between Giardini and the subscribers to his academy. By our reckoning, it was after Hamilton's marriage in 1758 and before December 1759.

of your former papers this is certainly the most valuable one we have receivd from you."[104] What exactly transpired if Hamilton and Cavendish came together in Giardini's academy is unclear, but it undoubtedly had to do with listening together, and very likely it involved performing together.

Giardini, Burney wrote in his history of music, "formed a morning *accademia*, or concert, at his house, composed chiefly of his scholars, vocal and instrumental, who bore a part in the performance." This we take to be a description of the academy to which our Cavendish may have subscribed. He may have been one of Giardini's "scholars" too, and he may have performed before an audience at the academy. It is hard to imagine the shy and taciturn Cavendish singing or performing on an instrument, but stutterers have been known to be great orators, and playing to his strength Cavendish "performed" experiments before competent audiences using scientific instruments.[105]

If Cavendish pursued an advanced education in music, there are reasons why he might have chosen to do so with Giardini. First, Giardini was a highly regarded teacher: in Thomas Mortimer's *The Universal Director* of 1763, he was listed not as a violinist but as a teacher of singing and harpsichord. Second, with Giardini's arrival in London, the "standards" of London concerts rose, coming to equal those of the best in Europe. Third, he eliminated from performances all possible extraneous ornaments, among other changes. We find parallels in Cavendish's scientific and life preferences.[106]

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[104] Joseph Banks to William Hamilton, 30 Nov. 1794, BL, Edgerton 2641, 155–156.

[105] In Italy a private concert by dilettantes was called an "accademia," which may have been Giardini's meaning. This information is from a work of the time, Charles Burney (1771), quoted in Walpole (1954, 18:13, note 16a). Charles Burney (1789/1935, 1012–1014). Stanley Sadie (1988, 320).

[106] Simon McVeigh (1993, 14, 197, 220).

Chapter 7
Science

Henry Cavendish's family is said to have been greatly disappointed that he did not pursue a regular public career, and that his father accordingly treated him in a niggardly fashion.[1] The first half of the statement is plausible, since the Cavendishes were a political family and naturally had expectations. This was a time, we must remember, when sons of peers and even sons of sons were practically duty-bound to enter the House of Commons.[2] To appreciate how extraordinary Henry's career as an unsalaried natural philosopher might appear, consider that in the same year that he entered the Royal Society, the House of Commons had four Cavendish's, five Manners, and five Townsends, and, in general, an ample representation of aristocratic young blood. The allegation, however, that Charles Cavendish was one of the family members who disapproved of Henry's course in life runs up against certain known facts, chief among them is that he brought his son into his scientific circle from an early age. As to the charge of niggardliness, we have little to go on. Since Henry did not marry, there is no settlement in writing, and we have not found any written agreement between father and son. According to one source, until he was forty Henry received an annuity of only £120, which was modest, though by living at home he could have got along fine. The chemist Thomas Thomson said that Henry's annuity was £500, which was handsome,[3] the same as the annuity Charles received from his father at the time of his marriage; before then, he had received only the standard £300. Charles was not wealthy and he was careful with money, and he may even have been tight, but it seems unlikely that he would have punished his son for following his example. He left politics for what we take to have been for him a more fulfilling life. Bypassing politics entirely, Henry took up science, which provided him with a life that suited him. There is no reason to think that his father tried to disuade him, but on the contrary, there is every reason to think that his father instructed him in science and supported him completely.

By foregoing a career in politics, Henry Cavendish deprived his family of a reliable vote in Parliament for a number of years, but by then his vote was dispensable. What was enduring in the family tradition was a commitment to public service, and nothing in the record suggests that he deliberately defied his relatives by his choice of ends to serve. If he experienced any conflict as a result of being both a Cavendish and a servant of science, it was not obvious to people who knew him. The basic agreement between his view of British government and his family's is evident in the part he took in the politics of the Royal Society, discussed later.

With his way of life, Cavendish brought together the two main reference points of his identity, his rank and his work: in the organizations where he performed his duty of

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[1] George Wilson (1851, 161).
[2] L.B. Namier (1929, 5).
[3] Thomas Thomson (1830–1831, 1:336). Wilson (1851, 160).

service, he was welcomed as a natural philosopher bringing useful knowledge, skill, and intelligence. The English aristocracy was in ascendancy in the social world, and during his lifetime its postion was not seriously threatened; and in the century after the Scientific Revolution, which had exhibited the power of experiment, observation, and mathematics to build solid structures of knowledge, natural philosophy was in ascendancy in the world of learning. In his time, Cavendish was enviably placed in English life.

Introduction to Scientific Society

In the summer of 1753, soon after leaving Cambridge, Henry together with his brother Frederick accompanied their father to William Heberden's house for dinner. A number of friends and colleagues of their father were invited that evening: Thomas Birch, William Watson, Daniel Wray, Nicolas Mann, and the physician and poet Mark Akenside, whom Charles Cavendish had recommended for fellowship in the Royal Society for his knowledge of natural philosophy.[4] Heberden and the first three men in this list were to sign the certificate for Henry's membership in the Royal Society. Frederick, who suffered a serious accident the following year, did not come to any more of these collegial dinners, but Henry came with his father to at least twenty-six of them. The most frequent of Henry's hosts was Heberden, though the dinners were sometimes held at Yorke's house and occasionally at Watson's, Stanhope's, Wray's, and his father's houses.[5]

Fellows of the Royal Society commonly introduced their sons to other members by bringing them as guests to the meetings.[6] Charles Cavendish first brought Henry on 15 June 1758, by which time he had already introduced him to many of the active fellows of the Royal Society at dinners at his and his friends' houses. As his father's guest, Henry came to a total of seventeen meetings of the Royal Society, and at three more meetings he came as a guest of Birch, a friend of the family, of Peter Newcome, the teacher at Henry's school at Hackney, and of Michael Lort, who had connections with the family.[7] The year before Henry began coming to the meetings, Charles had received the Copley Medal of the Society, and as vice president he presided over almost half of the meetings to which he brought Henry as his guest. Henry could feel reassured in this new public world of science.

On 31 January 1760, Henry Cavendish was proposed for fellowship in the Royal Society by Lord Willoughby, Lord Macclesfield, and James Bradley, an appropriate combination of rank and skill. Over the next three months, the certificate recommending Cavendish for fellowship, which was drafted by Heberden, was signed by six more fellows: Birch, Wray, Watson, Thomas Wilbraham, John Hadley, and Samuel Squire. All of them were members of Charles's dining circle, with whom Henry too had dined. Henry was balloted and unani-

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[4] 25 Aug. 1753, Thomas Birch Diary, BL Add Mss 4478C, f. 235.

[5] Henry came with his father to dinner at Heberden's twelve times. Our knowledge of this dinner and others like them comes from Thomas Birch's Diary, and so we know only about those social occasions at which Birch was present.

[6] Examples from about this time: John Canton, Jr., was a guest of John Canton, and Jonathan Watson, Jr., was a guest of Jonathan Watson. Entries for 26 Mar. and 9 July 1767, JB, Royal Society 26.

[7] Entries in JB, Royal Society 23 (1757–60). Michael Lort was an antiquarian, who in 1759 was appointed professor of Greek at Cambridge. Since he was not yet himself a fellow of the Royal Society, he must have had the right to invite guests as a university professor. Lort was a good friend of the Cavendish in-law Philip Yorke, and he is said to have been librarian to the duke of Devonshire.

mously elected on 1 May 1760.[8] What the certificate said was that Cavendish had "a great regard for Natural Knowledge" and that he was "studious of its improvement." General though the description was, it was of a kind often given,[9] and in Henry Cavendish's case the generality was justified, as he would become known as a universal natural philosopher.

Just as at the Royal Society, at the Royal Society Club—the official name was still the Society of Royal Philosophers, changing only in 1794—prospective members were customarily brought as guests before they were elected members. This was the case with Henry Cavendish, though he was proposed for membership before he had actually attended a dinner of the Club. On 10 November 1757, Macclesfield, who as president of the Royal Society presided over the dinner, recommended Henry Cavendish for membership. This was no doubt by prearrangement, as Charles Cavendish attended that dinner. Around this time, the most active members of the Club—as indicated by their attendance at the yearly business meetings and a few special meetings and by their attendance at ordinary dinners—were members of Charles Cavendish's dining circle, which Henry Cavendish had lately joined: Watson, Knight, Squire, Wray, Birch, Colebrook, and also Burrow. Others who came frequently to the Club's dinners were also dining companions of Charles's; in particular, Willoughby, Newcome, and Akenside.[10]

Candidates for membership in the Club were not always elected. For example, at an annual anniversary meeting of the Club, there were seven candidates, two of whom were chosen unanimously, one of them the astronomer William Herschel. The others had various numbers of "black balls" against them, as reported in a letter from the president of the Club.[11] Henry would face no opposition, but he had to wait until there was a vacancy before he could be balloted. The wait, it turned out was considerable, two and a half years, though it was a formality readily circumvented. He was invited to dinners as a guest of his father's four times in 1758 and two times the following year, treated as if he were a member from the time of his proposal. As it happened, the timing was right, for he was elected member of the Club on 31 July 1760, just two months after he was elected to the Royal Society.[12] Henry was then twenty-eight; his father did not attend dinners at the Club regularly anymore, so Henry came mostly on his own.

We join Henry at his first dinner as a member, on 14 August 1760, at the Mitre Tavern on Fleet Street. He paid his admission fee of one pound one shilling together with three shillings for the dinner that day. He sat down at four o'clock before the following choices: nine dishes of meat, poultry, and fish, two fruit pies, plum pudding, butter and cheese, and wine, Porter, or lemonade.[13] A foreign guest left the one detailed description of a dinner of the Club in the eighteenth century, held on 12 August 1784, at which Cavendish was present. The members sat down to dinner at 5 PM, breaking off at 7:30 PM in time for the Royal Society meeting at 8 PM. The president of the Royal Society Joseph Banks presided over the dinner, and the astronomer royal the Reverend Nevil Maskelyne gave a short prayer. The guest noticed the quantity of alcohol that was drunk during and after the dinner, selected from a wide

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[8] 1 May 1760, JB, Royal Society 23:845.

[9] Certificates, Royal Society 2:198 (proposed 31 Jan. 1760). Maurice Crosland (1983, 173–174).

[10] Minute Book of the Royal Society Club, Oct. 27, 1743–June 29, 1809, Royal Society, 1.

[11] Joseph Banks to Charles Blagden, 28 July 1785, Blagden Letters, Royal Society, B.35.

[12] Archibald Geikie (1917, 63, 70). At the beginning of Minute Book 4, covering the years 1760–64, it says that everyone is charged for a pint of wine, and that for those who preferred lemonade and porter, their value was reckoned as equal to that of a bottle of wine.

[13] 14 Aug. 1760, Minute Book of the Royal Society Club, Royal Society, 4.

menu: beer, port, madeira, claret, champagne, brandy, rum, and other strong liquors. It was the prince of Wales's birthday, and the Elector Palatine was admitted that day to the Royal Society, and they and each member and each guest received a toast, each calling for wine. According to the guest, by the time they left, they "were all pretty much enlivened," though their "gaiety was decorous."[14] At meetings of the Club, between eating well and drinking, members and guests talked about scientific news and sometimes performed experiments.[15]

The Club met every Thursday throughout the year. In his first year, Cavendish came to sixteen dinners, the next year twenty-eight, and eventually he came to nearly all of them. From 1770 on, he attended no fewer than forty-four dinners in a year, and usually around fifty. A dozen or so members and guests made up a typical dinner party, but there was considerable fluctuation. Cavendish's regularity is indicated by the following events. In 1767, on a day in which the meeting room of the Club was appropriated by the Society of Antiquaries, another arrangement was made, and only one member of the Club turned up for it: he was Cavendish, who brought with him as a guest Nevil Maskelyne. In 1777 the treasurer made an error in scheduling a dinner on Christmas, but Cavendish came anyway, along with two others.[16] Cavendish was the most constant attender of all the persons who had ever belonged to the Club,[17] qualifying Wilson's conclusion that Cavendish was "one of the most ungregarious of beings."

Wilson learned from his sources that Cavendish was interested only in science. That would seem to be largely borne out, though it is incomplete. Geikie in his history of the Club recognized that Cavendish had wider interests than the laboratory, as shown by his guests, who included physicians, surgeons, politicians, manufactures, engineers, explorers, seamen, and still other types.[18] Examples are John Belchier, surgeon of Guy's Hospital, Paul Joddrell, who became a physician in India, William Ogilvie, professor of humanity at the University of Aberdeen, and Henry Penruddock, former mayor of Salisbury and sheriff of Wiltshire who was interested in antiquities and topography. Some persons he brought as guests were candidates for membership in the Club, in which event he may have been performing a duty, but usually this was not the reason. He did more than attend dinners: in addition to bringing guests, he presided over an annual general meeting in the absence of the president at least once,[19] and he made gifts of fish and venison.[20]

In 1780 the meetings of the Club were moved to the Crown & Anchor Tavern on the Strand, closer to the new location of the Royal Society in Somerset House. If Cavendish had an interest in music, he might have been familiar with the Crown & Anchor: this tavern with its great ballroom had long been the site of the fortnightly concerts of the Academy of Ancient Music, as it would continue to be until 1784, combining excellent music with food and drink.[21]

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[14] Geikie (1917, 169–171).

[15] Joseph Banks to Charles Blagden, 28 Sep. 1782, Blagden Letters, Royal Society, B10.

[16] Geikie (1917, 73–74, 80, 95, 97). Hector Charles Cameron (1952, 172).

[17] As of the time of Geikie's book, *Royal Society Club*, 73.

[18] Geikie (1917, 147, 154, 202, 234).

[19] 25 July 1782, as recorded in the Minute Book of the Royal Society Club, 7.

[20] 4 Apr. 1782, 25 Aug. 1785, Minute Book of the Royal Society Club, Royal Society, 7.

[21] Robert Elkin (1955, 51–52).

In 1760, the same year that he was elected to the Royal Society and the Royal Society Club, Cavendish was elected to the Society of Arts.[22] His father had again preceded him, having been elected three years before. The Society had been in existence for six years, and its membership was growing rapidly; at any one meeting, twenty to fifty persons might be elected. From a handful of founders, the membership stood at nearly 2000 by 1768.[23] The subscription was two guineas, or three for persons who could afford it, and five guineas were expected of peers, of whom there were many; the duke of Devonshire was elected the year after Cavendish. The membership was a cross-section of English society: mechanics, iron masters, watchmakers, opticians, glass manufacturers, wine merchants, portrait painters, writers, politicians, and a good many prominent fellows of the Royal Society, including present and future presidents of the Royal Society Sir John Pringle and Lords Macclesfield and Morton. Active in committees of the Society around the time of Cavendish's election were John Hadley, Gowin Knight, William Watson, Benjamin Franklin, Henry Baker, Matthew Maty, Lord Willoughby, and William Heberden.

Cavendish held no office in the Society of Arts, he did not publish in its journal, and it seems he did not belong to any of its committees. In 1786 he was summoned to attend the committee of polite arts to take part in an educational experiment, but he did not go.[24] It is conceivable that he attended the weekly general meetings, but there is no way of knowing this,[25] and it seems unlikely. If his membership was passive, this does not mean that he was uninterested, for he kept up his membership for fifty years, to the end of his life. We know that he was interested in many of the subjects that came up in the Society. There was probably more to his patronage of the Society than performing a duty.

The idea of the Society of Arts at its inception was that industry would be stimulated by prizes donated by interested parties. To this end, six main committees were set up, at least two of which were of interest to Cavendish, those for chemistry and mechanics. Historians of the Society find that the competitions stimulated the early stages of the industrial and agricultural revolutions, especially the latter. In industry the Society's main concern was mechanical inventions, having to do with, for example, water and steam power, measuring instruments, and standards of measurements; it was also concerned with chemicals used in industry, including the chemical processes of smelting and refining iron ore. These industrial subjects interested Cavendish, as we learn from the journeys he made, which come up later in this book. As an example, the Society awarded a gold medal to Abraham Darby III for building the first iron bridge, at Colebrookdale, which Cavendish visited on one of his journeys. In 1783, the Society began its own regular publication, *Transactions*, the first issue of which announced a gold medal for a method of burning smoke from steam engines and smelting furnaces; on a journey Cavendish took an interest in Watt's invention of a furnace for burning smoke. There is evidence that in the 1770s, leading members of the Society of Arts who were also fellows of the Royal Society agreed that the former would deal mainly

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[22] On 9 January 1760, Henry Cavendish was proposed for membership by Mr. Cosheap; at the next meeting, on 16 January, he was elected. Minutes of the Society, Society of Arts, 4.

[23] *A List of the Society for the Encouragement of Arts, Manufactures, and Commerce.* 6 April 1768. Printed by order of the Society.

[24] D.G.C. Allan, personal communication, 1966, and *Journal of the Royal Society of Arts*, 1966, 1033, n. 11.

[25] After 14 Dec. 1757, the Society Minutes stopped recording names of members present at meetings.

with applications of science and the latter mainly with basic science.[26] Cavendish's original work belongs to basic science, but he was interested in applications too.

Science at the Royal Society

In Cavendish's time, scientific books were written for a variety of purposes and readers; for example, to educate students, to present the state of a field for researchers, to simplify a field for lay readers, to serve as practical manuals, to bring out new research or interpretations, to bring together previously published papers, and to make money. For example, Robert Smith wrote a textbook on optics, Colin Maclaurin wrote a book popularizing Newtonian science, and John Michell wrote a manual on making artificial magnets. Cavendish would have been expected to publish at least one book over the course of his life. He began a book on mechanics, and he nearly completed one on electricity.

As it turned out, like a few of his colleagues, notably William Herschel and John Canton, Cavendish published only papers, which appeared in only one place, a journal for all of the sciences, the century-old *Philosophical Transactions* of the Royal Society. His fields, experimental and mathematical natural philosophy, were not the journal's strengths—only ten percent of the papers it printed were experimental, and a much smaller proportion were theoretical[27]—but we have no reason to think he was dissatisfied with the journal for that reason. The journal was one of the activities of the Royal Society, to which he was committed. The era of scientific specialization with specialized journals began only toward the end of his life.

At Cambridge, Cavendish studied the mathematical methods of natural philosophy. He learned about scientific research elsewhere, presumably at home under his father's guidance, using his father's instruments and reading his father's books and journals. His primer, the *Philosophical Transactions*, came regularly into his father's house during the years he was a student. Beginning in the year he came home from Cambridge for good, his father served on the Royal Society committee of papers, passing judgment on every paper appearing in its journal. As we have with textbooks in use at Cambridge, we examine the *Philosophical Transactions* as a source of examples of how to proceed as a scientific researcher and author.

With one exception, the important papers Cavendish wrote for the *Philosophical Transactions* were experimental. In the previous century, when the journal began, the meaning of "experiment" could be as general as "any made or done thing"; the goal of experiment then was usually to discover something or to solve a debate, and the argument it supported was usually inductive. By the time Cavendish entered science, the meaning of experiment had narrowed; it was usually undertaken to solve a problem or to prove a hypothesis or a theory. Before Cavendish was through, experiment was undertaken to establish or test a general claim. On the way, experimental papers grew longer and more argumentative, corroborative, and investigative.[28]

In reporting the results of scientific work, the Royal Society's strictures against fanciful language were expected to be honored. In an exchange of letters in the *Philosophical*

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[26] The competitions were extensive; for example, in 1764 there were 380 classes, and the premium list took up 91 pages. *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. 1, 1783. Derek Hudson and Kenneth W. Luckhurst (1954, 6, 15, 57–58, 101, 113–116, 119, 124–125).

[27] Richard Sorrenson (1996, 39–40).

[28] Charles Bazerman (1988, 66–68).

Transactions, the electrical experimenter Georg Matthias Bose conceded that by his "style and expressions" he had "embellished a little" the account of an experiment. His correspondent William Watson took him to task: "The language of philosophers should not be tainted with the license of the poets; their aim in the communicating their discoveries to the world, should be simple truth without desiring to exaggerate." Nature, the thing itself, was cause enough for "admiration."[29] Spare writing can have a force of its own, even eloquence. Cavendish's writing has that quality, and because his writing was the same whether the subject was phlogiston or farming, his adherence to the Royal Society's strictures would seem to have come naturally to him, as an extension of his personality. Few wrote as plainly as Cavendish; Bose was not unique, only chastened.

Most papers in the *Philosophical Transactions* appeared in English, the language in which they were written, though papers in Latin from abroad were not uncommon and were rarely translated, a reflection of British education and of the continuing use of Latin as a universal language of scholars. Papers in French, Spanish, and other modern European languages were translated, again reflecting British education and also British insularity.[30] Later in the century, the Council of the Society resolved to meet foreigners halfway, ordering that papers communicated in foreign languages be printed in the original language in small type at the bottom of the page containing the English translation. In a further step in this direction, English translations might be relegated to an appendix and, on occasion, omitted.[31] Fortunately, there were always fellows who were willing and able to translate, and like most readers of the journal, Cavendish was often in their debt.

Authors in the *Philosophical Transactions* were identified. At the head of his papers in the journal, Cavendishs name appeared together with his rank and affiliation, "Hon. Henry Cavendish, F.R.S." As the later president of the Society Joseph Banks explained to a contributor, by the "name" of an author the Society did not mean a "bare signature but such additions local and professional as may lead any one of us at once to a knowledge of the person intended by it."[32] The "additions" did not include terms like "botanist." Readers of a botanical paper would draw their own conclusion about the author's scientific field. In the body of their papers, authors sometimes referred to one another by specialized terms such as "botanist," "chemist," and "electrician," at other times by broad terms. A person who studied minerals might be called a "natural historian" or "naturalist," terms which also applied to a person interested in, say, stones from a rhinoceros's stomach. Someone who studied nature scientifically was a "philosopher," a term which was often qualified: Cavendish was called a "natural philosopher."[33]

Newton was the Royal Society's illustrious president forever. Over the course of Charles and Henry Cavendish's memberships, the Society elected seven presidents, none of whom remotely approached Newton in scientific stature, and in the case of several, the scientific accomplishment was negligible. As a point of honor the Royal Society was quick to defend its standard-bearer from criticisms perceived as partisan, but there was a subtle change. When Charles Cavendish entered the Royal Society, references to Newton in the

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[29] William Watson (1750, 355–356).

[30] An exception was a letter sent to the instrument maker James Short, translated from the Latin: Joseph Steplin (1755).

[31] 20 May 1773, Minutes of Council, Royal Society, 6. In 1780, a paper in Swedish by Carl Peter Thunberg and one in Italian by Felice Fontana were printed in the body of the journal, their English translations in an appendix.

[32] Draft letter by Joseph Banks, 28 Dec. 1791, Banks Correspondence, Royal Botanic Gardens, Kew.

[33] Here and there; e.g., *PT* 46 (1750): 118, 362, 589.

Philosophical Transactions were generally to praise. Twenty years later, when his son Henry was at college, references to Newton were still to praise and were always respectful, but they tended to be tempered and occasionally were critical. Halley in his ode prefixed to the *Principia* wrote of Newton's "own divinity," of a thinker "nearer to the gods no mortal may approach"; to Henry Cavendish and his contemporaries Newton was definitely mortal, capable of occasional error and in need of correction. Thomas Simpson, mathematics teacher at the Royal Military Academy at Woolwich and the principal contributor of mathematics to the *Philosophical Transactions* at this time, solved a problem in inverse fluxions (integration) conscious that his solution differed from Newton's, acknowledging that it was "impossible to disagree without being under some apprehensions of a mistake."[34] Concerning the precession of the equinoxes Cavendish wrote in a letter, "As well as I remember Newton as you said really made a mistake from not considering this."[35]

If foreigners pointed out Newton's mistakes, it was in their interest to be certain. An Italian who claimed to have discovered six errors in Newton's *Principia* was answered by the home guard.[36] The French astronomer Alexis Claude Clairaut maintained that Newton's inverse-square law of gravitation was inexact. Having detected an absurdity in Clairaut's reasoning, the astronomer and fellow of the Royal Society Patrick Murdoch wrote a paper to dispel the erroneous view that Newton's propositions on the motions of the moon were "mere mathematical fictions, not applicable to nature"; on the contrary, Newton's work was "fully confirmed and verified."[37] Clairaut wrote a kind of apology for the *Philosophical Transactions*, saying that he had not intended to disparage Newton. Newton had not thought it impossible to be "opposed by experience," but in their zeal some people did not distinguish "between the different ways of opposing that great man's sentiments"; still, if the Royal Society wished, Clairaut would reword his disagreement with Newton.[38] Clairaut changed his mind about the inverse-square law and made a public retraction. His criticism of Newton was turned to praise by the Swiss mathematician Leonhard Euler, who too had once believed that Newton's theory conflicted with observations of the motion of the moon; Clairaut's retracted claim, he said, had not been damaging but on the contrary had given "quite a new lustre to the theory of the great Newton."[39]

Euler did, however, pick a quarrel with Newton on the subject of aberration in refracting telescopes. The imperfection of the image was understood to arise from two sources, the different refrangibility of different colors, and the shape of the eye-glass. The latter was a matter of craft; the former was believed to have no remedy. Newton was cited as the authority for this discouraging conclusion, and though in principle he had not ruled out the possibility of an achromatic lens, he had not succeeded in constructing one and had come to doubt its practicability.[40] Euler believed that Newton was wrong, and he corrected him in letters to the *Philosophical Transactions* containing his prescription for making achromatic refracting telescopes. The English optical instrument maker John Dolland gave the rejoinder this time, deferring to Newton, "that great man," who had proved that it was impossible

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[34] Thomas Simpson (1748, 333).

[35] Henry Cavendish to Nevil Maskelyne, 29 Dec. 1784, draft; in Jungnickel and McCormmach (1999, 600).

[36] James Short (1753a, 14–15).

[37] Patrick Murdoch (1751, 62–63, 74).

[38] Alexis Claude Clairaut (1753, 82–83).

[39] Leonhard Euler (1753).

[40] D.T. Whiteside in Newton (1967–1969, 442–443).

to eliminate that aberration.[41] Dolland would change his mind; his polemic with Euler led him to make new experiments, the results of which differed "very remarkably" from those in Newton's *Opticks*.[42] By combining different kinds of glass, Dolland constructed achromatic lenses, for which bold heterodoxy he was awarded the Copley Medal in 1758. The problem of indistinctness of images in refracting telescopes was not completely solved, and Cavendish would investigate it. Thomas Melvil was more speculative in his disagreement with Newton. He rejected Newton's understanding that the different refrangibilities of light were owing to different sizes or densities of the particles of light of different colors, explaining that Newton had been misled by an "analogy" between the refraction of light and the gravity of bodies; the true cause of different refrangibilities was the different velocities of particles of light of different colors. As this serious challenge to Newton had observational consequences, the Royal Society ordered the instrument maker and astronomer James Short to investigate them and report back; Melvil's hypothesis was found not to hold up.[43] Henry Eeles combined his explanation of the ascent of vapors with a broad criticism of Newton. Defending his "hypothesis" of the fluid of fire against the disapproval of "our great modern philosopher" of the use of hypotheses in general, Eeles observed that Newton's objection to hypotheses appears in a place in his writings that is entirely hypothetical, the queries in his *Opticks*. Even gravitation, he said, would not have occurred to Newton without a hypothesis since a "supposition must always precede the proof"; if a hypothesis is rationally founded, it should be tested, for that is how science advances.[44] In various researches of his, Cavendish confidently spoke of his "hypothesis." Newton at midcentury was still the great Newton, but opinions could be conflicting on his authority on this or that point.

Scientific conclusions had to be supported by facts, but on the question of whether greater trust was to be placed in observation or in theory, the answer was not always observation. James Short set out to clarify the disagreement between the observed shape of the earth and Newton's theoretical prediction of it. Critics of Newton's theory such as Clairaut had erred, Short said, in regarding their observations as absolutely exact (Clairaut denied that he placed too much certainty in observations) whereas other observers such as Roger Joseph Boscovich had erred in thinking that observations were too inexact to draw any conclusions. When theory and observation were compared, theory could not be faulted until the disparity with observation was greater than the errors attributed to the instrument and its user. Newton had a just appreciation of such limits, as shown by his calculation of the ratio of the two diameters of the Earth as 229 to 230, that is, to three figures, not to four or more figures, which would have been a pretense of accuracy. It would be "absurd" for an observer to compute an angle to a second or a length to a part of an inch if the instrument could only measure to a degree or a foot. Mathematical results were rigorously true, but observations had "certain limits," and the error of the instrument was itself one of the "*data*." Short urged observers to follow the "judicious caution" of Newton and to read Cotes's treatise on errors.[45] To "di-

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[41] Under the general heading: "Letters to a Theorem of Mr. Euler… for Correcting the Aberrations in the Object-Glasses of Refracting Telescopes," *PT* 48 (1753:287–96). One letter was by James Short; other letters were Leonhard Euler, "Letters Concerning a Theorem of His, for Correcting the Aberrations in the Object-Glasses of Refracting Telescopes," and John Dolland, "A Letter […] Concerning a Mistake in M. Euler's Theorem for Correcting the Aberrations in the Object-Glasses of Refracting Telescopes."

[42] John Dolland (1758, 736).

[43] Thomas Melvil (1753, 262).

[44] Henry Eeles (1755, 124–125).

[45] Short (1753a, 5–7).

minish the errors arising from the imperfections of instruments, and of the organs of sense," the mathematician Thomas Simpson proved that it was better to make many observations than only a few and that by taking a mean of them, the chance of making small errors was reduced and the chance of making great ones was almost eliminated. The method was used by astronomers, and Simpson urged all experimenters to adopt it.[46] In part because of his consideration of the limits of accuracy, Cavendish's experimental work was advanced for his time.

For a fact to be established by experiment, the experiment had to be repeatable. William Watson said of an experiment purporting to prove that electricity communicates odors through glass that it must succeed in Venice and Leipzig, as it did, and also in Wittemberg, Paris, Geneva, and Turin, where it did not. A friend of the original experimenter and six fellows of the Royal Society met at Watson's house to repeat the experiment, after which Watson reported that the experiment did not succeed in London either.[47] The original experimenter might himself repeat his experiment in the presence of one or more witnesses. John Canton repeated his experiment with powerful artificial magnets before the president of the Royal Society, who then informed the Society of what he had witnessed.[48] A still more objective way was for the original experimenter to have his experiment repeated by another operator as well as having it witnessed; Cavendish took this course in answering the objection of experimenters who were unable to repeat one of his experiments.

To establish a fact by observation instead of by experiment, independent observations were desirable. Peter Newcome of Hackney Academy reported that six persons in his house felt an earthquake upstairs but no one downstairs did. A similar experience was reported by another person in another house, but that report was not as valuable, since it depended "indeed upon the perception of a single person; whereas his [Newcome's] is verified by the sensations of six different ones."[49] Testimonials by witnesses were collected and weighed. The mental capacity of witnesses was considered relevant to the testimony, as were their profession, wealth, and rank.[50] The author of a paper on a bright rainbow said that he heard about similar rainbows from "intelligent persons."[51] Another author heard about earthquakes from "a very sensible Scotchman" and a woman with "superior" judgment, accuracy, veracity, and a title.[52] The president of the Royal Society was assured that certain observers of an earthquake in Plymouth were not "mean, ignorant, or fanciful" but truthful, "rational and just."[53] When a great storm struck a village, the reporter went to the spot taking with him reliable men, the local physician and clergyman.[54] The dimensions of an "extraordinary" young man, two feet seven inches tall and twelve or thirteen pounds, were confirmed by eight witnesses, all "of figure and fortune" in the neighborhood.[55] In the cases above, reliability became an issue in part because of the uniqueness of the phenomenon, which unlike an

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[46] Thomas Simpson (1755).

[47] Watson (1750, 349; 1751, 237–238). Steven Shapin (1988, 399).

[48] John Canton (1751, 32–33).

[49] Peter Newcome (1750). James Burrow (1750a).

[50] Shapin (1988, 398–399).

[51] Peter Davall (1749, 195).

[52] James Burrow (1750b, 626). Lady Cornwallis told James Burrow of her experience of an earthquake: James Burrow (1750c, 703).

[53] William Barlow (1750, 693).

[54] William Henry (1753, 1).

[55] John Browning (1751, 279). This was actually an account of premature aging. The child was displayed for money in Bristol.

experiment could not be reproduced, though the young man presumably could be measured again. The character and maturity of assistants were also relevant. An experimenter who had been assisted by untrustworthy servants became "very delicate in the choice of the persons who I was desirous should be admitted to our experiment"; he would never again use "children, servants, or people of the lower class."[56] Persons Cavendish invited to witness his experiments likely were fellows of the Royal Society, whose reliability was assumed to be beyond question.

Observers sometimes came together to examine instruments jointly[57] or to collaborate in making observations.[58] No one was more active in cooperative astronomical work in the middle of the eighteenth century than James Short. At his house, he with three other persons observed the occultation of Venus by the Moon,[59] and at his and another house, he with two others observed the transit of Mercury, while at five more locations observations of this event were made by still others.[60] To observe an eclipse of the Sun, Lord Morton invited Short and a French astronomer to his castle north of Edinburgh. This excursion was part of a wider effort in Scotland to observe the eclipse, which was coordinated by cannon fired from Edinburgh Castle; bad weather obscured it at Edinburgh, but observations were made at Morton's and at nine other locations in Scotland.[61] Cavendish did extensive preparations for observing the transits of Venus, a project calling for a collaboration of observers around the world.

The *Philosophical Transactions* regularly contained papers about instruments usually submitted by the persons who made them. They were invariably illustrated by detailed, scaled drawings, without which descriptions of instruments were hard to follow; Smeaton said that the construction and use of his pyrometer were clearer from the drawing than "from many words."[62] The importance of instruments was obvious—almost; from Norwich, a keeper of records of the weather complained that many people in his neighborhood judged the weather only by their "outward senses," without resorting to the thermometer, and accordingly they made mistakes, such as putting the hottest day in June when it was in July.[63] In astronomy the importance of instruments and their quality had long since been demonstrated, though James Bradley thought that the point was still worth making in the middle of the eighteenth century. Not long ago, he said, astronomy had seemed perfected and no further progress was expected, a conclusion based on the instruments at hand, the telescope and the pendulum clock, and on the theory of "our great Newton." Bradley had shown that this confidence was misplaced. First he discovered the aberration of light by observation, and then recently he discovered another annual change in the place of the stars, nutation, caused by a nodding of the axis of the earth, which was perceptible only because "of the exactness of my instrument." The pull of the Moon on the equator of the Earth was under-

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[56] Abbé Nollet (1749, 377).

[57] John Smeaton (1754a, 535, 537, 539–540).

[58] Romé de l'Isle (1954). The subject is the parallax of Mars, determined by observations at two places on earth, in France and in England.

[59] The other observers were John Bevis, John Pringle, and the duke of Queensbury. John Canton observed the event at his house too. John Bevis (1751). Also James Short (1751).

[60] The other observers at different places were John Birch, Jonathan Sisson, John Bird, John Smeaton, John Canton, and Lord Macclesfield. James Short (1753b).

[61] James Short (1748, 591).

[62] John Smeaton (1754b, 600, 605).

[63] William Arderon (1750, 574).

stood theoretically, but the nutation of the Earth had not been foreseen. This object lesson in discovery demonstrated the "great advantage of cultivating this, as well as every other branch of natural knowledge, by a regular series of observations and experiments." The "more exact the instruments are [. . .] and the more regular the series of observations is [. . .] the sooner we are enabled to discover the cause of any new phenomenon." Bradley advised astronomers to begin by examining the correctness of their instruments,[64] a practice he himself followed religiously. No astronomer before him had so thoroughly examined his instruments in search of error, studying them individually and comparing them one with the other.[65] In Bradley's spirit, Cavendish examined instruments in both of these ways and in every branch of physical science, and as Bradley recommended he cultivated experimental fields comprehensively. It is significant that Bradley signed the certificate proposing Henry Cavendish for membership in the Royal Society.

In the middle of the eighteenth century, observations with measuring instruments appeared in reports on a wide variety of subjects in the *Philosophical Transactions*: a measured draft given to, and blood taken from, a patient;[66] the path of a stroke of lightning;[67] the heat of a cave.[68] Henry Miles, a clergyman with a wide-ranging interest in quantities, who reported a measurement of the "bigness" of a fungus, 210th part of an inch,[69] communicated an unusual kind of paper to the *Philosophical Transactions*, a philosophical essay on quantity. In it quantity is identified with "measures," which require a "standard," so that "all men, when they talked of it, should mean the same thing."[70] As quantity applied to anything short of affections and appetites, so did measures and standards. For example, the physician John Pringle laid down "standards" in his quantitative ranking of salts by their power to resist putrefaction.[71] A quantitative experimentalist, Cavendish defined and routinely used standards.

The quantitative direction in scientific work is seen in various forms in the *Philosophical Transactions*. Chemistry suffered from the unrepeatability of its experiments, according to Cromwell Mortimer, a physician who studied the effects of chemical remedies in diseases. The reason, he said, was the failure of chemists to record the heat: the chemist's laboratory should be equipped with "various Sorts of Thermometers, proportioned to the Degree of Heat he intends to make use of," and he should keep track of the time the heat is applied, observing "his Clock with as much Exactness as the Astronomer."[72] Cavendish used thermometers extensively in his experimental work, and he improved their accuracy; and in his heat experiments he used clocks to find the rate of cooling. Richard Davies, formerly a Cambridge fellow, published an impressive quantitative study based on weighing, a table of specific gravities, justified by their "manifold applications [. . .] for the purposes of Natural Philosophy," as shown by the "great author" Newton, who determined specific gravities with the "most scrupulous care and exactness" in his optical inquiries, and as further shown by Hauksbee, Cotes, Jurin, Musschenbroek, and other natural philosophers, mathe-

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[64] James Bradley (1748, 1–5).

[65] Allan Chapman (1993, 209).

[66] George Bayly (1751).

[67] Henry (1753).

[68] William Arderon (1748).

[69] Henry Miles (1750b).

[70] Henry Miles (1748, 506).

[71] John Pringle (1750).

[72] Cromwell Mortimer (1747/1746, 673). This paper was first read in 1735 and printed later with revisions.

maticians, and physicians.[73] In chemistry, Cavendish distinguished different species of air by their specific gravities, an experimental measure capable of considerable accuracy. His precision chemical balance is described later in this book. Wilson could not picture Cavendish without his measuring instruments: wherever we catch sight of Cavendish, he said, "we find him with his measuring-rod and balance, his graduated jar, thermometer, barometer, and table of logrithms; if not in his grasp, at least near at hand."[74] Cavendish was doing what investigators in many subjects in the second half of the eighteenth century were doing, making measurements.

Electricity was the most active experimental field in mid century. In this "new field of researches," Stephen Hales wrote in the *Philosophical Transactions* for 1748, "there are daily new discoveries made."[75] Emanuel Mendes da Costa, future clerk of the Royal Society, wrote in 1753 that electricity was "now a days the chiefest occupation of philosophers."[76] Cavendish's father carried out experiments on electricity in collaboration with William Watson, who had improved the device that transformed the field, the Leiden jar.[77] Important in a related way was Watson's review of Benjamin Franklin's book on electricity, consisting mainly of letters to his English correspondent, all or parts of which had been read at the Royal Society.[78] There was a sense among electrical investigators that they were no longer working on the periphery of the subject but were dealing with questions of the "nature" of electricity, its "general principles," "quantities" of electricity, and the "laws of electricity."[79] Twenty years later, drawing on the work of Watson and Franklin, based on a hypothesis about the nature of electricity, Cavendish pursued experimental and theoretical researches on the quantities, principles, and laws of electricity.

Electricity had begun to be studied in the laboratory of nature. In the *Philosophical Transactions*, Franklin proposed investigating lightning and referred to the "Philadelphia experiment."[80] Watson together with several fellows of the Royal Society tried without success to draw electricity during a thunderstorm, but John Canton, Benjamin Wilson, and John Bevis succeeded.[81] Daring experiments on lightning were reported to the Royal Society from around the world. Cavendish would serve on a lightning committee of the Royal Society.

Lightning was new insofar as it was explained by electricity but otherwise it belonged to the general class of violent events, which were a staple of the *Philosophical Transactions*, as they were of life in the eighteenth century. Incidents of thunder and lightning with their attendant "melancholy accidents" were regularly reported, minutely described, and occasionally measured. Lightning struck a ship in a "violent manner, disabling most of the crew

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[73] Richard Davies (1748, 416–435).

[74] Wilson (1851, 187).

[75] Stephen Hales (1748b, 410).

[76] Emanuel Mendes da Costa to William Stukeley, 9 Nov. 1753, in John Nichols (1817–1858, 4:503).

[77] William Watson (1747, 709 ff).

[78] In 1746, the Royal Society learned of the Leiden jar. Acting on a suggestion by John Bevis, Watson increased the effect of the Leiden jar by lining both sides of the glass with metal and also by making the glass thin. That same year he explained how his theory of electricity explained the action of the Leiden jar. In 1747, Charles Cavendish forwarded to Watson a letter from Franklin giving his explanation of the Leiden jar. In 1748, Watson told the Royal Society that his and Franklin's theories of electricity were effectively the same. Simon Schaffer, "Watson, Sir William," *DNB*, 2d ed. (http://www.oxforddnb.com/view/printable/28874).

[79] John Ellicott (1748, 196, 221–222).

[80] Benjamin Franklin (1752).

[81] William Watson (1752a); John Canton (1753). There were many papers at this time on lightning experiments.

in eye and limb."[82] The mainmast of another ship was shattered when a "large ball of blue fire" rolled over the water and exploded, "as if hundreds of cannon had been fired at one time."[83] In a valley, in the "violence of the storm," a cloudburst and flash flood threw up "monstrous stones," which were "larger than a team of ten horses could move."[84] A meteor that looked like a "black smoky cloud" split an oak, and its "whirling, breaks, roar, and smoke, frightened both man and beast."[85] Clouds and auroras were seen to turn "blood-red."[86] Plagues of locusts "hid the sun," and undeterred by "balls & shot," they "miserably wasted" the land.[87] Victims of the Black vomit" experienced delirium "so violent" that they had to be tied down so that they did "not tear themselves in pieces."[88] Bitten by a mad dog, a horse in its agony gave off breath "like smoke from a chimney-top," with "much blood scatter'd up and down the stable."[89] An experimental dog was held in a poisonous vapor on the floor of a grotto, "tortured for three minutes," then revived. After being given a South American poison, a "great number of living animals" were "seized with a sudden and almost universal palsy" before they died.[90] Many of the medical papers in the *Philosophical Transactions* described extreme pathologies and monstrocities in more or less ordinary language, unsparing of the reader. Medical procedures could be as terrible as the illness or trauma that called for them. A woman with a "violent pain" in her eye went to a surgeon, who cut out the eye, "bled her plentifully," applied a blister to her neck, and purged her repeatedly.[91] Children were carried away by contagion, in the course of which a five-year-old girl was observed to cough up a "large quantity of white rotten flesh" in her so "violent a death."[92] In Constantinople the plague was raging, becoming "most violent" when the weather was hottest, as if to make it worse.[93] Few persons escaped the "small-pox sooner or later in life," with its "very terrible consequences," and those who had escaped it lived "in continual apprehensuins and fear thereof."[94] A doctor of divinity and fellow of the Royal Society reported on an extraordinary case of a young man whose tendons and muscles were turning to bone, indicating that if the poor man lived, he would become "completely ossified."[95] When limbs were amputated, agaric was plugged into the severed arteries, eliminating the usual method of needle and ligature, the most painful part of amputations and sometimes the cause of death.[96] The fright and misery of the world eventually would be brought to an end because the world was going to end, according to astronomical calculation, by spiraling toward the Sun and on its way "necessarily be burnt."[97] Reading the journal could be a disquieting experience. Cavendish, who presumably read about violent events appearing in the *Philosophical Transactions*, was not drawn to them in his studies. He advised on the

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[82] William Borlase (1753); John Waddell (1749, 111–112).
[83] Chalmers (1749, 366).
[84] John Lock (1750/1749).
[85] Thomas Barker (1749).
[86] Henry Miles (1750a, 348). William Stukeley (1750c, 743).
[87] Anonym (1749, 30–37, on 30–31).
[88] Antonio de Ullöa (1749, 46:134–39, on 135).
[89] John Starr (1750a, 474, 478).
[90] Abbé Nollet (1751, 53). F.D. Herrisant (1751, 90).
[91] Edward Spry (1755).
[92] John Starr (1750b, 439).
[93] Mordach Mackenzie (1752).
[94] Richard Brooke (1752, 470).
[95] William Henry (1751, 89).
[96] Joseph Warner (1754).
[97] Leonhard Euler (1749, 204).

way to protect against lightning strikes, but he left no first-hand observations of them or, for that matter, of most one-of-a-kind phenomena.

In the laboratory the violence of nature was simulated, and it could be dangerous; lacking apparatus with effective safety features, investigators sometimes were "intimidated" and "deterred," in "danger of being hurt."[98] In 1753 the German physicist Georg Wilhelm Richmann living in Russia was electrocuted in a room containing his apparatus while performing an experiment on the electrical nature of lightning.[99] The discharge of a Leiden jar was analogous to lightning; if the Leiden jar was mishandled, its artificial lightning could be dangerous to the operator.[100] Cavendish was aware of the potential violence of the laboratory. "To avoid being hurt" by a bottle in which he exploded gases, he manipulated his apparatus by a string at a safe distance.[101]

The most frightening event reported in the *Philosophical Transactions* was an earthquake. The year 1750 "may rather be called the year of earthquakes, than of Jubilee," a fellow of the Royal Society observed. The earthquakes of that year occurred as if on command of the Royal Society, being thought to center on London, "the place to which the finger of God was pointed."[102] Cavendish was in his second year at the University when an entire issue of the *Philosophical Transactions* was devoted to earthquakes and to the "natural philosophical understanding" of such "wonders."[103] Presented as an appendix to the regular issues, the earthquake issue consisted of fifty-seven papers submitted to the Royal Society dealing with four earthquakes felt in England and on the Continent that year, a foreshadowing of the great earthquake of 1755 that destroyed Lisbon.

About half of the observers reporting firsthand on the earthquakes of 1750 in the *Philosophical Transactions* were fellows of the Royal Society, who also collected testimony and communicated letters from other observers who were not.[104] Fellows or otherwise, observers of earthquakes rarely noted the direction, time, and duration of the shock.[105] As earthquakes went, those of 1751 were not especially severe—Gowin Knight thought it was worth reporting that in a neighbor's house a "firkin of butter" was thrown from a shelf[106]—but witnesses experienced them as "violent." People thought first of gunpowder, cannon, the explosion of a magazine or powder mill or a mine, or lightning.[107] In his house, Martin Folkes along with Macclesfield and other visitors "felt themselves strongly lifted up, and presently set down again," while the coachmen standing outside Folkes's door feared

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[98] We go beyond the time when Cavendish was at the University to when he began his electrical and chemical experiments at home. CL'Epinasse (1767, 188); Peter Woulfe (1767).

[99] William Watson (1754).

[100] Henry Eeles (1752). Eeles took exception to the standard analogy between fired gunpowder and thunder, proposing in its place an up-to-date explanation based on the fire observed in electrical experiments.

[101] Henry Cavendish (1766); in *Sci. Pap.* 2:77–101, on 82.

[102] William Stukeley (1750a, 669; 1750c, 732).

[103] Issue no. 497, *Philosophical Transactions. Being an Appendix to Those for the Year 1750.* Simon Schaffer (1983, 17–18).

[104] Of the 57 papers, the first 26 were all by fellows of the Royal Society. Of the remaining 31 papers, at least 16 were by fellows of the Royal Society. They included many prominent members, but Charles and Henry Cavendish were not among them, and few of them had Cavendishes' interests: astronomy, chemistry, mathematics, and natural philosophy. The earthquakes did not provide an opportunity for those who used instruments and made measurements on a regular basis.

[105] "It is no wonder, that in a shock so sudden and alarming, that very few satisfactory observations are made." William Cowper (1750, 648).

[106] Gowin Knight (1750b, 604).

[107] Smart Lethieullier (1750).

the house coming down on their heads.[108] Gowin Knight's house "shook violently," and the duke of Newcastle's servant told him that all the way from London Bridge the people were frightened.[109] Animals too were frightened: a cat was startled, a dog was terrified, cows and sheep were alarmed, fish were disturbed, a horse refused water, and crows took flight.[110] Sensations were described variously, such as "falling into a fit."[111] Roger Pickering, a close observer of the weather and natural curiosities, gave a detailed account of his sensations while lying in bed; being a clergyman, he gave his reflections, which led him beyond the "secondary causes" of the earthquake to the grandeur and majesty of the "Lord of Nature."[112]

The "secondary causes" were the scientific question, to which two answers were published in the *Philosophical Transactions*. Stephen Hales, a clergyman, said that both the ordinary and the extraordinary events of nature were caused by God, but that they did not lie outside natural explanation for that reason. After describing his sensations while lying in bed during a tremor, he explained with reference to an experiment from his *Statical Essays* that an earthquake is caused by the explosive mixing of air with sulfurous vapors rising from the the pores of the Earth.[113] William Stukeley, another clergyman, after a perfunctory consideration of the religious view, attributed earthquakes to "electrical shock, exactly of the same nature as those, now become very familiar, in electrical experiments." With reference to Franklin, Stukeley said that the "little snap, which we hear in our electrical experiments, is the same snap, only magnified, that we hear in thunderstorms." Having gotten to know the "stupendous powers" of electricity by experiment, he called on electricity to explain the "prodigious appearance of an earthquake."[114] Hales's and Stukeley's causes of earthquakes, aerial substances and electricity, were the main experimental subjects in Britain in the second half of the eighteenth century, as they were two of Cavendish's main experimental fields.

Reports of the catastrophic Lisbon earthquake in 1755 filled the last roughly hundred pages of the volume of the *Philosophical Transactions* for that year and much of the next year's. Unlike reports of the earlier earthquakes of 1750, these recounted loss of life and physical destruction. The most important single response to the earthquake was John Michell's paper on the general cause of earthquakes, which he owed to the bounty of facts about the earthquake of 1755, many of which had been collected by the Royal Society and published in its journal. He acknowledged that observations of the earthquake were often carelessly made and reported, but the "concurrent testimonies" of so many persons established the main points. Having selected data that had the "greatest appearance of accuracy," he took a "mean" of them.[115] We move ahead a few years after Cavendish had left Cambridge to consider Michell's paper, which was printed in the *Philosophical Transactions* for 1760; Michell would be important to Cavendish, and this paper suggests why.

Michell disagreed with Hales and Stukeley, who located the cause of earthquakes near the surface of the Earth. Volcanoes were proof that fires could exist underground without contact with the air, and by analogy (and for other reasons) Michell concluded that volcanoes

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[108] Abraham Trembly (1750, 611).

[109] Gowin Knight (1750b, 603).

[110] Various reports: *PT* 46 (1750): 618, 621, 641, 651, 682.

[111] Thomas Birch (1750, 616).

[112] Roger Pickering (1750, 625).

[113] Stephen Hales (1750, 676–677).

[114] William Stukeley (1750b, 642–644; 1750a, 663).

[115] John Michell (1760, 629).

and earthquakes had the same cause, the contact of underground water with underground fire, turning the water instantly and explosively into steam. The steam in turn compressed the matter of the Earth, and because the Earth was elastic, the compression was followed by dilation, generating waves that were propagated horizontally over a long distances. Michell made the scientific study of earthquakes quantitative by developing methods for determining their velocity, location, and depth, which he applied to the Lisbon earthquake, with implications for geological science. His theory of earthquakes was a beginning of an exact, dynamical science of the Earth. When Cavendish heard Michell's paper read, he would have recognized its author as a fellow natural philosopher. In the judgment of later geologists, Michell's earthquake paper contained results that were more important than his theory of earthquakes, having to do with his understanding of the Earth as consisting of uniform strata.[116] Cavendish made a prolonged study of strata, in communication with, and at least in part because of, Michell.

Like earthquakes, the weather was a force of nature to be reckoned with, and some persons (not Michell) believed that there ought to be a connection, consulting their thermometers and barometers whenever they felt a tremor.[117] Some persons read their weather instruments every day, compiling local histories both of extreme and of normal activity. In the accounts they sent to the Royal Society, they usually gave rainfall, pressure, and temperature, often including the mean and the highest and lowest. The clergyman Henry Miles submitted a paper about the thermometer, an indispensable instrument of the weather, which Newton had considered and others had tried to bring to "greater Perfection."[118] The credibility of the mercury thermometer, which was generally accepted as the best kind of thermometer, was implicitly put to the test in the extreme climate of Siberia, where temperatures below -100°F were recorded.[119] Cavendish clarified the behavior of mercury thermometers and at the same time corrected reports of extreme natural cold on Earth. He was recognized as the Royal Society's leading expert on the thermometer and other instruments of the weather.

The naturalist William Arderon, who published frequently on the weather in Norwich, kept a record of the constant temperature in a cavern under nearby hills, which he compared with the mean of the temperatures above ground, finding them almost identical, and he found the same for the temperature of a spring in the cavern.[120] Cavendish frequently measured the temperature of springs and deep wells, encouraging a worldwide effort to measure average climates that way.

Some authors appearing in the *Philosophical Transactions* worked in both the physical and the life sciences, or they brought the physical sciences to bear on the problems of the life sciences. The Royal Society's Croonian Lecture on the nature and laws of muscular motion in 1747 was given by the physician Browne Langrish, who explained muscular motion by Newton's atttracting and repelling forces, dedicating his lectures to Stephen Hales, whose "indefatigable Researches into Nature" showed that particles of air are attracted to solids. Langrish's "scheme" was based on "those Hints which Sir Isaac Newton has given us in the Queries at the End of his incomparable Book of Opticks."[121] In 1751 the physician Charles

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[116] Michell (1760, 582).

[117] Henry Miles (1749).

[118] Henry Miles (1750c).

[119] John Fothergill's extracts from Gmelin (1748, 260). William Watson (1753a).

[120] William Arderon (1748).

[121] Browne Langrish (1747, i–ii, 7–8).

Morton published a paper on the same subject, muscular motion, which he, a follower of the "Newtonian, which is the philosophy of nature," organized by observations, experiments, lemmas, and scholia; in keeping with tradition, Morton regarded his subject as belonging to "natural philosophy."[122] To the physician William Watson, known for his researches alike on plants and electricity, the study of living nature had the same goal as the study of the physical world, which was to learn the "general laws" of nature, from "which however she sometimes deviates." Cavendish did research in all parts of physical science; he did not do research on plants and animals to understand *their* laws, but in several of his researches he studied physical properties of plants and animals.

Astronomy and classics came together in the *Philosophical Transactions*. The antiquarian William Stukeley said that scholars had gotten the year wrong for the solar eclipse predicted by Thales. With the help of an astronomer, he corrected them, demonstrating the "admirable use to be made of astronomy in ascertaining matters of history."[123] There was a tradition of astronomical reasoning in history, and just as in science, in chronology Newton received gentle criticism.[124] A Jesuit who had worked out a chronology of ancient China proposed to do the same for Chinese astronomy.[125] Cavendish made a study of the Hindu calendar.

Honoring Bacon's ideal of a scientific society that "labours to relieve the necessities of human life,"[126] the Royal Society accepted communications that were directed to utilitarian ends. At the time Cavendish was studying at the University, the *Philosophical Transactions* included papers on mechanical power, manufactures, gunnery, navigation, medicine and health, and the prevention of disasters. Distinguished "both as a chemist, and as a philosopher," William Brownrigg investigated salt-making. In a review, Watson hoped Brownrigg would do what the Royal Society's historians of salt-making had not, overcome Britain's disadvantage in this trade.[127] John Mitchell gave a history of potash-making, which in England was "practiced only by the vulgar, and neglected and overlooked by the learned." No nation could do without potash, an essential ingredient in soap, bleach, and glass, and England was a nation that did not know how to make it correctly.[128] John Smeaton showed the Royal Society a tackle of twenty pulleys small enough to fit into the pocket, and with another block of pulleys, he offered an Archimedean-like demonstration of a single person lifting a gun and carriage aboard a naval ship. The reason he brought his compound pulley before the Society was its promise of "much utility […] for merchants, seamen, builders, engineers, &c."[129] Like the pulley, the steam engine made possible the lifting of heavy weights, and it too could be improved, as Smeaton showed by his modification of Thomas Savery's early steam engine, which was useful in raising water from mines and supplying water.[130] In Newgate prison, infectious fevers killed convicts and officers of courts of justice who were exposed to convicts during trials; to achieve "purity of air" in the prison, it was decided to install

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[122] Charles Morton (1751, 308, 314).

[123] William Stukeley (1753, 222).

[124] Ibid. George Costard (1753, 19).

[125] Gaubil (1753, 309–317).

[126] William Watson's expression, from his abstract and review of a book that fit the Royal Society's ideal: "An Account of a Treatise by Wm. Brownrigg …" (1748b, 372).

[127] Ibid., 352.

[128] John Mitchell (1748, 541).

[129] John Smeaton (1752a, 497).

[130] John Smeaton (1752c).

a ventilator designed by Hales, worked by a machine resembling a windmill.[131] On Hales and Lord Halifax's recommendation, Captain Henry Ellis installed Hales's ventilators in his ship, which caused candles to burn better, bells to ring louder, and cargo to hold up better, in addition to being "good exercise for our slaves."[132] Electrical healing was more often the product of enthusiasm than of repeatable experiments. Claims for it were received with proper caution, but some medical virtue of electricity seemed evident to nearly everyone at the time, including the careful William Watson, who acknowledged that the administration of a "large quantity" of electricity "greatly heats the flesh, and quickens the pulse," conferring "very great advantages."[133] Bills of mortality documented the relative unhealthiness of places, useful knowledge for "many excellent purposes," including the calculation of annuities on lives, on which a sizeable part of the "real estates of these kingdoms" depended.[134] Spring waters had medical uses, and seawater might be converted to freshwater.[135] Improvements were made in navigation, especially in the mariner's compass, the invention of which, Gowin Knight said, had "probably been of more general and important use to human society, than the invention of any one instrument whatsoever."[136] To celebrate the recent peace, 6000 rockets were fired in Green Park without incident, thanks to Hales's recommendation of spreading a layer of dirt or fine gravel over the wood floor to prevent fire.[137] The *Philosophical Transactions* published papers in these years on military applications such as projectile paths in gunnery and rockets. There were many papers on lightning rods; in this direct application of science, Cavendish was repeatedly called on by the Royal Society.

We see that many of the kinds of scientific problems Cavendish worked over his long life were addressed in the *Philosophical Transactions* at the time he was studying at the University. Through his manner of treating problems and not his invention of them, he left his mark on science.

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[131] John Pringle (1753, 42).

[132] Henry Ellis (1751).

[133] William Watson (1752b, 406).

[134] James Dodson (1753, 333–334).

[135] John Bond (1753). William Watson (1753b).

[136] Gowin Knight (1750a, 505). John Smeaton (1750).

[137] Stephen Hales (1748a).

Chapter 8
Early Researches

William James's observation that "in most of us, by the age of thirty, the character has set like plastic"[1] applies to Cavendish, if we take his "character" to include a narrow focus on science. His earliest known extended series of experiments were in chemistry and heat, specifically on arsenic and on specific and latent heats. This was around 1764,[2] twelve years after he had left the university and four years after he had been elected to the Royal Society. His first publication came two years later, on the chemistry of air, when he was thirty-five; this was rather late for a scientific researcher to begin, but in this as in other ways he was not typical. Never in a hurry to bring his work before the world, he was concerned to perfect it before communicating it.

Cavendish's Correspondent

The earliest contributions to the *Philosophical Transactions* were letters to its founder, Henry Oldenburg. Over time, the pretense of letters was dropped, and the genre of the scientific paper emerged as authors increasingly wrote for their readers instead of to the editor. With the introduction of a committee of papers in 1752, the editor withdrew further.[3] Still, during the time Cavendish was a student and beyond, publications in the *Philosophical Transactions* commonly took the form of "letters" addressed to the president of the Society or to a member who was knowledgeable about the subject. Sometimes a letter by an author would be published as a preface to a paper. The practice of sending letters to the journal is the background of Henry Cavendish's papers written to be read by a person referred to as "you." Given Cavendish's habits of privacy, a correspondent draws our interest.

"You" might have been his father, who was convenient, though here an informal way of communicating would have been more natural. Among other possibile correspondents is the longtime family friend William Heberden, who having lectured on chemistry at Cambridge would have been a competent reader; Cavendish's first published chemical research was carried out at Heberden's request. Another possibile correspondent is another family friend William Watson, who together with Heberden signed Cavendish's certificate at the Royal Society. Others are the London apothecary Timothy Lane, the London schoolmaster John Canton, and the Cambridge fellow and Anglican minister John Michell.

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[1] Paul T. Costa, Jr., and Robert R. McCrae (1994, 21–22).

[2] Cavendish's editor Thorpe refers to "an interpolation table calculated by Cavendish, from the results of measurements made in conjunction with his father on the Tension of Aqueous Vapor…. They appear to have been made about 1757 and are based upon a number of observations over a considerable range of atmospheric temperature and probably, therefore, at various seasons of the year." If Thorpe is correct about the year, they are the earliest experiments of Henry Cavendish's we have record of. *Sci. Pap.* 2: 355.

[3] Charles Bazerman (1988, 130, 137).

Timothy Lane published papers in the *Philosophical Transactions* on an electrometer in 1766 and on mineral water in 1769, which were Cavendish's interests around the same time. In 1766 Cavendish informed himself on electricity,[4] later making use of Lane's electrometer in his researches, and in 1767 he published a paper on mineral water. Lane took up the problem of mineral water where Cavendish left it, tying it closely to pneumatic chemistry and submitting his experiments privately to Cavendish for his opinion before publishing them. Lane and Cavendish had a similar aptitude for accuracy: Lane spoke of Cavendish's known "accuracy," and his own electrometer introduced a "much greater degree of precision" in the field of electricity, being capable of measuring the quantity of electric fluid stored in a Leiden jar with "tolerable accuracy."[5] In 1769 Cavendish invited Lane to five meetings of the Royal Society before his election the following year, Cavendish having signed his certificate along with John Canton, Watson, and Heberden.[6] The Royal Society extended a scientific exchange that had already been established between Lane and Cavendish, which may have included Cavendish's sending him papers to read.

A variety of evidence points to John Canton, a schoolmaster in Spital Square, as Cavendish's correspondent. Thirteen years older than Cavendish, Canton was elected fellow of the Royal Society in 1749, and he began publishing his experiments in the *Philosophical Transactions* four years later. Cavendish had a connection with Canton through his father, who in 1762 confirmed Canton's proof of the compressibility of water, discussed earlier. In 1766 Cavendish wrote to Canton about a book on electricity, establishing that the two had a connection by then; electricity was a major interest for both of them. The second possible evidence is an undated manuscript by Cavendish, "Paper Communicated to Dr Priestley," in which Cavendish referred to what Priestley wrote about mephitic air in 1767, which he would have got personally from Watson or Canton, probably the latter.[7] In his manuscript "Experiments on Heat," Cavendish left a clue concerning the identity of a correspondent "you," which fits Canton. Cavendish said that a certain substance differed from other substances by not transmitting heat as fast, commenting on his choice of the word "transmitting": "I forbear to use the word conducting as I know you have an aversion to the word, but perhaps you will say the word I use is as bad as that I forbear."[8] Fluids are conducted; if heat, as Cavendish thought, is not a fluid, "conduction" conveys a false idea, implying that his reader "you" accepted the idea of heat as the motion of particles, narrowing the circle of potential correspondents. In a paper in 1768, Canton showed that he regarded heat as the agitation of the parts of bodies.[9] Canton was generally interested in Cavendish's subject, heat, studying its effect on diverse phenomena: magnetic strength, electrical conduction in

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[4] Roderick W. Home (1972)

[5] Timothy Lane (1769, 216; 1767, 451); "Description of an Electrometer … with an Account of Experiments …," *PT* 57 (1767): 451–460.

[6] On 20 Apr., 4 and 11 May, 8 June, 9 Nov. 1769, JB, Royal Society 26.

[7] Henry Cavendish, "Paper Communicated to Dr Priestley," Scientific Mss, Misc. The paper is directed to "you," who is either Canton or Watson, most likely the former, who would have passed it along to Priestley. At this time, Cavendish did not know Priestley, who lived in Leeds, and Canton who knew Priestley lived in London. Two letters Priestley wrote to Canton in 1767 refer to Priestley's experiments on mephitic air. Joseph Priestley to John Canton, 27 Sep., 12 Nov. 1767, in Joseph Priestley (1966, 58).

[8] Henry Cavendish, section of "Experiments on Heat," entitled "Experiments to Shew That Bodies in Changing from a Solid State to a Fluid State Produce Cold and in Changing from a Fluid to a Solid State Produce Heat," *Sci. Pap.* 2:348–50, on 350.

[9] John Canton (1768, 342–343).

solids and air, absorption of electric fluid in solids, and emission of light in phosphorescence and luminescence.

The persons mentioned so far were capable of serving as a sounding board for Cavendish's experiments but probably not for his mathematics. At the bottom of the last page of a carefully drafted paper on the motion of sounds, Cavendish added a note addressed to "you," mentioning a demonstration, "which if you have a mind I will show you."[10] A possible mathematical reader for this paper was John Michell, with whom Cavendish later had a known connection, but the paper is undated and Cavendish had many Cambridge acquaintances who understood mechanics and mathematics.

As a special case, we consider one more possibile correspondent, John Hadley. He died suddenly in November 1764, the year Cavendish began saving his experimental papers, but in his writings that year, Cavendish could have had him in mind. Latent heat was one of Cavendish's first subjects, and we know about an experiment Hadley performed on latent heat. Chemistry, Cavendish's other early subject, was also Hadley's subject. Born the same year as Cavendish, Hadley entered the same college in Cambridge in the same year, and like Cavendish, he was good at mathematics, graduating fifth wrangler in the mathematical tripos examination.[11] Elected to the Royal Society before Cavendish, Hadley signed the certificate for Cavendish's membership, suggesting that he knew about Cavendish's work before Cavendish had published anything. Both were members of the Royal Society Club, and Hadley was a guest at the Cavendish home in London, so they had opportunity to keep in touch. When in 1756 a proper chair of chemistry at Cambridge was endowed, Hadley was appointed to it. He published a plan of chemical lectures in 1758, and that year and the next he lectured in the chemical laboratory at Cambridge.[12] He based his course largely on the work of foreign chemists, including the same ones Cavendish took his first chemical problems from, and he also included the British chemists Hales and Black, whose work was the starting point of Cavendish's first published paper. In an unpublished part of his first paper Cavendish mentioned Hadley's account of the distillation of a salt with a metal as support for his own experiments on the distillation of various substances.[13] Hadley gave close attention to mineral water in his lectures, even beginning his own investigation of a mineral water, which he broke off when it became too difficult.[14] Cavendish's second publication was a chemical analysis of a mineral water. Cavendish addressed his earliest preserved chemical research, in 1764, to "you." If he had been in the practice of writing for Hadley, he may have continued to write for him even after 1764, *as if*.

Given the range of his researches, Cavendish likely had more than one correspondent. Considering that his scientific manuscripts contain no responses to his early researches, it is conceivable that he did not send his work to anyone but simply adopted the form of the letter-

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[10] Henry Cavendish, "On the Motion of Sounds," Cavendish Mss VI(b), 35:10.

[11] "Hadley, John," *DNB*, 1st ed. 8:878–880, on 879.

[12] John Twigg (1987, 212–213). John Hadley (1758). At Trinity College, Cambridge, there is a two-volume manuscript of Hadley's lectures: "An Introduction to Chemistry, Being the Substance of a Course of Lectures Read Two Years Successively in the Laboratory at Cambridge by John Hadley …." "Hadley, John," 879.

[13] Hadley's work is referred to in a footnote to the unpublished fourth part of Cavendish's paper on factitious air in 1766. "Experiments on Factitious Air. Part IV. Containing Experiments on the Air Produced from Vegetable and Animal Substances by Distillation," *Sci. Pap.* 2:307–316, on 313.

[14] Hadley wrote to the secretary of the Royal Society that the analysis of mineral water was "very difficult & would lead into very extensive chemical inquiries, "and his own papers on it were "not of consequence enough to be printed." John Hadley to Thomas Birch, 13 Sep. 1762, BL Add Mss 4309, f. 9.

report from the *Philosophical Transactions*. In the absence of more revealing documents, we can only speculate about his correspondents.

Chemistry

By all accounts Cavendish cut an awkward figure in public. He did not do so at home, where everything was made to fit. Furnished with instruments and books, his home was the principal location of his chosen life. The gentleman's double house on Great Marlborough Street, with its elegant stairs leading off the entrance and its rooms for entertaining, was unlikely to have been used also as a chemical laboratory. If Cavendish carried out his chemical researches at home, as he no doubt did, the location would have been either the stables or the separate apartment on the grounds behind the main house, and most likely in the former. Since we know that his father had chemicals, a laboratory in some form might already have been in place for Henry. In any case, by the time he wrote his earliest surviving papers on chemistry, he had a substantial chemical laboratory. We have no description of it, but we know in general what it had to be like (Figs. 8.1–8.2). It would not have been located in the underground rooms of the apartment behind the main house (if he was living there then), for in the dampness, metals would have rusted, furnaces collected mold, salts turned watery, and labels fallen off bottles. The laboratory would have been in a ground-floor room or in a room in or above the stables, with openings to the outside at each end for admitting fresh air and clearing away poisonous vapors. We suppose that there was a chimney high enough to walk under and wide enough to walk in front of. Beneath it we picture various furnaces and probably a double bellows to fan the flames from gentle heat to red hot. Ready at hand, suspended on hooks, would have been pokers, pincers, tongs, shovels, and pans, much as in a kitchen of that day. Near the chimney was an anvil along with hammers and a range of other tools. Lining the walls were shelves for containers and chemicals, near which were bins for storing bulk charcoal, sand, and quicklime. Since acids, alkalis, metals, and earths had to be as pure as possible, standing in a corner of the laboratory was a lead or stone "fountain" with a drain pipe for cleaning vessels after each use, no doubt by an assistant. In the center of the room was probably a large table for chemical operations not requiring a high heat, on which were laid out scales, mortar and pestle, filtration paper, corks, stirrers, pencils, pens and ink, and a stack of small sheets of paper for keeping notes.[15] From Cavendish's manuscripts, we can be specific about what he required to carry out his early researches. Heat entered into most of his operations: roasting, calcining, dissolving, subliming, evaporating, and distilling. His sources of heat were lamps, a forge, and a reverberatory furnace, designed to direct the flame back on the heated substance, placed high into the chimney in anticipation of "obnoxious" fumes. There was a sand pot for distilling at "sand heat" and for holding bottles. Other operations included precipitating, crystallizing, filtering, deliquescing, and weighing. At some time Cavendish acquired a cabinet containing scales of high quality. He had an elaborate collection of containers, some made of metal, some earthen, most of

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[15] We have been guided in our sketch of Cavendish's laboratory by the entry "Laboratory (Chemical)" in Pierre Joseph Macquer's *Dictionary of Chemistry*, originally published in 1766, just after Cavendish had begun his known chemical experiments. Macquer's laboratory was intended for the "philosophical chemist," and together with his list of reagents, it sufficed for "any chemical experiment." P.J. Macquer (1771). A more detailed itemization of apparatus divided into items used in preparation of operations and items used in operations is given in Peter Shaw and Francis Hawksbee (1731, 19–21).

glass. There were open flasks, Florence flasks (having long, narrow necks), retorts (having downward bending necks for distilling), receivers (flasks for retaining condensates and distillates), adapters (for connecting retorts and receivers), pipkins (small pots and pans), bottles of various sizes, glass tubing, and copper pipe. There was a lead crucible for keeping the bottom of another crucible placed in it cooler than the top. There was another crucible designed by Cavendish for use in the reverberatory furnace, complete with a set of aludels (pear-shaped pots open at the bottom as well as at the top and made to fit over one another for subliming). Cavendish's apparatus was made for the purpose, to which he added a humble coffee cup for calcining. His materia chemica included solvents, acids, solutions of metals and acids, alkalis, neutral salts, and solutions and treated papers for testing acids and alkalis. Cavendish's chemical experiments depended on a sizable investment in chemical apparatus and supplies. The chemist James Keir may have had Cavendish in mind when he gave as one reason for the emergence of chemistry as a science its recent cultivation by "persons who employ the advantages attending rank, opulence, leisure, and philosophical minds."[16]

Ever since Wilson's biography, Cavendish's mind has been likened to a calculating engine, and although it is a caricature, he was an experimenter who made copious quantitative observations and calculations. He filled his laboratory notes with numbers standing for proportions of reactants and weights expressed in ounces and their breakdown into drams or grains. In combination with his measurements, he expressed in numbers various aids such as standards, equivalents, and saturation (the point at which acids in combination with other substances lose their acidity or at which solutions have dissolved as much solutes as they can). Cavendish's skill in quantitative work is evident in his early chemical research, in which he worked with uncommonly small amounts of substances, ounces instead of the familiar pounds.

Cavendish typically began an experiment with carefully weighed quantities of substances, which he then combined and performed various operations on, and the products he obtained he would again weigh. He might then put the products through a series of tests, "small experiments" as he called them, in which he did not record, and probably did not measure, the quantities involved. As he proceeded, he described as well as measured: in his investigation of neutral arsenical salt, he witnessed fuming, shooting of crystals, and other manifestations of chemical and physical activity. By smell, he distinguished between acids and their products. He observed textures: dry, hard, thin jelly, gluey, thick, stiff mud, and lump. With colors, he made the most distinctions: milky, cloudy, yellow, pale straw, reddish yellow, pale madeira, red, reddish brown, dirty red, green, bluish green, pearl colored, blue, and transparent. His account of arsenic was the record of a complete investigation, if under "complete" we include the activity of a thinking mind. Cavendish's goal was understanding, which involved hypotheses and explanations.

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[16] James Keir, "Preface," iii, in his translation in 1771 of Macquer's *Dictionary of Chemistry*.

Chemical Apparatus and Laboratory

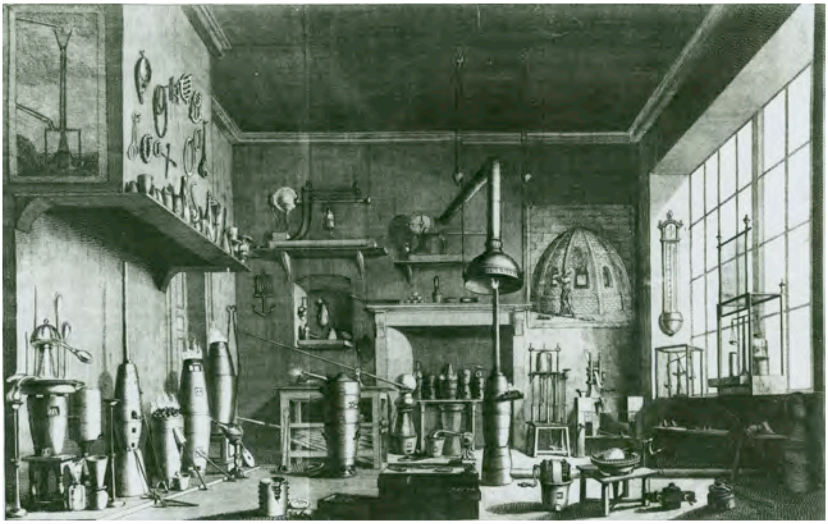

Figure 8.1: Chemical Laboratory. This idealized laboratory with metallurgical furnaces is from William Lewis, *Commercium Philosophico-Technicum* (London, 1756). Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

Figure 8.2: Chemical Laboratory. From Denis Diderot, *Dictionnaire raisonné des arts et des métiers*, 1780. Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

Chemistry in the middle of the eighteenth century was still closely tied to pharmacy, medicine, metallurgy, and manufactures, but it had a strong scientific direction too. A major scientific source was the work of Johann Joachim Becher and Georg Ernst Stahl, who introduced an oily earth given off in combustion and presumed to be present in every combustible body. "Phlogiston," the name given it by Stahl, the Greek word for "inflammable matter," was one of four elements (the other three being water, mercury, and another kind of earth), but because of its common presence in chemical processes, his chemistry came to be identified with *phlogiston*. Stahl and his followers took little notice of the physical properties of substances, and they denied that chemistry had mechanical foundations. The other scientific source of chemistry was Robert Boyle (Fig. 9.2), Newton, and Boerhaave, who regarded chemistry as a branch of physical science that made use of mechanical concepts.[17] Because merit could be seen in both approaches, the chemical and the physical, attempts were made to bring together the "chemist" Stahl with the "physicist" Newton or Boerhaave, a route to a unified chemistry advocated by Macquer, Macquer's collaborator Antoine Baumé, and L.B. Guyton de Morveau.[18] By Cavendish's time, the physical approach to chemistry had incorporated the combustible principle from Stahlian chemistry. Cavendish's approach was physical, and he was a phlogiston chemist.

An advantage of phlogiston chemistry was its unified explanation of combustion and of the calcination of metals (the transformation of metals by intense heating or by chemical combination into a powder having the properties of an earth). When combustibles such as charcoal burn, their phlogiston separates and flies off, the evidence for which is obvious to the senses. When metals, which like combustibles contain phlogiston in combination with another constituent, are calcined they lose their phlogiston, and when the calces are heated with charcoal they reacquire phlogiston, returning to pure metals. Phlogiston, by its presence or its absence, affects most chemical reactions, and by keeping a balance, the chemist could foresee the outcome. The experimental proof of phlogiston seemed incontravertible, the reason why the physical school of chemistry accepted it. However indispensable it was in understanding chemical operations, phlogiston by itself was elusive, thought to be the "least accurately known" of chemical substances or principles and incapable of being isolated and studied on its own.[19] Cavendish would disagree on this important point.

When Cavendish took up chemistry, phlogiston was familiar in Germany, but in Britain and France it was just taking hold. Interest in phlogiston in France was stimulated especially by translations of Becher's and Stahl's writings by Guillaume-François Rouelle and his group in Paris.[20] Rouelle's student Macquer's text on theoretical and practical chemistry in 1758 and Casper Neumann's lectures on chemistry in 1759 were the first accounts of phlogiston in English.[21] Cavendish's colleague Hadley, an early English advocate of phlogiston, said that in preparing his lectures in Cambridge he was "much beholden" to Becher and Stahl. In his lectures in 1758 and 1759, he used the word "phlogiston" throughout.[22]

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[17] Maurice Crosland (1963, 408, 440).

[18] Mi Gyung Kim (2003, 203). Antoine Baumé (1763, 41–44). Crosland (1963, 408).

[19] Thomas Thomson (1830–1831, 2:257–260). Macquer (1771, 2:516).

[20] Thomas L. Hankins (1985, 95). Henry Guerlac (1959, 103).

[21] W.A. Smeaton (1975, 619). Macquer's *Élémens de chymie théorique* (Paris, 1749) and *Élémens de chymie practique* […] (Paris, 1751) were brought out in English translation by Andrew Reid in 1758 as *Elements of the Theory and Practice of Chemistry*. Casper Neumann (1759). Nathan Sivin (1962, 73).

[22] Quotation from p. 8 of Hadley's lectures. L.J.M. Coleby (1952a, 295).

Arsenic

Cavendish's earliest completed chemical research was an experimental study of "arsenic," our arsenious oxide. (His paper was described ominously by one commentator as "Notes on some experiments with arsenic for the use of friends.")[23] Halfway through his laboratory notes the date December 1764 appears.[24] An unnamed reader is referred to in a carefully written draft of his paper on arsenic as "you," who worked with the same substance, "as you tell me you have tried yourself," and who evidently visited Cavendish's laboratory, "particulars of this exper. which I showed you before."[25] Hadley could have been this person, especially since his Cambridge lectures contained an extended discussion of arsenic among the "semi-metals,"[26] qualifying him as an informed reader.

By the time of his experiments on arsenic, Cavendish had been coming to meetings of the Royal Society for about seven years, five years as a member, during which time he had heard few reports or read few papers dealing with chemical topics in the *Philosophical Transactions*, and none relevant to the work in question.[27] The Londoner Cavendish, who was just then setting out on chemical research, would have consulted books and papers from abroad, written in the foreign languages he could read, Latin, French, and German, or else in English translation. His point of departure was the French chemist Pierre Joseph Macquer's discovery and naming of "neutral arsenical salt" (potassium arsenate), which appeared in two papers published by the Paris Royal Academy of Sciences in 1746 and 1748. Macquer's work on arsenic was noticed in Britain; Hadley, for example, took an interest in it.[28]

In this, his most important early work, Macquer distilled arsenic with nitre (potassium nitrate), leaving as residue a compact, white, soluble, mild salt, the neutral arsenical salt. The salt had obvious value for scientific chemistry, and it probably had practical uses, though Macquer doubted that these included medicine despite its actual mildness, since the "name

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[23] Quoted in John Pearson (1983, 118).

[24] The earliest chemical work by Cavendish for which there is an apparently complete record consists of the following: a bundle of 59 numbered pages of laboratory notes on arsenic, with index; a carefully written 25 page version of the account; and 19 unnumbered pages constituting a rough draft. Cavendish Mss II, 1(a), 1(b), and 1(c). A brief description and analysis of these papers is given by Thorpe, in Cavendish, *Sci. Pap.* 2:298–301.

[25] Henry Cavendish, "Arsenic," Cavendish Mss II, 1(b):20, 25.

[26] It was probably sometime after December 1764 that Cavendish wrote or at least completed the paper for "you." To give an idea of the extensiveness of Hadley's familiarity with arsenic, the topics he addressed under "Of Arsenic" in his lectures were: "The Orders of Arsenic; Cobalt, white Pyrites, Orpiment, Realgar. – Of *white*, *yellow*, and *red* Arsenic, and the Method of procuring them – Artificial Realgar, Orpiment fused – Regulus of Arsenic procured from Cobalt by Distillation – Zaffer and Smalts – Sympathetic ink made with Zaffer – Glass rendered Blue by fusing it with Zaffer – Acid of Niter procured by distilling Nitre with Arsenic – The Residuum considered – Arsenic fixed by fusing it with Nitre – Regulus of Arsenic deflagrated with Nitre – White Enamel of Arsenic – Reduction of Arsenic to its Reguline form – Butter, Oil, and Cinnabar of Arsenic, procured by distilling Orpiment with Corrosive Sublimate – Sympathetic Ink from Orpiment and Lime, and its use in discovering the adulterations of Wine by preparations of Lead." Hadley (1758, 17–18).

[27] In the years 1755–64, the *Philosophical Transactions* contained eight papers on "chemical philosophy" and two on "chemical arts," according to the classifications used in the abridgment of the journal, which lists all papers appearing in the full journal. Five other papers were about natural waters, the subject which Cavendish would take up in his second published paper on chemistry.

[28] Pierre Joseph Macquer, "Researches sur l'arsenic. Premier mémoire," and "Second mémoire sur l'arsenic," *Mémoires de l'Académie des Royal Sciences*, 1746 (published 1751), 223–236, and 1748 (published 1752), 35–50. Macquer described this work in 1766 in his *Dictionary of Chemistry*, translated in 1771. The article "Neutral Arsenic Salt" is in vol. 2, 666–667. Shortly before Cavendish's researches on the subject, Macquer's work on arsenic was described in English in an annotation by William Lewis to the translation of Casper Neumann (1759, 143). Coleby (1952a, 301).

of arsenic is so terrible."[29] The agonizing symptoms and fatal consequences of arsenic were mentioned in every book of chemistry. The German chemist Caspar Neumann cautioned that arsenic is a "most violent poison to all animals," so that the "utmost caution is necessary in all operations upon arsenic, to avoid its fumes," which have a "strong fetid smell resembling that of garlic"; and in solution, it has a nauseous taste. Arsenic, it seemed, had no attractive qualities. Little wonder that it, Neumann said, had been "so little examined" by the chemist.[30]

When Cavendish took up the study of arsenic, chemists had not been able to "determine what it really is, or to what class of bodies it belongs."[31] Independently of its noxious properties, arsenic has "singular properties, which render it the only one of its kind." It was the "very singular and extremely different" properties of arsenic from those of other metallic calces that led Macquer to investigate this little-known calx in the first place.[32] Neither fish nor fowl, but something of a flying fish, arsenic behaves like a metal in some states and like a salt in other states. On the one hand, like every metallic calx, "arsenic" can be changed into a metallic form, a "true semi-metal," or "regulus of arsenic," by combining it with phlogiston. On the other hand, like salts, arsenic is soluble in water. Even when it is regarded as a salt, arsenic is uncommon, neither acidic nor alkaline, yet it behaves as if it were an acid.[33] When it is considered as a calx, arsenic differs from other known calces: it is volatile with a strong smell, it is fusible, it unites with metals and semi-metals, and—the difference that Macquer and Cavendish picked up on—it decomposes nitre when distilled with it.[34] From the standpoint of its readiness to unite with other substances, arsenic is exceptional too.[35] Cavendish did not say why he investigated arsenic, but from the state of chemistry at the time, we get an idea of its considerable interest, at once dangerous, difficult, unique, scientifically puzzling, and incompletely known.[36] Its study demanded manipulative skills of a high order, a stiff challenge and testing ground for a young chemist.

In practice, chemistry looked complicated because it dealt with all kinds of matter with a large repertoire of operations. In principle, chemistry looked simple, though this appearance was changing. "Neutral salts," Cavendish's starting point, are a case in point. These were salts composed of acids and other substances that were without acidity, usually alkalis. Not long before, neutral salts could be arranged in a compact table of twelve entries, but when Cavendish began to work with them, the table of neutral salts was fast expanding.[37] The

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[29] Macquer (1771, 1:100, 2:666–667).

[30] Neumann (1759, 145).

[31] Ibid., 140–141. What Neumann, Macquer, Cavendish, and their contemporaries called "arsenic" is a dense, brittle substance with a crystalline or vitreous appearance; this substance, arsenious oxide, is a common byproduct of roasting metallic ores. Another name for it then, as now, is "white arsenic," the calx of regulus of arsenic, the white, shiny semi-metal.

[32] Pierre Joseph Macquer (1758, 1:96).

[33] Macquer (1771, 2:634).

[34] Ibid. 1:99–100.

[35] Arsenic has the least, or next to least, affinity of the soluble substances for the several acids, with the exception of aqua regia. Gellert's "Table of the Solutions of Bodies," at the end of vol. 2 of Macquer's *Dictionary*.

[36] For example, arsenic was soluble in acids, and the results had "not yet been sufficiently examined." Macquer (1771, 1:103).

[37] The Scottish chemist William Cullen's table of twelve neutral salts was reproduced in Donald Monro (1767). Monro, on page 483, pointed out that a table had been published in Germany giving three or four more of these salts, and that there were actually many more because vegetable acid was in reality many acids each with its own neutral salts.

subject of salts in general was recognized as highly undeveloped, with so many "little known, or not even thought of."[38]

Cavendish examined the action of several acids and alkalies on arsenic. He procured Macquer's neutral salt using Macquer's method of distilling arsenic with nitre, noting the misnomer: the salt was slightly acidic, not neutral. He dissolved arsenic in spirit of nitre (nitric acid), and then by adding the alkali pearl ashes (potassium carbonate), he made a discovery: the change that arsenic underwent when dissolved by spirit of nitre made it acidic. To see if he could isolate the acid, he dissolved arsenic in concentrated spirit of nitre (which he called aqua fortis, another name for nitric acid) and then drove off the acid by heat. The experiment succeeded: the residue dissolved in water, which turned acidic (arsenic pentoxide). To be certain that he had an acid, he tried it on other alkalies, calcareous earths, earth of alum, and magnesia, and he tested it with syrup of violets, which turned red, the color of acid. What combined with an alkali to form the neutral salt was not any known acid but "arsenical acid" ("if you will allow me to call it by that name"). The product had "all the properties of an acid," a conclusion Cavendish qualified with an implicit acknowledgment of the fatal reputation of arsenic, "unless perhaps it should fail in respect of taste which I have not thought proper to try." He showed that the crystals formed by dissolving a fixed (non-volatile) alkali in arsenical acid resembled Macquer's neutral arsenical salt. The discovery of an acid was the high point of Cavendish's researches on arsenic.[39] A new acid was important, for few acids were known at the time, and each was a valuable reagent for the chemist.[40]

In going from a first draft to a revised draft of his paper on arsenic, Cavendish made revealing changes of wording. Whereas in the first draft he expressed his opinions such as his differences with Macquer forcefully, in the revised draft he toned them down. Even in the semi-privacy of a correspondence, Cavendish was cautious. In the revised draft, he combined his experiments with a "hypothesis" that explained them; it is significant that he presented the experiments before the hypothesis, for by this time a priori conjectures were not regarded as the way to advance chemistry. The hypothesis was that all metals including the perfect metals are deprived of their phlogiston when dissolved in acids. Associating arsenic with other "metallic substances," which by the phlogiston theory are rich in phlogiston, Cavendish accounted for the changes that arsenic undergoes by the readiness with which the attacking acid, spirit of nitre, unites with the phlogiston in arsenic.[41] In keeping with this explanation, Cavendish concluded that "the whole difference" between arsenic and arsenical

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[38] Macquer (1771, 2:642, 649).

[39] Cavendish, "Arsenic," 1(b), 10, 13. Thorpe, in Cavendish, *Sci. Pap.* 2:299. A.J. Berry (1960, 46–47).

[40] We see the chemist's dependence on many reagents and testing materials in Cavendish's study of arsenic. From his well-supplied laboratory, he made use of (in his spelling) distilled vinegar, spirits of salt (hydrochloric acid), oil of vitriol (sulfuric acid), spirit of nitre (nitric acid), aqua fortis (concentrated nitric acid), nitre, syrup of violet (a botanical extract that changes color when exposed to acids or alkalis), tournsol paper (litmus paper, a mix of dyes that turns color when exposed to acids or alkalis), blue vitriol (copper sulfate), green vitriol (ferrous sulfate), solutions of silver, mercury, copper, and iron in nitric acid, solutions of mercury, copper, and iron in concentrated nitric acid, solution of tin in hydrochloric acid, solutions of gold and nickel in aqua regia (mixture of nitric and hydrochloric acids), solution of regulus of cobalt, sope leys (potassium hydroxide), pearl ashes (potash), fixed alkali (potassium carbonate), calcareous earth (whiting, or carbonate of lime), volatile alkali (ammonia), magnesia, earth of alum, sedative salt (boric acid), white flux, sulphur, linseed oil, and charcoal. Cavendish also had at hand pure "rain" water.

[41] Macquer wrote: "Nothing can equal the impetuosity with which nitrous acid joins itself to phlogiston" (1771, 1:11). Cavendish, "Arsenic," 1(b), 19–20.

acid is that the acid "is more thoroughly deprived of its Phlogiston."[42] The importance of phlogiston in Cavendish's reasoning in chemistry is evident in his earliest research.

We look next at Cavendish's other surviving early chemical research, probably carried out about the same time.[43] The subject was tartar, a hard, thick crust deposited on the sides of wine casks, red or white depending on the color of the wine. Upon purifying, filtering, and crystallizing by evaporation or cold, it forms small, white crystals, "cream of tartar" (potassium hydrogen tartrate), a known acid at the time.[44] Cavendish's interest seems to have been in determining the amounts of alkali in cream of tartar and in soluble tartar (normal potassium tartrate); in the course of his experiments, he isolated tartaric acid. There is a similarity between this problem and the previous one: like arsenic, cream of tartar has a complex nature, a possible reason Cavendish was drawn to them. The stimulus was probably a publication in 1764 by the German chemist Andreas Sigismund Marggraf, who showed that despite its reputation as an acid, tartar contains an alkali.[45] A pupil of Neumann's who was renowned for his precision, Marggraf has been called the "beginner of chemical analysis."[46] An admirer of Marggraf, Hadley said in his chemical lectures that he was "most uncommonly Eminent whether we consider his ingenuity in Contriving, his practical Skill in conducting his Experiments, or his Sagacity and judgment in the Conclusions he draws from them."[47] Cavendish began his chemical researches in contact with one of the best.

In his experiments on tartar, Cavendish made use of equivalent weights. The word "equivalent" was original with him, but the concept went back to the turn of the eighteenth century, to the Dutch physician and natural philosopher Wilhelm Homberg, who introduced equivalent weights as a measure of the quantity and strength of various acids required to neutralize a given quantity of salt of tartar, an alkali. Cavendish determined the quantity of alkali needed to saturate cream of tartar and the equivalent weights of other alkalis, marble and pearl ash (potassium carbonate). Thorpe found Cavendish's work on tartar to be "remarkably accurate."[48]

Both arsenical acid and tartaric acid became known to chemists through publications in the 1770s by the Swedish chemist Carl Wilhelm Scheele, who was celebrated for his discoveries of acids (Figs. 14.9–14.10).[49] If Cavendish had published his experiments on tartar, he would have come before the scientific world as a chemist skilled in chemical synthesis and analysis. Instead he came before it as a pneumatic chemist. Because of his surviving early chemical manuscripts, we can see him move from the one to the other.

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[42] Cavendish made the acid or, in effect, the same thing, the neutral arsenical salt, three ways: distilling arsenic with nitre, dissolving arsenic in concentrated spirit of nitre, and heating arsenic with fixed alkali. All three ways had the same rationale: the effect of exposing a metal (for that is how he regarded arsenic) to an acid or to heat and open air was to deprive it of its phlogiston. "Arsenic," 1(b), 16.

[43] Cavendish performed two sets of experiments on tartar, neither carrying a date, described on unnumbered sheets: "old experiments on tartar," 10 ff., and "new experiments on tartar," 24 ff., plus 6 more sheets. Cavendish Mss II, 2(a) and 2(b), respectively.

[44] Macquer (1771, 1:771–772).

[45] Thorpe, in Cavendish, *Sci. Pap.* 2:301. Cavendish "discovered the true nature of cream of tartar … and its relation to soluble tartar": J.R. Partington (1957, 104).

[46] Thomson (1830–1831, 1:271).

[47] Coleby (1952a, 295).

[48] Thorpe, in Cavendish, *Sci. Pap.* 2:304.

[49] Carl Wilhelm Scheele (1786). Partington (1961–62, 1964, 2:729). Thomson (1830–1831, 2:63). Thorpe surmises that Cavendish's later experiments might have followed Scheele's paper on tartaric acid in 1769, though they could have been earlier, a possible reason he did not publish his own. Cavendish, *Sci. Pap.* 2:302.

Factitious Air

Air was studied scientifically in the seventeenth century by Boyle, J.B. van Helmont, and John Mayow among others, but the branch of chemistry known as pneumatic chemistry did not begin with them. Although some experiments at the time suggested that there were different kinds of air, the early chemists held to the ancient belief of air as an element, and until that belief was seriously questioned, there was little incentive to study the chemical properties of air. Boyle's law relating the pressure and volume of an air was a physical law, which because of its universality reinforced the idea of a single elementary air. The early investigators were also hampered by their inability to collect air in a pure state, a problem which was solved by Stephen Hales early in the next century. From a variety of substances, by means of heat, fermentation, and putrefaction, he freed "fixed air,"or air fixed in liquids and solids, collecting it over water using what he called a "pneumatic trough." When he experimented on air, he measured its volume without however recognizing that airs differ from one another by their solubility in water and by their sources. He studied air quantitatively while ignoring its qualitative features, which he regarded as inessential, because like everyone else at the time he believed in a single air. For this reason the foundation of pneumatic chemistry is usually attributed to Joseph Black, who thirty years later recognized chemically distinct airs.[50] After Black the next major contributors to pneumatic chemistry were the Irish physician David Macbride and Cavendish.

We begin where we left off, with Cavendish's early experiments on tartar. In his *Treatise on [...] Air*, Tiberius Cavallo said that fixed air can be obtained from many substances, giving as examples cream of tartar and salt of tartar, which contain a great quantity of it. As evidence he referred to Cavendish's finding that crystals of salt of tartar contain 423/1000 of their weight of fixed air, and to Priestley's production of 170 ounces by volume of elastic fluid by heating an ounce of cream of tartar, about two thirds of which was fixed air.[51] The release of air from tartar was known to be powerful, capable of bursting into slivers the vessels used in distilling tartar. Cavendish observed "effervescence" in his experiments on tartar. Likewise, in his experiments on arsenic, he observed "effervescence," "air," "vapors," and "fumes." Cavendish did not yet collect airborne substances to be studied in their own right, but in retrospect we see that he was partway to pneumatic chemistry. Direct evidence that his work in pneumatic chemistry connected with his work on arsenic is a theoretical discussion he wrote for his paper on arsenic and rewrote for his paper on factitious air, "On the Solution of Metals in Acids: Digression to Paper on Inflammable Air."[52]

The connection is also evident in his first chemical work to be laid before the Royal Society, in 1764, two years before his paper on factitious air. William Heberden's brother Thomas acquired an alkali from the lip of a volcano, a place where brimstone (sulfur) might be expected but not a salt like the one he found, fossil alkali or natron (a mineral hydrous sodium carbonate). From experiments "made and communicated to me by the Hon. Henry Cavendish," William Heberden set out propositions about ways of making fossil alkali. He said that this alkali differs from the vegetable alkali (potash) by crystallizing upon the addi-

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[50] Aaron J. Ihde (1964, 30–38).

[51] Tiberius Cavallo (1781, 594–596, 606–608).

[52] The title of the paper is not Cavendish's, and in the end he did not publish it. It generalized the conclusion he had arrived at in the published part of his paper on factitious air, which is that acids deprive metals of their phlogiston, which flies off with the acid. His earliest chemical experiments on arsenic have substantial overlap with his study of factitious air through their common concern with phlogiston, metals, acids, and aerial substances.

tion of fixed air (carbon dioxide), and here he cited Black's experiments on magnesia alba (magnesium carbonate), the second to do so, it would seem, just after Macbride. In quotation marks, Heberden stated Cavendish's conclusion, a comparison between fossil and vegetable alkali, finding that the latter has a stronger affinity to the mineral acids than the fossil alkali. It is conceivable that in his chemical examination of a mineral for Heberden, Cavendish's thoughts were directed to pneumatic chemistry. Another possible connection is with his study of tartar: one of his experiments for Heberden included a compound of tartar.[53] To this point in his life, when undertaking something new, Cavendish had always made the first move with his father; this time, coming into print, it was with his father's close friend, another eminent member of the Royal Society, Heberden.

We can see why Joseph Black was important to Cavendish (Fig. 14.5). In 1756 he published an enlarged version of his medical thesis at the University of Edinburgh on magnesia alba. He selected his subject, magnesia alba, to learn if he could acquire a lime water from it that was more effective than the lime water then in medical use. When he found that magnesia did not form a lime water, he abandoned his original project to focus instead on the interesting chemistry of the substance. Twenty-seven years old and an expert experimenter, Black had an advantage Cavendish did not, a great teacher, William Cullen. If Cavendish's father was in some ways an equivalent, there is no evidence that he was particularly drawn to chemistry. Cullen regarded chemistry as a branch of natural philosophy with laws as fixed as those of mechanics, and Black's work in chemistry agrees with this. Like Cavendish, Black was an admirer of Macquer, recommending his text to his students, and of Marggraf, whose essays he said he would rather have written than anything else in the library of chemistry. *Experiments upon Magnesia Alba* was Black's major publication, on which his chemical reputation was based.[54]

Black and Cavendish were similar in a number of ways. Both were methodical, unaffected, cautious in their reasoning, exacting in their research, and alert to careless error. Cavendish was rich, and Black was well-to-do. Both led outwardly uneventful lives. Both made chemistry and heat major fields of research, and in both fields they began with the same subjects, factitious air and specific and latent heats. Both were reluctant to publish, Black even more so than Cavendish. They both shirked correspondence. Otherwise, in their dealings with people, they were not alike. Cavendish was difficult to engage in conversation, and uninterested in any subject that was not scientific. Black was affable, always ready to enter into conversation, serious or trivial. For the whole of his career, Black was a professor, who lectured on his discoveries. If Cavendish had been a professor, his researches, like Black's, would have been spread by his students, and he would have had greater influence on the course of science in the eighteenth century. So far as we know, Black and Cavendish never met.[55]

Black's originality began with his observation that when subjected to fire, magnesia alba loses a substantial proportion of its weight and that the lost portion is mainly a kind of air, or gas (carbon dioxide); he further observed that the loss of weight is recovered when the calcined magnesia alba, a caustic substance he called magnesia usta (magnesium oxide), is recombined with the same air. He showed that this same air, "fixed air" (Hales's term), is found in other alkalis such as chalk (calcium carbonate); when caustic quicklime, which is

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[53] William Heberden (1765). This paper was read at the Royal Society on 7 Feb. 1764.

[54] William Ramsay (1918, 4–5, 14–15). Henry Guerlac (1957, 433–434).

[55] Ramsay (1918, 1–2, 114–115, 133).

produced by calcining chalk with heat, is combined with fixed air (not directly but through a series of steps involving slaked lime, potash, and caustic potash), the chalk is recovered. Black performed an experiment that showed that the air contained in calcareous earths such as chalk is chemically distinct from common air, a novel claim. Beyond that, he had little to say about the properties of the new air, but he recognized in it a widening field for research. He said that the air would probably be the "subject of my further inquiry," but he did not get to it, leaving the field to Cavendish and others. Black's study is significant for proving by means of careful weighing that an elastic fluid is fixed in exact proportions in magnesia alba and related substances. More than anyone before him, Black used the chemical balance to advantage, and in this respect too Cavendish was to follow in his footsteps.[56]

Cavendish's first scientific publication under his own name appeared in 1766 in the *Philosophical Transactions*, an exacting investigation of an experimental field, pneumatic chemistry. Coming ten years after Black's publication on magnesia alba, Cavendish's paper was the next major study of elastic fluids fixed in substances. Called the "first true disciple" of Black's, Cavendish recognized what was important in Black's work and carried it further, introducing novel methods for distinguishing airs and determining their properties. His paper of 1766 "marked the beginning of the systematic study of gases."[57]

For the kind of study it was, Cavendish's paper was unusual, as a glance at the journal shows. His paper was preceded by one by John Michell on determining the degree of longitude at the equator and by a paper on an uncommonly large hernia and followed by an account of the Polish cochineal and four more papers about animals. Cavendish's second paper, in 1767, appeared in similar mixed company: an account of men "eight feet tall, most considerably more" observed near the Straits of Magellan in the country of Patagonia, an account of a locked jaw and a paralysis cured by electricity, and an account of a meteor and another about a swarm of gnats seen at Oxford. In the context, Cavendish's reports of laboratory precision were perhaps the most remarkable.

Instead of the term "factitious" air, Cavendish could have used "fixed," since the usual meaning of "fixed air" then was any sort of air contained in bodies, but he wanted to retain the specific meaning for "fixed air" that Black had used for the air he studied. To avoid confusion Cavendish borrowed Boyle's expression "factitious air," by which he meant "any kind of air which is contained in other bodies in an elastic state, and is produced from thence by art."[58] The names Boyle and Black are revealing. For his work on arsenic and tartar Cavendish's sources were foreign chemists, while in his paper on factitious air and the related paper the next year on fixed air in mineral water, they were British: in addition to Boyle and Black, they were Cotes, Hales, Macbride, and Brownrigg.[59] In the new field, British chemists took the lead.

The paper was three papers published as one, as the title says, "Three Papers, Containing Experiments on Factitious Air." The first paper was received by the Royal Society on 12 May and read on 29 May 1766, on the eve of the long summer recess, and the second and third papers were read on two successive meetings after the recess, on 6 and 13 November.

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[56] Henry Guerlac (1970, 2:173–183).

[57] Guerlac (1957, 454–456).

[58] Cavendish (1766, 77). Black gave a fuller description of "factitious air." "Chemists have often observed, in their distillations, that part of the body has vanished from their senses, notwithstanding the utmost care to retain it; and they have always found, upon further inquiry, that subtle part to be air, which having been imprisoned in the body, under a solid form, was set free and rendered fluid and elastic by the fire." Joseph Black (1898, 16).

[59] Cavendish (1766, 83, 95–96; 1767, 105).

Cavendish drafted a fourth paper but withheld it. The papers, the three published ones and the unpublished fourth, formed a series, their experiments relating to each other by subject, method, apparatus, and theory. Each addressed a certain kind of factitious air produced by certain kinds of processes: inflammable air from metals and acids; fixed air from alkalis by solution in acids and by calcination; mixed airs from organic substances by fermentation and putrefaction; and other mixed airs from organic substances released by distillation. Within the text, the four divisions are called "parts" rather than "papers"; adopting that terminology, we refer to the publication as one paper with four parts.

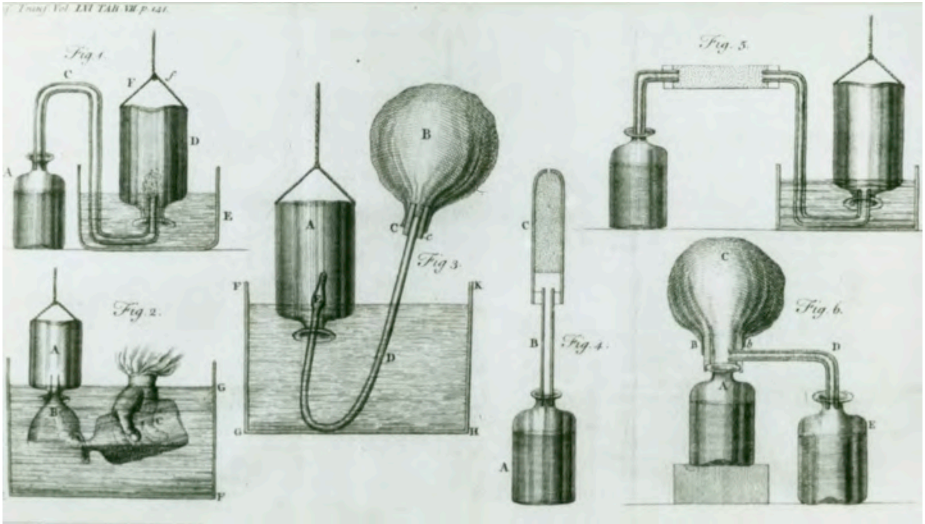

Figure 8.3: Factitious Air Apparatus. The numbered figures are from Cavendish's first publication, for which he received the Royal Society's Copley Medal. Figure 1 shows his technique for filling a bottle D with air. The bottle, containing water, is inverted in the vessel of water E; the air to be captured is generated by dissolving metals by acids and by other means in bottle A. The measure of quantity of air is the weight of the water it displaces in D. Figure 2 shows how air is transferred from one bottle to another. Figure 3 shows how air is withdrawn from a bottle by means of a bladder. The speckled substance in Figures 4 and 5 is dry pearl ash, through which air is passed to free it from water and acid. Cavendish (1766).

Cavendish's techniques for collecting and transferring inflammable and other airs are seen in his drawings (Fig. 8.3). In both spirit of salt (hydrochloric acid) and dilute oil of vitriol (sulfuric acid), he dissolved each of three metals, zinc, iron, and tin, and investigated the air that was released. He found that it was insoluble in water, allowing him to collect it in vessels inverted over water, adapting Hales's pneumatic trough. He assumed that the air came from the metal not the acid, a teaching of the phlogiston theory. The volume of air released depended on the metal, and the air in each case was permanently elastic. In the presence of common air, the new air exploded when lit, a property he investigated further, comparing the loudness of the explosions when the air was mixed with common air in different proportions. He determined the density of the air two ways: one was to weigh a bladder filled with the air and again with it empty, noting the increase of weight (in the case of an air that is lighter than common air); the second way was to note the loss of weight of the combined acid and metal when the discharged air was allowed to escape. He compared the density of several samples of the air obtained using different metals and acids with the density of water and the density of common air, concluding from a mean of his experiments that the air was "8760 times lighter than water, or eleven times lighter than common air,"which given his method is surprisingly close to our value 14.4. When the air was kept in bottles inverted over water, it was capable of holding "near 1/9 its weight of moisture," making the specific gravity of the moist air "7840 times less than that of water."[60] These figures and others served to specify the physical properties of a substance to which Cavendish gave the name "inflammable air," which again was not original. When Cavendish dissolved metals in concentrated instead of dilute oil of vitriol with the aid of heat, he obtained a non-inflammable air, which he regarded as a compound of the acid and phlogiston, the acid depriving the phlogiston of its inflammability, incidently contradicting Stahl.[61] On the day the first part of Cavendish's paper was read, the secretary of the Royal Society wrote in the Journal Book that "it is impossible to do Justice to the Experiments under the title 'On Inflammable Air' without reciting them wholly."[62] We agree with the secretary.

Part II of Cavendish's paper is about "fixed air," the factitious air released by alkalis when dissolved in acids or calcined, our carbon dioxide. As he had inflammable air, he examined fixed air for elasticity, density, solubility in water and in other liquids, and combustibility. Otherwise than being permanently elastic, fixed air had properties distinct from those of inflammable air and common air: it was 1½ times heavier than ordinary air, which being heavier than inflammable air was easier to work with; it did not support fire; it was soluble in water, because of which Cavendish collected it over mercury or caught it directly. Its solubility in water varied, suggesting to him that fixed air obtained from marble "consists of substances of different natures." He determined the quantity of fixed air in several alkaline substances, expressing the results in terms of marble. His use of marble as a standard is shone by the following typical statement: a parcel of volatile sal ammoniac "contained more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of… 217 to 100."[63]

Cavendish's point of departure in Part III was a study of fermented and putrefied substances by Macbride in 1764. Finding that "fixed air" was given off, Macbride concluded that this air plays an essential role as the cement of living bodies. He took his understanding of air from Hales, and in citing Black, he made Black's apparatus and work better known. This was his main contribution to pneumatic chemistry, his interest in the subject being primarily medical and physiological.[64] Cavendish wanted to know if fermentation and putrefaction yielded any factitious air other than what Macbride found, Black's fixed air. He discovered that the air produced by fermenting brown sugar and apple juice with yeast was the same as that produced from marble by solution in acids, "fixed air." The air he ob-

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[60] Ibid., 84–86.

[61] Stahl thought that a compound of phlogiston and an acid was inflammable. Thomson (1830–1831, 2:340).

[62] 29 May 1766, JB, Royal Society 25:876.

[63] Cavendish (1766, 89, 91, 93).

[64] E.L. Scott (1970, 46). Macbride's *Experimental Essays* were published in 1764. Guerlac (1957, 454).

tained from putrefying gravy broth and raw meat he found to be a mixture of fixed air and inflammable air, neither pure.[65]

In Part IV, Cavendish again treated vegetable and animal substances, this time distilling wood, tartar, and hartshorn, obtaining a mixture of non-flammable and inflammable airs. He found that the new inflammable air differed from the inflammable air produced by dissolving metals in acids, his test being the loudness of explosions when the air was mixed with ordinary air and lit. He completed Part IV after writing his second published paper, on a mineral water, since he referred to it there; if he had published it, it would not have appeared with "Three Papers," but later. He said that he intended to follow up Part IV with another publication. His laboratory notes indicate that he returned to this subject later but with no more conclusiveness.[66]

For his experiments on factitious air, Cavendish was awarded the Copley Medal of the Royal Society. Two others received the Copley Medal that year with him, Brownrigg for his analysis of mineral water and Edward Delaval for his study of the colors of metal films. Delaval showed that thin metal deposits on glass differed in color in the order of their density, a study which could be called chemical optics.[67] The year 1766 was the year of the chemists.

In Cavendish's study of factitious air, we see characteristics that will reappear in his later work. One is caution, shown by his wording. The inflammable air produced by putrefaction was "nearly of the same kind" as the inflammable air from metals but "not exactly the same."[68] An intended addendum to Part I is tentatively expressed, "I have not indeed made sufficient experiments to speak quite positively as to this point."[69] Another characteristic is patience; Cavendish inverted a flask of fixed air over mercury "upwards of a year."[70] Another is a mix of quantitative and qualitative methods, weighing air being an example of the former, judging the loudness of explosions an example of the latter. A related characteristic is his focus on physical properties: in addition to loudness, these were elasticity, solubility, and density. Another characteristic is thoroughness: in generating airs, he made use of a range of metals, acids, alkalies, and organic substances. Another is his use of equivalent weights: he measured the volumes of inflammable air from one ounce of each of three metals, from which the equivalent weights of the three metals can be found by assuming a constant volume of the air.[71] Other characteristics have to do with accuracy. He introduced a standard, marble, which he used to express the amount of fixed air in an alkali. He repeated his experiments and took the mean of the results. He estimated accuracies quantitatively: in determining how much fixed air water absorbs, his accuracy was "about three or four 1000th parts of the whole bulk of air introduced."[72] He claimed no greater accuracy for his conclusions than was justified by his experiments: he gave the specific gravities of inflammable and fixed airs to three places, the maximum accuracy for measurements of that

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[65] Cavendish (1766, 98–100).

[66] Henry Cavendish, "Experiments on Air. Part IV," *Sci. Pap.* 2:307–315.

[67] Edward Delaval (1765).

[68] Cavendish (1766, 100).

[69] Cavendish, "On the Solution of Metals in Acids," 305.

[70] Cavendish (1766, 88).

[71] Berry (1960, 51).

[72] Cavendish (1766, 89).

sort.[73] A final characteristic is his use of theory as a guide in his experiments, which brings us to phlogiston.

We look at Cavendish's view of phlogiston at the time of his early work in chemistry. In his paper of 1766, he wrote that when certain metals and acids react, the phlogiston of the metals flies off "without having its nature changed by the acid, and forms inflammable air."[74] Whichever metal he tried, iron, zinc, or tin, and whichever acid, dilute sulfuric or muiatic, he obtained the same air. Thomas Thomson understood Cavendish to have concluded from this that inflammable air from a metal is pure phlogiston.[75] Vernon Harcourt, a later chemist who studied Cavendish's work historically, concluded that Cavendish identified phlogiston with inflammable air "as early as 1766, or very soon after." Cavendish found that there is more than one species of inflammable air, but since the one he obtained from zinc and iron had a constant specific gravity and was constant in its combining properties, "*his Phlogiston* therefore *was* hydrogen and nothing else."[76] The identification of phlogiston in its elastic state with inflammable air is consistent with the experiments he reported in his paper of 1766.

A counter argument can be made. First, there was Cavendish's cautious wording: in 1766 he wrote that phlogiston "forms," not "is," inflammable air. Second, chemists who later identified phlogiston with inflammable air did not credit Cavendish with the idea. In 1782, Richard Kirwan having explained the origin of inflammable air much as Cavendish did went on to prove its "identity and homogeneity with phlogiston," though he also associated phlogiston with Black's fluid of heat, which Cavendish rejected.[77] In 1783, guided by experiments of his own, Joseph Priestley identified phlogiston with inflammable air.[78] What exactly Cavendish thought about the relationship of phlogiston and inflammable air at the time of his first paper we may never know for certain, and Cavendish himself may have believed that his experiments were not decisive on this point. What seems clear is that he was not in serious doubt about the reality of "phlogiston" and its importance in chemistry, as he would later be. In a footnote in Part IV he cited John Hadley, who explained the increase in weight of a metal upon calcination (oxidation) by the absorption of fixed air (carbon dioxide), forestalling a potential and eventually serious difficulty for phlogiston.[79]

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[73] The notion of significant figures had not taken hold everywhere. The chemist William Nicholson said that the best chemical balances were accurate to five or six places, according to claims made for them. In weighing an air, the error was thirty times as great in proportion to the whole as it was in weighing other substances. This means that if a balance was accurate to five places in common weighing, it was accurate to only three places in the case of an air, and because of the complications of temperature and pressure, the accuracy was probably less than three places. Lavoisier nonetheless gave the specific gravities of airs to five places, on which he made calculations to six or eight places, thousands of times their real accuracy in, what James Short (above) called a "pretense" of accuracy. Nicholson's comments in his translation of the notes by French chemists to the French edition of Richard Kirwan (1789, vii–ix).

[74] Cavendish (1766, 79).

[75] Thomson (1830–1831, 2:340).

[76] W. Vernon Harcourt (1839, 28).

[77] Richard Kirwan (1782, 195–197).

[78] Joseph Priestley (1783, 400).

[79] In the footnote, Cavendish says that Hadley distilled the salt sal ammoniac with red lead, or lead oxide, and also with bare metal, and that the different results show that metals contain no fixed air, or carbon dioxide, and that metallic calces, or oxides, contain a great deal. He says that the reason that minium, another name for lead oxide, weighs more than the bare metal lead is that lead absorbs fixed air on being converted into minium. In the manuscript of Hadley's lectures, we find what Cavendish refers to here: Hadley says that 100 pounds of lead give 110 pounds of minium, and that the increased weight is due to the fixed air united to the minium. The reference to Hadley

Following the work of Black, in his first published paper Cavendish helped to discredit the ancient idea of a single, a universal air. He showed that inflammable air and fixed air differ from one another and from common air, and that one of them, inflammable air is a single, uniform substance. He failed to recognize that like inflammable air, fixed air is a single substance, but the incompleteness of his analysis of this and other kinds of air only reveal the difficulty of the field at this early stage. His contribution to pneumatic chemistry was to have made the first attempt "to collect the different kinds of air, and endeavor to ascertain their nature."[80] By introducing methods for isolating and characterizing different kinds of air, he provided a "model to future experimenters," opening new avenues for research. The Scottish chemist Thomas Thomson, who was inspired by Black to take up the study of chemistry, wrote that Cavendish "first began the true investigation of gases," extending the bounds of pneumatic chemistry, with the caution and precision of a Newton."[81]

Cavendish's contribution to pneumatic chemistry can be contrasted to Priestley's. He did not discover new airs, which in any case was not his objective. An example makes the point. In the course of an experiment, he dissolved copper in muriatic acid (HCl) assisted by heat, producing an air that was soluble and not inflammable air, a new kind of air, but he did not examine it further. When Priestley read about this "remarkable kind of air" in Cavendish's paper, he "was exceedingly desirous of making myself acquainted with it." He collected the air over mercury and performed experiments on it, discovering a new air, "muriatic acid gas."[82] The air that Cavendish studied most thoroughly, and which he is most closely identified with, inflammable air, he did not discover; it had been known from Boyle's time, though it was confused with other airs we can identify now.

In the following year, 1767, Cavendish published an analysis of water obtained from a location near Soho Square, Rathbone-Place.[83] Having a practical use, mineral water was a familiar object of chemical study, though Cavendish's interest would seem to have been purely scientific. The chemist William Lewis wrote in 1759 that the analysis of mineral waters was held back by a great many experiments "more ostentatious than useful" and "for the most part fallacious," very different waters giving similar appearances because of faulty methods. He laid out a "simple and obvious method" of going about the analysis: first distill the mineral water, then separately analyze the distilled water and the residuum, which consists of soluble salts and insoluble earths, and lastly separate the salts by crystallization or directly by adding chemicals.[84] Cavendish's first two experiments followed these steps exactly, but the other experiments were about fixed air, calling for methods appropriate to this elastic substance.

The occasion for his study would seem to have been a paper in the *Philosophical Transactions* in 1765 by William Brownrigg, whom we mention earlier in the book where we dis-

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shows that Cavendish and Hadley were aware that the increase in weight on the calcination (oxidation) of metals was a problem and that phlogiston, as they understood it, could not solve it: they thought (incorrectly) that fixed air (carbon dioxide) was the explanation. Hadley's statement is based on Macquer's book on the elements of chemistry, though Macquer does not give an explanation for the increase in weight, commenting only on the "numerous ingenious but not altogether satisfying explanations." Hadley's explanation takes into account the experiments on airs by Stephen Hales and Joseph Black. Page 208 of the manuscript lectures, quoted in Coleby (1952a, 299).

[80] A.L. Donovan (1975, 219). J.R. Partington (1961–62, 3:316).

[81] Thomson (1830–1831, 2:1, 343).

[82] Ibid. 2:341. Joseph Priestley (1772b, 234–235).

[83] Henry Cavendish (1767).

[84] William Lewis, in Neumann (1759, 252–253).

cuss Charles Cavendish's executorship of the Lowther estate in Cumberland. Brownrigg has a place in the early history of pneumatic chemistry, which if not of equal importance to that of Black, Macbride, and Cavendish merits our attention all the same. His distinction is to have been the first to undertake a systematic study of dangerous air in coal mines. A native of Cumberland, he studied medicine in Leiden while Boerhaave was teaching, obtaining a doctorate there, and upon his return he set up practice in Whitehaven, in a coal-mining region. He married the daughter of John Spedding, steward to the estate of Sir James Lowther, whose personal physician he became. A few years earlier, in 1737, an explosion in one of Lowther's coal mines killed nearly two dozen men, and Brownrigg treated the injured, the background to his interest in two related questions, how to prevent explosions in mines, and how to treat miners who were poisoned by the fumes. In 1733 and 1736, he developed ways of transferring and collecting coal "damps" and provided Lowther with bladders filled with it to submit to the Royal Society.[85] In 1741 and 1742 Brownrigg presented a series of papers to the Society on explosive "fulminating damp" and on suffocating "choak-damp," on the basis of which he was elected to the Royal Society. With the backing of Lowther's colliery steward Carlise Spedding, in 1743 he proposed setting up a laboratory near one of the pits for him to carry out experiments on explosive and poisonous airs. Lowther agreed to pay half the cost of it. After a visit to a spa in Europe, Brownrigg prepared a paper on the air released from the water he found there, which he identified with the choke damp he had been studying, a "particular kind of air, or permanently elastic fluid" distinct from common air. He speculated correctly that the repulsive particles released from various kinds of dense bodies vary from one another, often composing "elastic fluids, which differ as much from each other, as those bodies differ from which they are produced…. So that two elastic fluids, although they both possess a repulsive quality, may yet in their other qualities differ as much as inelastic fluids [vapours] are found to differ." He had a clear notion of chemically distinct airs, the insight of pneumatic chemistry. His paper on the spa water, an extension of a paper read to the Royal Society in 1741, was published in the *Philosophical Transactions* in 1765 and awarded the Copley Medal the following year.[86] Cavendish would have been interested in Brownrigg's paper about air in mines and in mineral water, which was what his paper in 1767 was mainly about. Further evidence of his interest is a paper on damps written by Brownrigg for Lowther found among Cavendish's manuscripts.[87]

Produced by a spring, Rathbone-Place water until a few years before had been raised by an engine for public distribution in the neighborhood. Now a pump remained, from which Cavendish drew his sample, which he described as "foul to the eye," forming a "scurf" over time. To see if what Brownrigg found in the spa water was true of Rathbone-Place water, Cavendish evaporated a sample of it and analyzed the airs given off. Separating off the fixed air, he mixed the remaining air with inflammable air and lit it. From the loudness of the explosion, he determined that the water contained a quantity of ordinary air as well as a quantity of fixed air. He arrived at the answer to the question he began with: the reason for the suspension of calcareous earth in the water was "its being united to more than its

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[85] This was in 1733. "Sir James Lowther, 4th Baronet." Anon.,"William Brownrigg" (https://en.wikipedia.org/wiki/William_Brownrigg). Thomas Young (1816–1824, 436).

[86] William Brownrigg (1765, 218–219, 238); on 336–343 is an extract from a paper read to the Royal Society in 1741, from which the new paper was written. J.V. Beckett (1977a, 255–258). J. Russell-Wood (1950, 436–438).

[87] "Some Observations upon the Several Damps in the Coal Mines near Whitehaven by Dr Willm Brownrig Phisitian of that Town Communicated by Him to Sr James Lowther Bart," Cavendish Scientific Manuscripts, Devon. Coll., Chatsworth, Misc. Hereafter Cavendish Mss.

natural proportion of fixed air." When the fixed air was driven off, the earth was immediately precipitated.[88] Cavendish's examination of solubilities (of certain bicarbonates) can be seen as a continuation of his study of fixed airs. His analysis of Rathbone-Place water listed the impurities by weight in one pint of the water: fixed air, unneutralized earth (magnesium and calcareous earth), volatile alkali, selenite, and a mixture of sea salt and Epsom salt, the total solid contents coming to $17\frac{1}{2}$ grains. Cavendish concluded his study by examining three other London waters, including water from a pump near his father's house on Great Marlborough Street.

Cavendish's analysis of a mineral water was the first that could claim "tolerable accuracy," Thomson said.[89] Writing about the analysis of waters a few years later, the Swedish chemist Torbern Bergman said that it was "one of the most difficult problems in chemistry" because there were so many impurities in the water and the quantities were so small.[90] It was a problem to show Cavendish's skills as a chemist once again.

Instruments and Meteorology

By Cavendish's time, the craft of instrument making was highly advanced. Aided by improvements in materials and the graduation of scales, instrument makers kept up with (and stimulated) the demand for better instruments.[91] Living in a city with a flourishing trade in instruments, Cavendish could conveniently inspect, buy, and commission the thermometers, telescopes, and other tools he needed for his research. At some stage, he employed an instrument maker of his own. His interest and skill were recognized by the Royal Society, which regarded him as its resident authority on matters having to do with instruments of all kinds.

Because he was wealthy, Cavendish could buy any instrument he wanted, and because his scientific interests were wide-ranging, he owned a large number of them. In 1816, six years after his death, his collection was put up for auction. At the time, Cavendish was too recent for his instruments to be collected as memorabilia, and his name was not mentioned in the auction catalog, only a "Gentleman Deceased." The makers of the instruments not their owners were important to buyers: an air pump by Nairne and Blunt, a thermometer by John Bird, and a theodolite by Jesse Ramsden. Because the instruments used by Cavendish in the 1780s were still in use at the time of the sale, the unnamed buyers would have been persons with a scientific object. By the time of the auction, the collection had been well picked over, leaving behind a miscellany, telescopes, hygrometers, and thermometers (forty-four of them). The catalog lists ninety-one numbered items, some of which are multiple; all told, it lists 150 instruments together with bottles, retorts, and maps. At the time of Cavendish's death, his instruments were valued at £544; at the auction sale, they brought £159, a measure of the depletion of his collection by then.[92]

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[88] Cavendish (1767, 105, 107).

[89] Thomson (1830–1831, 2:344). Berry writes, "Truly indeed was Cavendish the founder of water analysis." (1960, 57).

[90] Torbern Bergman (1784, 109).

[91] Maurice Daumas (1963, 421–424).

[92] "Extracts from Valuations of Furniture," *A Catalogue of Sundry Very Curious and Valuable Mathematical, Philosophical, and Optical Instruments ... Of a Gentleman Deceased ... On Saturday the Fifteenth of June 1816, at Twelve O'clock*, Devon. Coll.

Accurate measurements in Cavendish's main experimental fields, electricity, chemistry, and heat, and in his main observational field, meteorology, began to become important around the time he began to do research, the 1760s and 70s. Researchers did not yet depend on great accuracy in their measurements, but physical theory, quantifiable concepts, and standards of work all pointed in that direction.[93] Colleagues considered Cavendish to be accurate in his work, by which they meant that he took care to come as close to the truth as was possible given the means available to him. They understood that what constituted accuracy and precision varied over time.

All instruments are imperfect in their infancy, J.A. Deluc said, and though they never achieve perfection, they approach ever nearer to it; the ordinary watch becomes Harrison's precise timekeeper, and the ordinary balance becomes the precise scales of the chemist.[94] The gradual approach to perfection was the instrument maker Jesse Ramsden's guide to practice: sensible that the "theory" of astronomy was held back not by the nature of its instruments but by their imperfection, he was "always inclined to improve rather than invent," except when he was convinced that the imperfection of an instrument lay in the principle of its construction.[95] Cavendish implicitly agreed with Ramsden, for he too was an improver of instruments, not an inventor.

To see how Cavendish worked with instruments, we consider those he used in studying the weather. His colleague Richard Kirwan traced the origins of the science to the invention of the thermometer and barometer, attributing its slow development to the imperfections of the instruments and also to the interruptions of the historical record of the weather. He intended his book as a step in the direction of a "theory of the winds," which he regarded as the object of meteorology, the first step of which was to connect the diverse phenomena of the weather by taking measurements of the weather at all latitudes and longitudes in both hemispheres. The single most important measurement of the weather is the temperature, which causes the winds, which in turn affects the temperature, determining the "state of the atmosphere." The science of the weather differed from most other sciences in that it did not enable people to "alter the spontaneous course of nature, except in a very few cases," such as in the promotion of vegetation and the drainage of morasses. In this respect, it was like astronomy, and like astronomy, which predicts the motions of the planets, a perfected meteorology would "foresee those changes [in the weather] we could not prevent."[96] We have no way of knowing if Cavendish's understanding of meteorology differed in any important way from Kirwan's, but we know that he regarded the science in its current state as incapable of prediction, unlike astronomy. His brother Frederick told him that he read in the paper that Herschel predicted a wet end-of-summer. Henry, who had read the paper too, told his brother that Herschel could have said no such thing since he had "too much sense to make predictions of the weather."[97] Henry knew his astronomical colleague Herschel, who earlier complained that the "papers have ascribed to me a foreknowledge of the weather […] which I am not so happy as to be in possession of."[98]

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[93] Daumas (1963, 418, 428–430).

[94] Jean André Deluc (1773, 430–432).

[95] Jesse Ramsden (1779, 419).

[96] Richard Kirwan (1787, v–vi).

[97] Frederick Cavendish to Henry Cavendish 10 Sep. 1809; Henry Cavendish to Frederick Cavendish, n.d., draft; in Russell McCormmach (2014, 260).

[98] William Herschel to Lord Salisbury, late Jan. 1789, Royal Astronomical Society, Mss Herschel, W 1/1, 170–171.

Like many other serious students of the weather before and after him, Cavendish designed a better wind measurer. Having commissioned the firm of Nairne and Blunt to build it, he requested the employee who made the instrument to be present when he came to pick it up. Cavendish "insisted upon his taking the whole apparatus to pieces, and then, by means of a file and a magnifying glass, he tested the pinions to see that they were properly hardened and polished, and of the right shape, according to his written directions."[99] We suppose that during the inspection of the pinions, the instrument maker felt some anxiety, but since the account ends here, we also suppose that the outcome was favorable to all parties. At Nairne and Blunt's, Cavendish was both a demanding customer and a frequent one, whose behavior would have been familiar and more than tolerated, his patronage of the firm serving as an advertisement for it. Edward Nairne was Cavendish's all-purpose instrument maker of choice, and also an experimental collaborator of his and fellow of the Royal Society. Thomas Blunt began as an apprentice to Nairne and then became a partner.[100]

A specific reason why Cavendish commissioned Nairne and Blunt to build a wind measurer may have been that they had recently built a portable wind gauge for use at sea for James Lind, physician to George III. This instrument was the best of its kind, which was the kind of nearly all early wind gauges. They were, in effect, pressure gauges, used by seamen who were interested in that property of the wind, its pressure.[101] The inspiration of Cavendish's earliest experiments may have come from Alexander Brice, who measured the velocity of wind by observing the motion of the shadows of clouds, his answer to the irregularities in the velocity of wind as determined by light objects such as feathers carried along in the breeze.[102] Cavendish thought that Brice's experiments published in the *Philosophical Transactions* in 1766 were "ingenious" but incomplete, since he failed to measure the wind on the ground in an open place to discover if there is a difference in wind velocity at the surface of the Earth and high above it, and he also failed to observe the angular velocity of the clouds at the same time as he observed their shadows, which would have determined their perpendicular altitude. "The most convenient way I know of measuring the velocity of the wind," Cavendish wrote to an unnamed correspondent, "is by a kind of horizontal windmill with rack work like that used for measuring wheels to count the number of revolutions it makes…. it will be easy finding by experiment the actual number of revolutions which it makes while the wind moves over a given space."[103] Cavendish's wind measurer was a horizontal windmill, built nearly on the scale of the familiar vertical windmill with the revolving arm measuring eighteen feet. This was the kind of instrument Cavendish commissioned Nairne and Blunt to build, described as "a train of wheels worked by a vaned fly."[104] It was of a different kind of wind measurer than the seamens' pressure gauges, one suited for meteorology in the tradition of the vane-mill (re)invented by Robert Hooke in the previous century.[105] Because Cavendish's method was to count the number of revolutions

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[99] The account of Cavendish originated with the instrument maker John Newman, of Regent Street, in Wilson (1851, 179).

[100] On Edward Nairne and Thomas Blunt: E.G.R. Taylor (1966, 62, 214, 256).

[101] A. Wolf (1961, 1:320–323).

[102] Wolf (1961, 1:324).

[103] Henry Cavendish to "your Lordship," undated, Cavendish Mss, Msc.

[104] Wilson (1851, 179).

[105] William E. Knowles Middleton (1969, 203). Before Robert Hooke, the Italian architect Leon Battista Alberti invented a mechanical wind measurer, consisting of a disc oriented perpendicularly to the wind mounted on an arm free to rotate. Hooke's device was similar.

corresponding to winds of different strengths, the accuracy of the pinions he insisted on inspecting at Nairne and Blunt's was key to the accuracy of the instrument across a wide range of wind velocities. Among his manuscripts are trials of the "Measurer of Wind" with dates scattered through them, in 1768–69, and twenty years later, in 1788.[106] He described the capability of the wind measurer: "By the help of such an instrument one might easily find the velocity of the wind at any time & if one had a mind to keep a register of its velocity almost as easily as one can that of the thermometer."[107] Ideally, a complete weather journal would record the velocity of the wind in addition to its direction, which was then routinely observed by the weather vane. Complex and cumbersome wind measurers were invented and reinvented throughout the century, without leading to a standard practice. By the procedures recommended by Cavendish for recording the weather at the Royal Society, the strength of the wind was denoted numerically, but only by rank: 0, 1, 2, and 3 stood for "no wind," "gentle," "brisk," and "violent or stormy."[108] To determine the strength, Cavendish advised observing how smoke was blown or listening to how the wind sounded,[109] a qualitative estimate. Like other patient observers of the weather, Cavendish probably desired greater exactness and settled for less.

There had long been instruments for tracking the weather—weather vane, rain catch, and even a crude indicator of humidity—but these did not make the study of the weather scientific. By Cavendish's time, it was understood that a science of the weather required measuring instruments capable of reasonable accuracy. Besides the barometer, the most important of these was the thermometer,[110] which was the subject of Cavendish's first assignment by the Royal Society, in 1766.

The rudimentary state of thermometry at the beginning of the eighteenth century is suggested by Newton's experiments with a linseed-oil thermometer and a scale fixed by two points, the heat of the air standing above water when it begins to freeze, and the heat of blood, from which Newton extrapolated freely to high temperatures.[111] Nearly forty years later, Robert Smith, who translated Newton's directions for making thermometers, observed that none of the thermometers he had seen had been tested for comparability,[112] still largely the state of affairs when Cavendish studied thermometers thirty years after Smith. There was a variety of scales in use and a wide variation in their adjustment.[113]

The precision of a thermometer—the fractions of a degree to which it could be read—had little meaning in practice owing largely to an uncertainty in the upper fixed point. Cavendish (probably with other fellows) tried a number of thermometers built by leading instrument makers, Bird, Ramsden, Nairne, and George Adams, finding that they differed in their readings of the boiling point of water by two or three degrees. Astronomical precision in meteorology was not regarded as important or obtainable, but a disparity of that magnitude in the boiling point of water was unacceptable. Cavendish recognized that to ensure the consistency and compatibility of readings with instruments used by different observers, it

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[106] Henry Cavendish, "No. 1. Measurer of Wind," and "Trial of Windgauge," Cavendish Mss, Misc.

[107] Cavendish to "your Lordship."

[108] Henry Cavendish (1776b).

[109] 9 Dec. 1773, Minutes of Council, Royal Society 6:202.

[110] Richard Kirwan (1787, iii).

[111] William E. Knowles Middleton (1966, 57–58).

[112] Robert Smith, "The Editor's Preface," in Roger Cotes (1747).

[113] Middleton (1966, 65, 75, 115). Britain and Scandinavia used the Fahrentheit scale, while on the Continent, the Réaumur, Delisle , and Swedish scales were used. Kirwan (1787, vi).

was necessary for all of the mercury in the thermometer to be heated equally. He carried out experiments to determine if the upper fixed point of a thermometer scale is affected by the rapidity of boiling of the water and by the immersion of the thermometer either in the boiling water or in the steam above the water. His experiments showed that the rapidity of boiling was not a factor and that immersing the thermometer in steam was more exact and convenient than immersing it in boiling water. In fixing the boiling point, the entire bulb and column were to be exposed only to the steam or else the bulb of the mercury column was to be just barely submerged, since at any appreciable depth it would be compressed, giving a reading that was too high.[114]

The Royal Society called upon Cavendish's skill with meteorological instruments again in 1773, this time to draw up a plan for taking daily meteorological readings and keeping a journal or register of the weather.[115] Weather journals began to appear with some frequency in the *Philosophical Transactions*, coming to outnumber isolated weather reports by the late eighteenth century. They were a means to the end, as the weather-journal advocate William Borlase put it, of making "more perfect Theories of Wind and Weather in our Climate" or else of showing the "uncertainty and vanity of all such attempts."[116] What Charles Hutton wrote in his scientific dictionary at the end of the eighteenth century could have been said at any time during the century:

> There does not seem in all philosophy any thing of more immediate concernment to us, than the state of the weather.... To establish a proper theory of the weather, it would be necessary to have registers carefully kept in divers parts of the globe, for a long series of years; from whence we might be enabled to determine the direction, breadth, and bounds of the winds, and of the weather they bring with them.... We might thus in time learn to foretell many great emergencies; as, extraordinary heats, rains, frosts, draughts, dearths, and even plagues, and other epidemical diseases.[117]

At once a challenge to science and a vital issue to humanity, the weather was the kind of problem the Royal Society regarded as its reason for being, meteorology embodying its early belief in the advancement of science and human welfare through natural histories. The means in the late eighteenth century was weather registers like the Royal Society's.

To keep the register, Cavendish directed the clerk of the Society to read the barometer and indoor and outdoor thermometers the first thing in the morning and again at midday and in the evening, and every morning to measure how much rain had fallen, every afternoon to estimate the wind, and one fortnight a year to consult the Earth magnetic variation and dipping needles four times a day. (Because the magnetism of the Earth draws the needle not only north but also down, there are two kinds of instruments, the variation compass and the dipping needle.) The clerk was also directed to calculate an involved series of means of readings. He was to set down the mean morning and midday heats for each month, the mean

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[114] Henry Cavendish (1921a, 2:351–353); Cavendish (1776b, 115). William E. Knowles Middleton (1964, 132). Middleton dates the increase in accuracy of calibration from about 1770, the time we are considering.

[115] The Council ordered the clerk of the Society to make daily observations of the weather "with the instruments to be procured for that purpose, & proper accommodations under the inspection of the Hon. Henry Cavendish." 22 Nov. 1773, Minutes of Council, Royal Society 6:197.

[116] J. Oliver (1969, 291).

[117] Charles Hutton (1795–1796, 2:677).

heat for each year, and the mean height of the barometer and the mean heat of the thermometer placed near it for each month and each year. Following Cavendish's recommendation, the register was printed at the end of the last part of the *Philosophical Transactions* for each year, beginning with the weather in 1776; the annual readings were set out in nine columns, including one for the date. So that members did not have to wait until the end of the year to learn what the weather had been, the clerk was ordered to post the previous week's record in the public meeting room of the Society.[118]

The Royal Society's "Meteorological Journal," as Cavendish called it, was a conventional journal in the features of the weather it reported: temperatures, pressures, and the like. It did not contain a chemical column for the composition of atmospheric air, and in a few years Cavendish would show that there was no need for such a column, for the composition was unchanging. Nor did it contain electrical columns, though there was some interest in this. Recently the atmosphere had taken on a new complexity and interest as an electrical medium, and prosaic events such as fog and falling weather and spectacular phenomena such as lightning, thunder, auroras, meteors, earthquakes were observed with that in mind. William Henly, inventor of an electrometer Cavendish used, urged readers of the *Philosophical Transactions* to keep an "electrical journal" of the weather, as he did: "Let a large book be provided, and ruled in the manner of a bill-book, used by tradesmen …." The entries in the columns would be the same as in the standard weather journals except for a new measurement, the divergence of the balls of an electrometer, and a new observation, the type of electricity. Henly recommended another new standard measurement, the temperature of the upper air in all kinds of weather, for which he thought Charles Cavendish's self-registering minimum thermometer carried as high as possible by kites would serve.[119]

Even without the complications of electrical and upper-air measurements, the keeping of the Royal Society's weather register was demanding, requiring the clerk to make multiple observations at different times of the day. Less confining would have been fully automatic clock-driven instruments, which were already an old idea. Christopher Wren in the previous century had proposed a "weather clock," and Robert Hooke had developed the idea into a futuristic meteorograph using punches on rolled paper.[120] Cavendish had ideas of this sort, though in connection with a thermometer only: he considered an elaborate mechanical contrivance for recording the temperature every ten minutes on a rotating barrel, making a carefully ruled drawing to scale, probably for his instrument maker.[121] He owned a self-registering meteorological instrument, a dial-type thermometer, not original with him, in which a bulb containing alcohol was connected to a U-tube containing mercury. A heavy pointer registered the temperature at the time, and two lighter pointers moved by the heavy pointer registered the maximum and minimum temperatures (Fig. 8.4).[122]

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[118] "The following scheme drawn up by the Hon. Henry Cavendish for the regulating the manner of making daily meteorological observations by the Clerk of the Royal Society …," 9 Dec. 1773, Minutes of Council, Royal Society, 6:200–204. "Meteorological Journal Kept at the House of the Royal Society, by Order of the President and Council," *PT* 67 (1777): 357–384.

[119] William Henly (1774, 426–427).

[120] Middleton (1969, 254–255).

[121] Henry Cavendish, "Clock for Keeping Register of Thermometer," Cavendish Mss IV, 1.

[122] This instrument was calibrated at Chatsworth in 1779, more or less dating it. Charles Cavendish could have designed it, but at that late date it was more likely Henry Cavendish, if it was not an instrument maker. Through Humphry Davy this instrument eventually passed to the Royal Institution, where it is kept in its collection of historical instruments. Middleton (1966, 138–139). Cavendish, *Sci. Pap.* 2:395–97. Among Cavendish's manuscripts

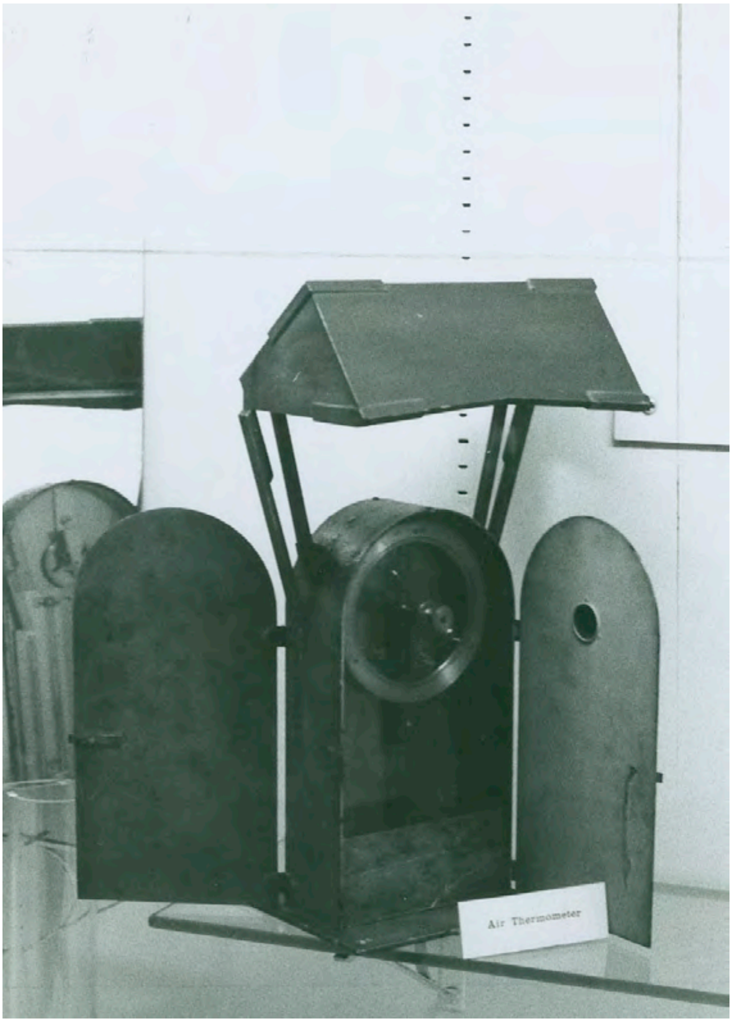

Figure 8.4: Register Thermometer. Photograph by the authors. Cavendish's original instrument is in the Royal Institution, a gift of Humphry Davy's. Alcohol contained in a large tube expands with heat, causing mercury in the U-end of the tube to move. Through a cord attached to an ivory slip on the surface of the mercury, a hand moves across a circular scale graduated in degrees of heat. This hand in turn moves light friction hands, which remain at the maximum and minimum heats for any one setting of the instrument. A description of the instrument together with an engraving of it is in George Wilson (1851, 477–478).

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is "Thermometer for Greatest Heat by Inverting the End of Tube into a Movable Cyl. Of Spt. & Water," Cavendish Mss III(a), 14(c).

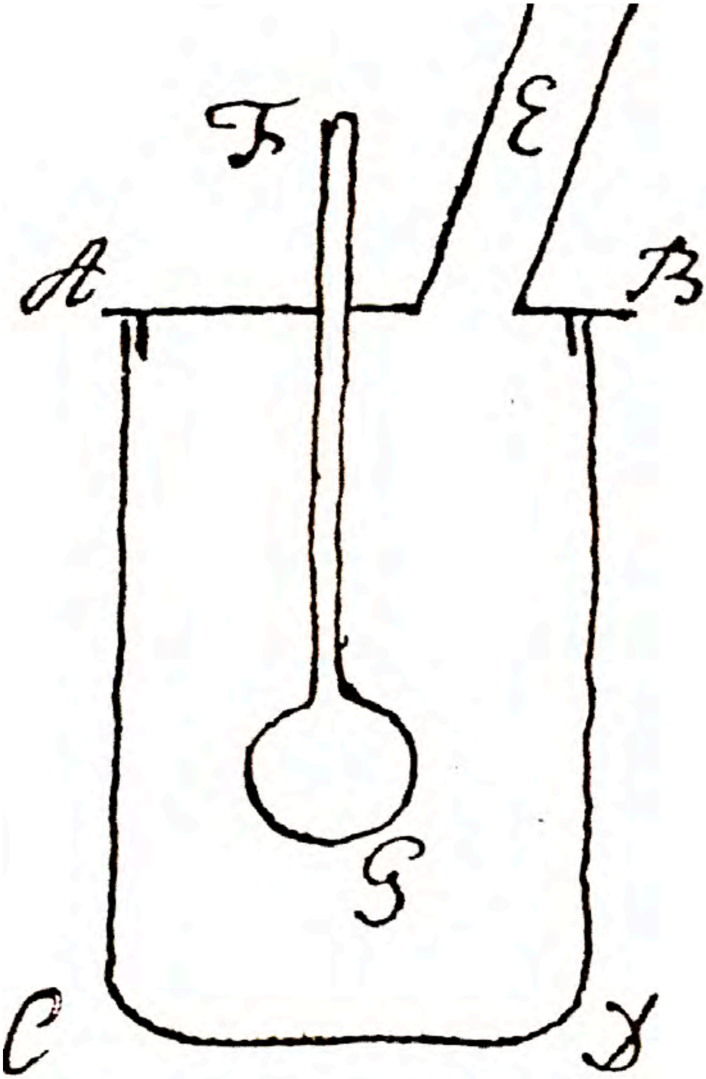

Figure 8.5: Apparatus for Adjusting the Boiling Point. The committee of the Royal Society, which Cavendish chaired, conducted experiments to determine the regularity of the boiling point. ABCD is the pot, AB the cover, E the chimney to carry off steam, FG the thermometer fitted tightly to the cover. The stem of the thermometer as well as the ball are immersed in steam, not water, in accord with Cavendish's recommendation. The committee recommended this apparatus, including an almost identical drawing, in its published paper. "The Report of the Committee Appointed by the Royal Society to Consider of the Best Method of Adjusting the Fixed Points of Thermometers; and of the Precautions Necessary to Be Used in Making Experiments with Those Instruments," *PT* 67 (1777): 816–857, opposite 856. The drawing by Cavendish is in Cavendish Mss III(a), 2. Reproduced by permission of the Trustees of the Chatsworth Settlement.

In 1776 Cavendish together with Aubert, Maskelyne, and Nairne was appointed a committee to "examine into the state of the Society's instruments."[123] Meanwhile a larger committee of seven was formed with Cavendish as chairman to examine the "best method of adjusting the fixed points of thermometers" and the precautions to be taken in "making experiments with those instruments." The other members of the committee were Maskelyne and Aubert, who as astronomers necessarily concerned themselves with temperature and also constantly with instruments; Samuel Horsley, a mathematician, astronomer, and avid observer and analyst of the weather; William Heberden, who kept a meteorological journal; the Swiss meteorologist J.A. Deluc, the most important member other than Cavendish, who had published an influential work calling for the perfection thermometers; and the secretary of the Society Joseph Planta. It was recognized that two fixed points on a thermometer were better than one, with melting ice universally used for the lower fixed point.[124] The recommendation by the committee on the upper fixed point was drawn from Cavendish's earlier report. Because it was known that the boiling point varies with atmospheric pressure, the committee specified a standard pressure to be used when adjusting the fixed point, 29.8 English inches of mercury, giving a formula to be used when the adjustment was made at a different pressure. The committee's paper, which at least in part was written by Cavendish, as we know from his manuscripts, was published in the *Philosophical Transactions* in 1777.[125] (Fig. 8.5). What Cavendish said about the adjustment of the upper fixed point on the scale of a thermometer applies to his overall effort in meteorology: "It is very much to be wished, therefore, that some means were used to establish an uniform method of proceeding; and there are none which seem more proper, or more likely to be effectual, than that the Royal Society should take it into consideration, and recommend that method of proceeding which shall appear to them to be most expedient."[126] Apart from its implicit justification of a national scientific society, Cavendish's wish supported Kirwan's belief that no other science required "such a conspiracy of nations" as meteorology,[127] demanding a uniformity of practice of observers around the world. The method of adjusting the upper fixed point recommended by the committee was made standard on the authority of the Royal Society, and it has been used ever since.[128]

Cavendish published a full account of the meteorological instruments of the Royal Society in the *Philosophical Transactions* in 1776, beginning with the thermometer, the instrument he had examined for the Society ten years before. He again explained the need to immerse the mercury in the stem as well as in the bulb of the thermometer in the steam of boiling water when setting its upper fixed point. He described the proper method for reading the barometer, making corrections for the capillary depression of mercury in the tube based upon his father's observations, though it seems that Cavendish made the calculations for the table he included. To determine if the variation compass was affected by any iron work in the Society's house, Cavendish removed the instrument to the large garden "belonging to a house on Great Marlborough Street," no doubt his father's house, distant from any iron work. He compared the compass readings in the two locations, finding that in the Society's

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[123] 14 Nov. 1776, Minutes of Council, Royal Society 6:303.

[124] Middleton (1966, 116–117, 127). Douglas W. Freshfield and H.F. Montagnier (1920, 176–177).

[125] Signed by Cavendish (listed first), Heberden, Aubert, Deluc, Maskelyne, Horsley, and Planta (1777).

[126] Cavendish (1776b, 115).

[127] Kirwan (1787, iv).

[128] Middleton (1966, 128).

house the needle was drawn aside $15\frac{1}{2}$ minutes toward the northwest by the iron work in the vicinity. He told how to determine the "error of the instrument" by inverting the magnetic needle of the compass. He discussed an "error" of the dipping needle, which he regarded as an "unavoidable imperfection": the ends of the axis of the needle of this instrument rolled on horizontal planes, the error arising from the ends of the axis not being truly cylindrical. In this case, Cavendish was satisfied that the Society's dipping needle was "as least as exact, if not more so, than any which has been yet made." As he had with the variation compass, Cavendish removed the dipping needle to the garden on Great Marlborough Street to determine the true dip, finding a difference of 7 minutes, showing that the dipping needle in the Society's house was not much affected by nearby iron work. "Accuracy" in the recording of the weather, a first consideration in making meteorology more scientific, was improved by raising the funnel collecting rain above the roof of the Society's house where there seemed "no danger of any rain dashing into it," and by sheltering the hygrometer from the rain and locating it "where the Sun scarce ever shines on It," leaving it open to the wind. Acuracy was also improved by taking the mean of observations, by applying corrections such as Deluc's corrections of the barometer by the thermometer, and by modifying instruments; for example, by preventing the vibration of the needle of the variation compass from disturbing the observation of the needle.[129]

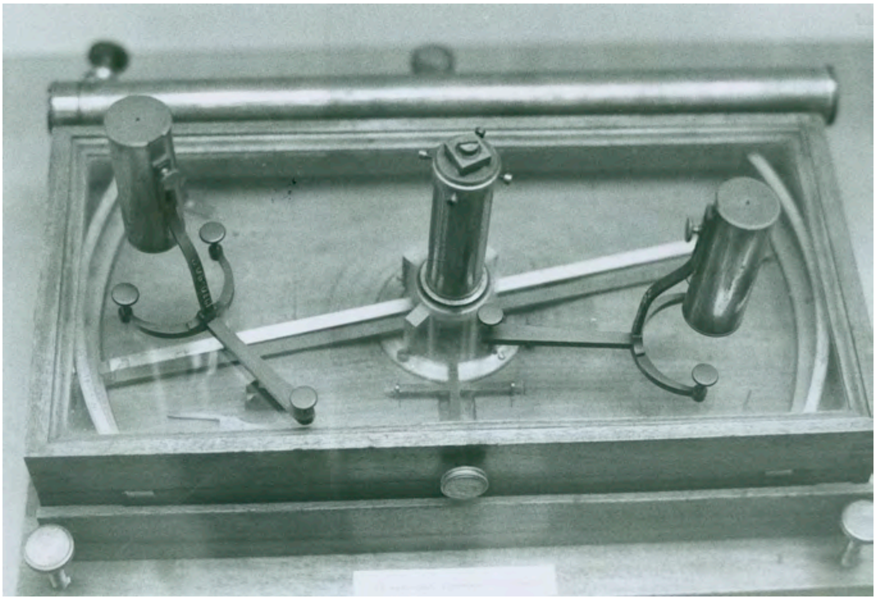

Figure 8.6: Variation Needle. Earth magnetic instrument owned by Henry Cavendish. Photographs by the authors. By permission of the Science Museum, London/Science & Society Picture Library.

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[129] Cavendish (1776b, 117, 124–125).

Figure 8.7: Dipping Needle. Earth magnetic instrument owned by Henry Cavendish. By permission of the Science Museum, London/Science & Society Picture Library.

We return to Cavendish's garden and magnetic instruments. Like the weather, the Earth's magnetism varies complexly from place to place and from time to time, periodically and secularly. Cavendish observed the Earth's magnetic variation and dip at regular intervals and calculated their mean yearly values. Before his study of the Royal Society's meteorological instruments, in the early 1770s he and his father alternated in taking readings with a variation compass in the "garden." (Fig. 8.6). Mixed in with Cavendish's readings are others taken by Heberden at Heberden's house and also, it would seem, in Cavendish's garden.[130] Upon moving from his father's house, Cavendish kept a record of variation of the magnetic compass at his next house at Hampstead from 1782 and later at his house on Clapham Common until 1809, the year before he died. This record consists of more or less daily readings through the summer months,[131] beginning before eight in the morning and ending about 11 at night. He did not place much weight on his readings; when he was asked about the mean variation of his observatory at Clapham Common, he provided it for the past summer but not for past years, because, he said, many other persons there had observed the variation longer than he had.[132] His interest centered on the instruments, experimenting with different suspensions, shapes, and sizes of magnetic needles, trying his father's, Sisson's, and Nairne's needles and his own variant. (Fig. 8.7). He drew up directions for using a dipping needle on several voyages.[133]

We have chosen meteorology as a source of examples to show Cavendish's way with instruments. Whoever examines his meteorological manuscripts must be struck by the tenacity with which he compared his instruments among themselves and with those belonging to the Royal Society and others belonging to colleagues. Take hygrometers, the instruments for measuring the moisture of air, a variety of which were invented from the 1780s with their respective champions. One of the inventors Deluc criticized Saussure's hair hygrometer, and Saussure responded, the two disputing with with such spirit that Blagden spoke of "open war."[134] Deluc had the better temper, but Saussure had the better hygrometer, his being the only one used for serious meteorology by 1820.[135] Their claims aside, all inventors agreed with what Deluc called the "essential point" about hygrometers, that they should be contrived so that all "observers might understand each other, when mentioning degrees of humidity."[136] John Smeaton, another inventor, agreed that the goal was to make hygrometers that, like the best thermometers, were "capable of speaking the same language."[137] To that end Cavendish made trials with Smeaton's hygrometer, which was used by the Royal Society, and with other hygrometers labeled variously "Nairne's," "Harrison's," "Coventry's," "common," "old," "new," "4-stringed," and "ivory." The type of instrument he studied was the hygroscopic hygrometer, which either weighed the water by the increase in weight of dry salt after moist air was passed over it or measured the change in dimensions of a moistened substance such as the contraction of strings; Cavendish generally preferred weighing to measuring as the more exact method, but in this instance he preferred measuring in contrast to our preference today, weighing. He roasted, salted, wetted, and stretched moisture-absorbing strings, and he mixed vapors from acids and alkalis with the air to see

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[130] Cavendish, "Horizontal Needle." On page 3, alongside Cavendish's readings taken in his garden, there are readings by Heberden, who must have been there too. Cavendish's manuscripts also contain readings of the variation compass taken at Heberden's house. Cavendish Mss IX, 19, 21, 23.

[131] Henry Cavendish, "Observations of Magnetic Declination," Cavendish Mss IX, 1. The earliest observations in this manuscript of 256 numbered pages were made at Hampstead; those from page 30 on were made on Clapham Common.

[132] Henry Cavendish to J. Churchman, n.d. [after 12 July 1793], draft; in Jungnickel and McCormmach (1999, 694).

[133] Cavendish's manuscripts contain his instructions to an instrument maker. "Dipping Needle"; "Trials of Dipping Needle"; "On the Different Construction of Dipping Needles," Cavendish Mss IX, 7, 11, and 40. He drew up directions for the use of the dipping needle for three voyages, by Richard Pickergill, James Cook and William Bayley, and Alexander Dalrymple. Ibid., 41–43.

[134] Middleton (1964, 100). On Saussure and Deluc's disagreements: Charles Blagden to Henry Cavendish, 23 Sep. 1787; in Jungnickel and McCormmach (1999, 641).

[135] Middleton (1969, 103, 106).

[136] Deluc (1773, 405).

[137] John Smeaton (1771, 199).

if they made a difference. At times he took readings daily, morning and evening, as often as every twenty minutes, in warm rooms and cold rooms, often together with thermometer readings.[138] For ten years he compared hygrometers. If this activity seems obsessive, it was an essential scientific activity, for the reliability of the instrument and the method of its use were an inseparable part of the scientific argument. It could be said, and Cavendish would have agreed, that an unexamined instrument was not worth using.

In Cavendish's day it was common for researchers to build some of their apparatus but they usually bought or commissioned their instruments. Researchers occasionally invented instruments and instrument makers like Nairne made scentific experiments, but instrument making was a business, and science for someone like Cavendish was a full-time activity. Nearly all of Cavendish's instruments were made in London by contemporary, highly skilled artisans. An exacting experimenter, Cavendish lived in the right place at the right time.

Cavendish's examination of Nairne and Blunt's wind measurer for accuracy was an implicit form of tribute. His colleague George Shuckburgh made it explicit, remarking on the "singular success with which this age and nation has introduced a mathematical precision, hitherto unheard of, into the construction of philosophical instruments."[139] In his living quarters at Greenwich Observatory, the astronomer royal Maskelyne exhibited in addition to a bust of Newton, maker of reflecting telescopes as well as explicator of the system of the world, prints of the builder of the great eight-foot mural quadrant for Greenwich, John Bird, and of the inventor of the achromatic telescope used at Greenwich, John Dolland.[140] In the advancement of science in Cavendish's time, instrument makers were as important as their users.

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[138] Henry Cavendish, "Hygrometers," Cavendish Mss IV, 5. This manuscript consists of 77 numbered pages of laboratory notes and an index.

[139] George Shuckburgh (1779, 362).

[140] 29 July 1785, "Visitations of Greenwich Observatory, 1763 to 1815," Royal Society, Ms. 600, XIV.d, f. 36.

Chapter 9
Electricity

Mathematics and Theory

Today physical scientists look at mathematics as a "tool for reasoning" about the physical world, judging it an "extremely useful tool."[1] It was the same in Cavendish's time. In his book on Newton's discoveries, Maclaurin said that mathematics was the "instrument" that enabled Newton to do his great work. From experiments and observations alone, Newton could not have inferred causes from effects and explained effects by causes; for that, he needed "sublime geometry." Maclaurin did not know if Newton showed more skill in "improving and perfecting the instrument, or in applying it to use."[2] Mathematics, the mathematics teacher and instrument maker Benjamin Martin wrote, is the "science or doctrine of quantity."[3] In the practice of science, mathematics was the intellectual tool that complemented the material tools, the instruments of weighing and measuring. Just as patient experiments could lead to discoveries, so could mathematics with its long chain of reasoning. In the eighteenth century, there was a general expectation that the physical sciences would acquire a mathematical form, if they had not already done so. The history of the physical sciences seemed to have demonstrated that when they became mathematical, progress was made in them. This, we assume, was in Cavendish's thoughts when he began his researches, which would impress his contemporaries for their mathematical and quantitative exactitude. In papers he wrote out carefully, he sometimes included drawings, made with the aid of drawing instruments, a complementary form of mathematical exactitude (Figs. 9.1–9.2).

Not all British natural philosophers were knowledgeable in mathematics, but those who like Cavendish studied at Cambridge probably were. For learning materials, they had Newton's *Principia* on geometrical methods and his lectures on the method of fluxions. They also had more recent texts, the best of which was Mclaurin's *Treatise on Fluxions* in 1742, the first systematic presentation of Newton's version of the calculus, written to quell doubts about it.[4] Maclaurin's and other mathematical texts applied fluxions to physical problems, and they occasionally discussed the agreement between mathematical results and measured phenomena, directly addressing the interests and needs of natural philosophers. Original work in mathematics was published in books and journals including the *Philosophical Transactions*. In Cavendish's time, about a fifth of the papers in the journal were on pure mathematics or on mathematics applied to astronomy, mechanics, optics, pneumatics, and other parts of natural philosophy. Papers presenting mathematical theories of nature were rare.[5]

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[1] Richard Feynman (1994, 34). Murray Gell-Mann (1994, 108).

[2] Colin Maclaurin (1748, 8).

[3] Benjamin Martin (1759–1764, 1:1).

[4] Colin Maclaurin (1742). J.F. Scott, "Maclaurin, Colin," *DSB* 8:609–612, on 610–611. I. Grattan-Guinness (1986, 167–168).

[5] Richard Sorrenson (1996, 37).

The English preferred Newton's fluxions to Leibniz's analytical form of the calculus, used on the Continent. The Scottish natural philosopher John Playfair said that Maskelyne was a good mathematician but not well-versed in the writings of Continental mathematicians. "Indeed, this seems to be somewhat the case with all the English mathematicians; they despise their brethren on the Continent, and think that every thing great in science must be for ever confined to the country that produced Sir Isaac Newton."[6] Playfair thought that Maskelyne was less prejudiced than some of his countrymen. Like Maskelyne, in the calculus Cavendish used only Newtonian fluxions.

An English mathematical natural philosopher understood the concept of "function," a variable quantity dependent on one or more other variable quantities. He knew the elementary parts of mathematics: geometry, algebra, trigonometry, and logarithms. He was well acquainted with fluxions and their inverse, "fluents," the mathematics for describing motion. He knew about infinite series, a companion to the calculus. He knew ordinary and partial differential equations and the calculus of variations, branches of mathematics arising from the application of the calculus to physical problems such as pendulum motion, elasticity, fluid flow, and propagation of sound. If he had an interest in mathematics for its own sake, he knew other branches such as probability, differential geometry, and number theory. Cavendish was familiar with most if not all of these branches. Unlike their seventeenth-century predecessors, Cavendish and his scientific contemporaries did not need to invent new mathematics to advance science. They needed only to be inventive with (and trust) the mathematics of their day. Mathematics and mechanics, particularly the theory of motion, were developed together and by the same people, so that it is meaningful to speak of a "virtual fusion" of the two.[7] In his text on fluxions, William Emerson characterized them as a method of calculation that "discovers to us the secrets and recesses of nature." The image of motion, a velocity, entered the common understanding of the mathematical concept of fluxions.[8]

Given the nature of eighteenth-century mathematics, and given Cavendish's way of working, a hard and fast line cannot be drawn between his mathematical and his scientific interests, though certain of his papers are concerned with mathematical problems having no obvious connection with experiments and observations. One deals with prime numbers,[9] and several deal with topics in De Moivre's subject: the probability of winning more than losing in a game, the probability of throwing a certain number with a certain number of dice, the possible ways of paying a sum with coins of different denominations, and annuities on lives.[10]

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[6] Playfair (1822, 1:lxxvii, Appendix, No. 1, "Journal").

[7] Morris Kline (1972, 394–396).

[8] The method of fluxions is founded on the principle that "any quantity may be supposed to be generated by continual increase, after the same manner that space is described by local motion." William Emerson (1768, iii).

[9] Henry Cavendish, "On Prime Numbers," Cavendish Mss VI(a), 8.

[10] Cavendish Mss VI(a), 1, 23, 46, 48.

Mathematical Instruments

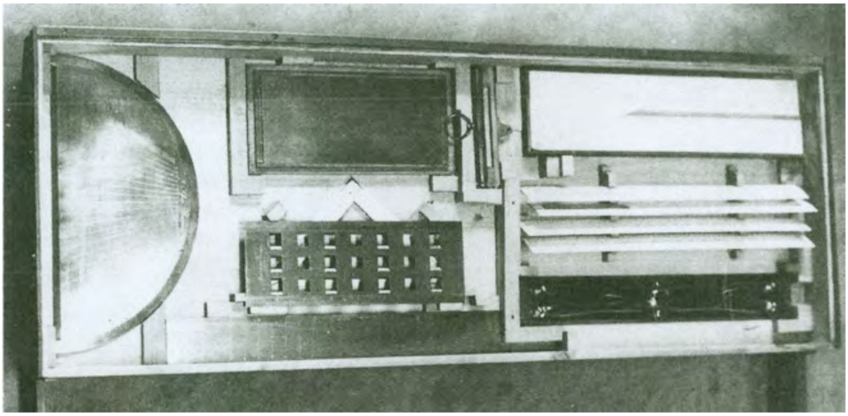

Figure 9.1: Mathematical Instruments. The instrument cases in this and the illustration below are drawers that fit into a cabinet belonging to Henry Cavendish. There are many scales and rulers, a brass globe map projection, an ivory triangle, and more, bearing the names of well-known instrument makers: Jesse Ramsden, Jonathan Sisson, John Morgan, and Fraser, presumably William Fraser. Photograph by the authors.

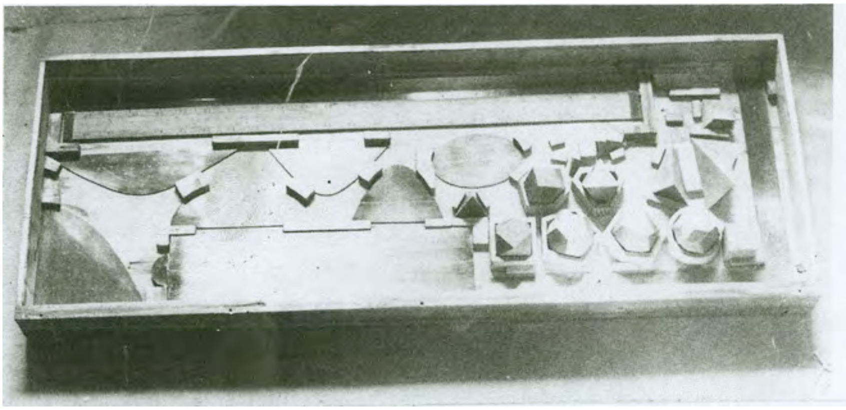

Figure 9.2: Mathematical Instruments. This drawer contains more brass and wood scales and rulers. The regular solids are made of boxwood. Cavendish's scientific papers contain drawings made with these instruments, including drawings from which plates were made for his publications. Photograph by the authors. Chatsworth. Reproduced by permission of the Chatsworth Settlement Trustees.

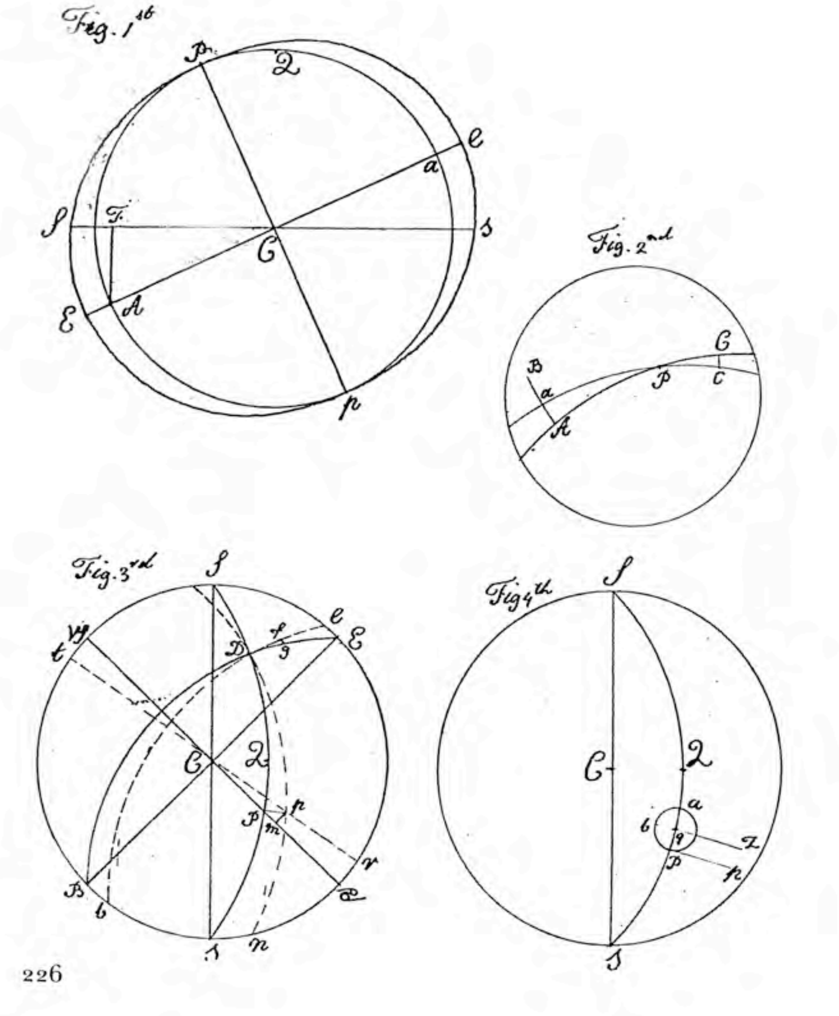

Figure 9.3: Mathematical Drawings. The figures are part of a carefully drafted but unpublished manuscript, “Precession of Equinoxes,” Cavendish Mss VIII, 9. Reproduced by permission of the Chatsworth Settlement Trustees.

Other papers are about parts of mathematics having applications in the physical sciences such as the binomial theorem, the multinomial theorem, infinite series, and the construction and solution of algebraic equations.[11] There are also papers on subjects with a direct bearing on Cavendish's work in the laboratory and the observatory, such as Newton's rule of interpolating, the accuracy of taking the mean of observations, triangular forms that reduce the effects of errors of measurement, and errors of instruments.[12] Most of the mathematical papers deal with problems in plain or spherical geometry, some of which had scientific applications, for example, a curve drawn with reference to three points.[13] Many of Cavendish's mathematical papers were written late in life, when he was doing less experimental work.[14] He published none of his work on mathematics. Doubtless solving mathematical problems gave him satisfaction, and because they were close to his work in natural philosophy, his mathematical exercises might be likened to his regular handling and comparing of instruments.

Cavendish has entered the history of science primarily for his experimental work. That is understandable, but it overlooks the important fact that he was no less skilled as a theorist. Maxwell appreciated this side of Cavendish, as is evident in his edition of Cavendish's electrical researches.[15] So did the theoretical physicist Joseph Larmor, editor of Cavendish's mathematical and dynamical manuscripts, who wrote that if Cavendish "had no other claim to renown he would be entitled to rank high among the theoretical physicists of his period."[16] The historian of science James Crowther made an insightful observation: Cavendish's "experiments were always guided by a theoretical idea, and intended to collect data bearing on it."[17]

Without theories, generalizations, rules, and laws, natural philosophy was incomplete. Knowledge of the physical world was improved by increasing the body of physical facts and equally by establishing their connectedness. To perform a thoughtful experiment was to inquire into the "truth of a conceived proposition," James Hutton said; for science to be "actually advanced," there had to be a "certain theory" in the mind of the experimenter.[18] Samuel Horsley said that the "true uses" of "theory" in science are "either to explain the mutual connections and the dependencies of things already known, or to suggest conjectures concerning what is unknown, to be tried by future experiment"; the investigator who understands the uses of theory "will always find it a useful engine." Cavendish and his colleagues would not have disputed the characterization of theory as a "useful engine." They understood that the right combination of experience and reason led to theories of nature that were a good approximation to the truth, and that true theories in turn brought new understanding to known facts and led to new facts. Like instruments and mathematics, theories were tools in the investigator's work kit. They ordered, explained, and predicted phenomena, and the

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[11] Ibid., 15, 16, 21, 22, 24, 27.

[12] Ibid., 6, 34, 45. The paper on the probable error of instruments does not have an identifying number. The problem is to determine the probability of the sum of the errors of two instruments given the error of any one instrument.

[13] Ibid., 17, 36.

[14] The mathematical manuscripts are not dated, but the watermarks on the paper give occasional indications. In the manuscripts on Braikenridge's surfaces and on the loci of third-order equations, some of the sheets bear watermarks from 1797 to 1804.

[15] In his edition, Cavendish, *Electrical Researches*.

[16] Larmor, in Cavendish, *Sci. Pap.* 2:399.

[17] James Gerald Crowther (1962, 302, 316).

[18] James Hutton (1794, 3).

complete natural philosopher worked with this understanding. Cavendish developed two general theories: the one he did not publish was on heat, the one he did was on electricity, which we discuss below. Both theories were mathematical. Just as Newton's mathematical principles of natural philosophy "gave an entirely new face to theoretical astronomy,"[19] according to the Cambridge professor of astronomy Roger Long, we can say that by recourse to these principles Cavendish did much the same for theoretical electricity and heat.

Electrical Theory

We get a sense of how far the subject of electricity had come by looking at it when Newton addressed it.[20] He described to the Royal Society how glass rubbed on one side attracts and repels bits of paper to and from its opposite side with an irregular and persisting motion.[21] On the face of it, these little agitations do not seem very impressive, but Newton intuited that a "certain most subtle spirit which pervades and lies hid in all gross bodies" might account for the forces of electric bodies and beyond that for light, cohesion, animal sensation and willed commands. To learn the laws of "this electric and elastic spirit," he said, more experiments were needed.[22] As more experiments were performed, and as techniques were developed for detecting, generating, and accumulating electrical charges, Newton's expectation of the importance of electricity in the scheme of things seemed borne out (and, to some of his followers, of his speculation about the electrical ether as well). Fifty years after Newton, the insightful investigator William Watson observed that electricity is an "extraordinary power" that "cannot but be of very great moment in the system of the universe."[23] On the eve of Cavendish's researches in electricity, Joseph Priestley said that electricity is "no local, or occasional agent in the theatre of the world," but plays a "principal part in the grandest and most interesting scenes of nature."[24] Watson and Priestley essentially repeated what Newton had said, only now with a good deal more evidence. Scientific expectations ran high; by the 1760s electrical researchers had come to associate electricity with a force that acts over sensible distances according to a determinable law, the starting point of a quantified science of electricity. The timing was right for Cavendish, whose skills with instruments and mathematics were well-suited to treat a second force of nature after the model of the first, gravitation. He planned a book about it after his model, Newton's *Principia*.

The idea of an electric fluid (sometimes two contrary electric fluids) owed something to the older idea of effluvia but more to the idea of an ether. Herman Boerhaave's doctrine of elementary fire was an influential intermediary between the ether and the "imponderable fluids" of eighteenth-century natural philosophy, one of which was electric.[25] Other fluids were postulated for magnetism, light, and heat, all bearing the distinctive characteristic of Boerhaave's fire: bodies "*sui generis*, not creatable, not producible *de novo*."[26] The ether, for its unity and simplicity, held a strong appeal to natural philosophers, but in the middle of

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[19] Roger Long (1742, 1764, 1784, 2:117).
[20] The following discussion draws on Russell McCormmach (1967).
[21] Reported in Joseph Priestley (1767, 13–14).
[22] Newton (1962, 2:547).
[23] William Watson (1752c, 375–376).
[24] Priestley (1767, xii).
[25] I.B. Cohen (1956, 214–234).
[26] Herman Boerhaave (1727, 1:233).

the century British progress in the exact understanding of electricity and other experimental fields owed more to imponderable fluids of fixed quantity.

William Watson was the electrical experimenter Charles Cavendish worked with and next to Benjamin Franklin the leading British electrician at the time. He continued to be regarded as one of the Royal Society's leading electricians into the period of Henry Cavendish's researches twenty years later. Watson's theory of electricity was based on an elastic, electric fluid permeating all bodies, which gives no sign of its presence when the "degree of density" is everywhere the same, but where there is a local inequality it moves to adjust its density to the same "standard," giving rise to electrical effects.[27] Watson's theory invited a mathematical treatment of electricity.

In his *History of Electricity* in 1767 Joseph Priestley wrote that English electricians and most foreign ones too had adopted Franklin's elastic fluid theory of positive and negative electricity. Priestley's opinion was that the basic features of Franklin's theory were as "expressive of the true principles of electricity, as the Newtonian philosophy is of the true system of nature in general."[28] Franklin defined a body to be "positively" electrified if it has more than its "normal" quantity of electric fluid, "negatively" electrified if it has less. The usefulness of his terms is evident in his analysis of the Leiden jar, one side of which is electrified positively in exact proportion as the other side is electrified negatively, the same amount of fluid entering one side as flows out the other. Franklin's analysis turns on the quantity of electric fluid in place of Watson's density, and although quantity alone is insufficient to explain all electrical phenomena, it explains most instances of attraction and repulsion of electrified bodies. Like Watson's theory, Franklin's theory pointed to a mathematical treatment of electricity.

"Thoughts Concerning Electricity," Cavendish's first electrical theory,[29] cannot be earlier than 1767, since it cites Priestley's *History of Electricity*. The paper is carefully written, but its organization is clumsy, conveying a sense of groping, certainly not a final draft. The theory is concerned with differences in densities of an expansive fluid, suggestive of Watson's theory. It makes use of Franklin's terms "positive" and "negative," but they are given a different meaning, associated not with quantity of electricity but with its "compression," what we call "pressure." An active concept borrowed from pneumatics, compression is suggestive of Watson's theory, in which the action of an electrical machine is likened to a "pump" for the electric fluid. In Cavendish's theory, a body is said to be "positively" or "negatively" electrified according to whether the fluid in it is more or less compressed than it is in its natural state. Because a key property of compression is its constancy throughout a connected system, in Cavendish's theory it is equivalent to the modern concept of electrical potential; this is the central idea of the theory.[30] "Degree of electrification," another expression Cavendish uses for compression, is one the two variables of the theory, the other being quantity of electricity, or charge. A body is said to be "overcharged" or "under charged" if it contains more or less fluid than it does in its natural state. Two overcharged bodies

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[27] William Watson (1748c, 95).

[28] Priestley (1767, 160, 455)

[29] Cavendish (1879j). The mathematical development of this theory is a separate paper: "Cavendish's First Mathematical Theory," *Electrical Researches*, 411–417.

[30] J.C. Maxwell, "Introduction," to Henry Cavendish (1879i, xxvii-lxvi, on xlix-l). Maxwell notes, ibid., 382–383. Maxwell thought that Cavendish was the first to use the idea of electric potential. In modern terms, electric potential is the work performed on a unit of electric charge in removing it from its actual place to infinity, free from electric influences.

repel one another, as do two undercharged bodies, and an overcharged and an undercharged body attract. Cavendish will refine his theory, but already he has the theoretical basis for an extraordinary course of electrical experiments.

To explain the attraction and repulsion of electrified bodies by the theory, Cavendish introduces local concentrations or deficiencies of electric fluid in a space initially filled with electric fluid of uniform density. If two localized regions have more than their normal quantity of fluid, one body will "appear" to be repelled by the other, just as a body of greater density than water "tends to descend in it." In the theory, the only true (as opposed to apparent) electric force is the mutual repulsion of the particles of the electric fluid accounting for its expansive tendency. Assuming that the force varies with some inverse power of the distance, Cavendish investigates mathematically the consequences for the theory of a range of possible inverse powers including the inverse square. For comparison, he includes a study of the same kind for another elastic fluid, common air, finding that the electric fluid and air cannot have the same law of force.[31]

"Thoughts Concerning Electricity" ends with a troubling thought. Cavendish questions how far the idea of an electric fluid "diffused uniformly through all bodies not appearing electrical," with the repulsion of its particles extending to considerable distances, "will agree with experiment." He writes, "I am in doubt." The paper breaks off in midsentence; evidently, the last page is lost, but it does not matter, for Cavendish has changed theories.[32] His new theory is again based on an expansive electric fluid, but it has a greater complexity of forces. He published this theory in the *Philosophical Transactions* in 1771.

The paper has two parts, the first theoretical, the second a comparison of the theory with experiments done by others. Given Cavendish's experimental skill, it might seem odd that he used only experiments by others to support his theory. There are two likely reasons for this. First, the experiments he cited were by Franklin, Canton, and other leading experimenters on attraction, induction, and the Leiden jar, phenomena that largely defined the experimental field. The other reason is that at the time his paper was read to the Royal Society, at the end of 1771, he had just begun his own experiments on a new class of phenomena predicted by his theory. He said that he intended to follow his paper with another containing his experiments. He also said that his experiments pointed to the inverse square law of distance as the law of electric force, but he had not yet made the conclusive experiments. The paper of 1771 was meant to be the beginning.

Before taking up Cavendish's paper, we need to look at his way of making a theory. Each of his two electrical theories rests on a hypothesis; in the first theory the hypothesis is divided into five parts, in the second theory it is singular. For a long time, hypotheses were considered the unacceptable face of natural philosophy, associated with unfounded speculation. Newton had disparaged them because they could not be deduced from phenomena, and his rejection of Descartes' vortices all but permanently tarred hypotheses for his early followers. British authors were naturally wary of them.

In due course, there came to be an acceptance of a larger activity of the mind in scientific work, and even Newton's warmest supporters acknowledged that their master had made use of hypotheses now and then. It was recognized that hypotheses could be combined with experiments, which remained the arbiter of the truth of nature. When applied with proper restraint, hypotheses could be helpful in directing research, and the question came to be

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[31] Cavendish, "Cavendish's First Mathematical Theory," *Electrical Researches*, 411–412.

[32] Cavendish (1879j, 103).

not the admissibility of hypotheses but their quality and appropriateness. The astronomer William Herschel called the proper motion of the Sun his “hypothesis,” but it was not a “mere hypothesis,” for it was based on established fact.[33] Cavendish understood that a theory begins with a hypothesis, a willingness to assume a statement about nature without assuming its truth, which depends on there being a match between the theory and experiment.

The hypothesis that stands at the head of Cavendish’s second theory of electricity reads: “There is a substance, which I call the electric fluid, the particles of which repel each other and attract the particles of all other matter with a force inversely as some less power of the distance than the cube: the particles of all other matter also, repel each other, and attract those of the electric fluid, with a force varying according to the same power of the distance.”[34] The hypothesis differs from Franklin’s in that there is no mention of electric atmospheres surrounding charged bodies, and it states the electric force as a mathematical law. Newton considered a range of distance dependencies of the gravitational force and showed that only the inverse square of the distance agreed with observations. Cavendish proceeded the same way.

In his experiments on air, Cavendish weighed the air and determined its density, a defining property. He could not do the same with the elastic fluid of electricity. He writes that “in all probability the weight of the electric fluid in any body bears but a very small proportion to the weight of the matter.”[35] By “weight,” he means what we do by “mass,” or quantity of matter; in his day, when talking about ordinary matter, “weight” was used for both mass and weight, which is a gravitational force, and there was no misunderstanding since weight is proportional to mass, and all ordinary matter responds to gravity. According to Cavendish’s hypothesis, ordinary matter has an electrical force, and we know that it also has a gravitational force because we can weigh it on scales. If Cavendish thought similarly about the contrary matter, the electric fluid, he said nothing about it; any gravitation of the fluid would have been insignificant. His reason for bringing up the mass of the fluid was solely to make clear that his hypothesis was about a real substance, not an abstraction; he did not make use of mass in developing the theory, needing only the distance dependency of the electric force. The question of whether or not the electric fluid responds to the gravitational force is interesting only for what it might say about Cavendish’s opinion of “imponderable fluids” or, much the same, about his opinion on the universality of gravitation, which Newton assumed. Bearing on the question is Cavendish’s agreement with his colleague John Michell that another extremely subtle substance, light, responds to the gravitational force; that light has weight, Michell said, “there can be no reasonable doubt, gravitation being, as far as we know, or have any reason to believe, an universal law of nature.” For the same reason, Michell thought that electricity too gravitates, though perhaps having a different measure of gravitational mass than ordinary bodies: he wrote to Cavendish that it is possible that “light (and perhaps too the electric fluid, which seems to be in some degree allied to it.) may not be so much affected by gravity, in proportion to their vis inertia, as other bodies.”[36]

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[33] William Herschel (1783, 248, 268, 275).

[34] Henry Cavendish (1771); in *Electrical Researches*, 3–63, on 3. Cavendish’s paper was read at two meetings of the Royal Society, on 19 Dec. 1771 and 9 Jan. 1772.

[35] Cavendish (1771); in *Electrical Researches*, 4.

[36] Maxwell said that Cavendish meant only mass, since the force by which the fluid is attracted to the Earth depends on the electrical condition of the Earth, whether it is over- or under-charged. Maxwell, in Cavendish, *Electrical Researches*, 362–63. Michell (1784, 37). John Michell to Henry Cavendish, 20 Apr. 1784; in Jungnickel and McCormmach (1999, 587).

Within the formal categories of definitions, propositions, lemmas, corollaries, problems, cases, and remarks, Cavendish develops his electrical theory through Euclidean-like demonstrations, a deductive model which had been extended from the geometry of the ancients to modern science and mathematics. Newton adopted the form for his *Principia*. Like its form, the physical content of Cavendish's theory follows the *Principia*, in which the law of gravitation is derived and its predictions are compared with the motions of the solar system. Cavendish's theory rests on the law of electric force, and its predictions are compared with the principal phenomena of electricity.[37] The mathematics of Cavendish's theory is the same as Newton's, the calculus, only Cavendish uses Newton's fluxions, whereas in the *Principia* Newton uses a geometrical form of the calculus. Cavendish analyzes the action of the electrical fluid in bodies connected by "canals," or wire-like threads of matter through which the electric fluid can move freely.[38] Assuming that particles attract and repel with a force inversely as the nth power of the distance, n being less than 3, and in some cases assuming that n is 2 as it is in the case of the force of gravity, he demonstrates as rigorously as possible the electrical behavior of mathematically treatable bodies. Recalling his education at Cambridge with its emphasis on Newton's mathematics and mechanics, Cavendish's electrical theory can be seen as the single most impressive extension of this education in natural philosophy in the second half of the eighteenth century.

Because of the mathematics, Cavendish's work in electricity stood apart from that of his British contemporaries, to the puzzlement of the Scottish natural philosopher John Robison. Since the attractive and repulsive forces of electricity produce "local motion in the same manner as magnetism or gravitation produce it," for which mathematical laws were known, Robison thought that the "countrymen of Newton, prompted by his success and his fame, would take to this mode of examination" in electricity, but this did not happen, with two exceptions, Cavendish and Stanhope, which made the point.[39]

We look closer at Cavendish's mathematical theory. The first consequence of his hypothesis is a demonstration. He imagines a truncated cone filled uniformly with matter whose particles mutually repel with a force inversely as the nth power of the distance. He derives the force of repulsion on a particle at the apex of the cone if it were continued. He considers three cases, n is greater than 3, 3, and less than 3, showing that in the first two cases, the particle is not affected by the repulsion of any matter except what is very near it, and in the third case, the particle is sensibly affected by all the matter regardless of how near or far. The latter is the realistic case, agreeable to his hypothesis. A further demonstration connects directly with his experiment to determine the exact value of n. He imagines a spherical shell filled with uniform matter whose particles mutually repel with a force inversely as the square of the distance, $n = 2$. He shows that a particle placed anywhere within the hollow sphere is repelled with equal force in one direction as in the opposite direction, so that it is not impelled in any direction, a result he takes from Newton's *Principia*. It follows from the same demonstration that if the repulsion is inversely as a higher power than 2, the particle is impelled toward the center of the sphere, and if the repulsion is inversely as a

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[37] At the time, the Plumian Professor in Cambridge was giving a course on experimental philosophy in which he ordered his lectures on electricity under the heading "Mechanics." Anthony Shepherd (1770, 3).

[38] The indispensable "canals" communicating electric fluid were derivative of the canals of fluid mechanics. Cavendish used the latter "canals" in his theory of the propagation of sound in air: "On the Motion of Sounds," Cavendish Mss VI(b), 35.

[39] John Robison (1822, 4:1–2); "Electricity," in Supplement to *Encyclopaedia Britannica*, 3d ed., vol. 1 (Edinburgh, 1803), 558. In 1779 Charles Stanhope, Lord Mahon, published *Principles of Electricity*.

power lower than 2 it is impelled away from the center.[40] He gives similar demonstrations for bodies of other shapes and for bodies connected by wire-like canals.

In the second part of his paper, where he compares "theory with experiment", he begins with the attraction and repulsion of electrified bodies, which "seem to agree exactly with the theory," as he proceeds to show. He considers the cases where the two bodies are electrified positively and negatively in the same or different degrees and are insulated or not insulated, and he considers the effects of electrical induction on the distribution of the fluid in the bodies. There are thirteen cases, comprising all the principal phenomena of attraction and repulsion he could "think of": the repulsion of two cork balls suspended by conducting threads, a common electrometer; the effect of points in causing a discharge of electricity, which relates to the demonstration above of the repulsion of a cone,[41] a subject relevant to the design of lightning conductors; and the action of the Leiden jar, or "phial," which Cavendish treats at length. In his comparisons of theory and phenomena, his reasoning is exact, though it does not do full justice to his theory, since none of the phenomena is quantitative, whereas his theory is capable of quantitative explanation. The experiments to confirm the predictions of the theory Cavendish will invent and carry out himself.

Cavendish moved easily between his fields of research, electricity and chemistry, which at the level of analysis showed certain similarities. The obvious connection is elastic fluids. His first publication was on air fixed in bodies and capable of being released. His second publication was on on air fixed in the earth suspended in mineral water and capable of being released. His third publication, the one we consider here, was about an elastic electric fluid fixed in bodies. The next two publications were on meteorological instruments, which measured the physical properties of common air. As we just saw, Cavendish likened the degree of electrification of a body to "compression," meaning "pressure," a measurable property of the electric fluid and of air alike. He introduced the idea of electrical "saturation," which applies where the attraction and the repulsion on any small bits of matter in a body are equal, and the body is in its normal uncharged state. He used the idea of "saturation" in his paper on factitious air as part of a method of measuring the quantity of fixed air in an alkali, the affinities being neutralized.[42] He spoke of the electric fluid and common matter as mutually attractive "contrary" matters, in which respect they resemble factitious airs and the bodies containing them. In his published paper on electrical theory, he compared the hypothetical electric fluid with the real elastic fluid of air. "Sir Isaac Newton supposes that air consists of particles which repel each other with a force inversely as the distance," a reference to the *Principia*, where Newton shows that Boyle's law relating the volume and pressure of an enclosed air implies that the only admissible force between particles of the air is one that varies inversely as the distance. Cavendish pointed out that if the repulsion of air particles extends to all distances, as the electric force does in his theory, air would not obey Boyle's law. The latter requires a force varying inversely as the distance, but one which extends only a very short distance to the closest particles, and because that distance is not fixed, Cavendish thought that this law of force was "not very likely."[43] Electricity and air are both elastic fluids, but the law of force is certain to be different in the two cases. Whatever similarities

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[40] Cavendish (1771); in *Electrical Researches*, 5, 8.

[41] Ibid., 47–55.

[42] His standard was 1000 grains of marble. By experiment, he determined the number of grains of pearl ashes needed to saturate as much acid as do 1000 grains of marble.

[43] Cavendish (1771); in *Electrical Researches*, 43; Maxwell in Cavendish (1879i, 381).

they might have, electricity and air are “extremely different” elastic fluids. This comparison is similar to his approach in chemistry of distinguishing between species of elastic air by their physical properties. His more or less simultaneous investigations in different fields suggested to him analogies to explore, a spread of interests which in another investigator might be a mark of a dilettante but which in Cavendish was a strength.

The occasion for Cavendish’s work in electricity is unknown. The fundamental researches of Watson, Franklin, and Canton belonged to an earlier time, the 1740s and 1750s. In the 1760s, British authors published several papers on electricity in the *Philosophical Transactions*, which we should look at. Two of them took up differences with foreign physicists. In 1759 Benjamin Wilson repeated Charles Cavendish’s “fine experiment” on the Torricellian vacuum, which he thought showed which electricity is plus and which also proved the existence of the ether.[44] The Russian physicist Aepinus criticized this conclusion, and Wilson answered him. Watson reported on a treatise by the French physicist Jean-Antoine Nollet, who critiized the principle of plus and minus electricity. Watson claimed and defended this principle as his own, referring to his experiments in 1745–46, which showed that electrical phenomena “arise from their electricity being either greater or less than their natural quantity.” Ebenezer Kinnersley published a letter to Franklin questioning his doctrine of a repulsive electric force.[45] Edward Delaval examined the change in a substance from electric to non- electric upon heating, rejecting an explanation by Canton, who responded.[46] Priestley published on the lateral force of electrical explosions and on colored rings on metals.[47] Lane published on a new electrometer. Watson and another author published on medical electricity. There were several papers on electricity by foreign authors, most of them by Bergman and the Italian physicist Giovanni Beccaria, in Latin. Cavendish was interested in plus and minus electricity and the repulsive force, and he would take an interest in Lane’s electrometer, but it is unlikely that any of the above papers acted as a specific stimulus; some of the papers appeared after Cavendish was already interested in electricity.

There were a few new books in English on electricity in the years before 1771, two of which were influential. Priestley’s *History and the Present State of Electricity with Original Experiments* in 1767 interested Cavendish for the experiments it conveniently brought together; he made six references to it in his 1771 paper, a majority of his references. The fourth edition of Franklin’s *Experiments and Observations on Electricity* in 1769 included a letter in which he spoke of the repulsion of negatively electrified bodies as a first principle, and in its defense he recalled Newton’s assertion of repelling forces throughout nature. Franklin’s book was not the reason for Cavendish’s researches on electricity, but it may have helped reshape them; Cavendish’s second electrical theory differs from his first in, among other ways, having just such a repulsive force as Franklin’s.

One of Newton’s legacies was his statement in the *Principia* that the way to advance natural philosophy was to to determine the forces of nature as laws, the example being his successful investigation of gravitation. Another was the “queries” in his optical treatise, a form his successors in the eighteenth century occasionally imitated. In his *History of Electricity*, Priestley combined the two legacies in asking by what law do the particles of the

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[44] Benjamin Wilson (1759, 339).

[45] Ebenezer Kinnersley (1763, 86).

[46] Edward Delaval (1761); John Canton (1761, 457–461) .

[47] Joseph Priestley (1769; 1768).

electric fluid repel one another.[48] He gave the correct answer, another legacy of Newton's. A well-known theorem of the *Principia* (which Cavendish drew on, above) states that if the force of gravity obeys the inverse square law of distance, there is no force in the interior of a gravitating spherical shell. From Franklin's observation that cork balls do not separate inside an electrified cup, Priestley inferred that the electric force obeys the same law as the gravitational force. The law of electric force was Cavendish's starting point of his theory of electricity, and his experiment on the inverse square law was an elaboration of the electrified cup. Priestley's astute observation was a possible incentive for Cavendish to investigate the law of electric force the way he did, though he was already informing himself about electricity the year before Priestley's book was published.

Another plausible stimulus (or deterrent) is ruled out. In the opening paragraph of his paper in 1771, Cavendish referred to Aepinus's *Tentamen theoriae electricitatis et magnetismi*, published in 1759. Cavendish said that only after he first wrote his paper did he learn that his hypothesis was not new, that Aepinus had used "the same, or nearly the same" hypothesis and had arrived at conclusions agreed nearly with his own. (It was Aepinus who introduced the mutual repulsion of negatively charged bodies, which Franklin eventually accepted.) Cavendish said that he had "carried the theory much farther" than Aepinus had, and that he had treated the subject in a "more accurate" manner. This is all that he said about Aepinus's theory in print. Just when Cavendish saw Aepinus's book is unclear. On 23 June of an unspecified year, he wrote to John Canton to say that he did not need to apply to Priestley for a copy of the *Tentamen* because he had since come across a copy in a London bookstore. The background of Cavendish's letter is the following exchange between Cavendish, Canton, Priestley, and Franklin. Franklin sent Priestley a copy of the *Tentamen* to help him prepare his *History of Electricity*. Cavendish knew about this copy, and not owning the *Tentamen* and wanting to see it, he asked Canton to ask his friend Priestley if he would send the book to Canton "for Mr. Cavendish."[49] When Cavendish saw the book at a bookstore, he wrote to Canton calling off his request. Roderick Home shows that the above exchanges took place in 1766,[50] five-and-a-half years before Cavendish's paper was read to the Royal Society. There are two ways of explaining the apparent disparity between what Cavendish said in his letter and what he said in his paper. The straightforward explanation is that Cavendish had, as he said, first written his paper before he saw Aepinus's book. However, if Cavendish acquired Aepinus's book in 1766, there is a problem with this explanation. His electrical manuscripts go back no earlier than his first electrical theory, in or after 1767, and it is the hypothesis of his second theory in 1771 that is the same as Aepinus's. We are to suppose that while carrying out electrical researches he ignored his own library for five years even where he had gone to the trouble to add to it a specific work on the subject. This is not out of the question. Cavendish did not always inform himself about publications on his subject, as we learn from an entry in Charles Blagden's Diary. Cavendish told Blagden that "when [he] wrote his paper on attraction, he showed his ignorance of what had been done by others."[51] He could have been referring to his late paper on weighing the world, but more likely it was to his early paper on electricity. In 1766, when Cavendish

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[48] Priestley (1767, 488).

[49] Henry Cavendish to John Canton, 23 June [1766]; in Jungnickel and McCormmach (1999, 534). John Canton to Benjamin Franklin, [1766], in Wilcox (1969/1974, 544).

[50] Roderick W. Home (1972).

[51] 8 June 1809, Charles Blagden Diary, Royal Society 5:328 (back).

inquired about Aepinus's *Tentamen*, he was in the middle of his researches in chemistry, which would lead to his first publication that year, and being busy he put the book aside or delayed its purchase.[52] The second way of explaining the apparent discrepency is that Cavendish bought the *Tentamen* in 1766 while he was engaged with his chemical experiments, and before he had time to read it Priestley's *History of Electricity* came out in 1767. Priestley, who lacked training in mathematics,[53] said, incorrectly, that Aepinus's mathematical theory was based on the wrong law of electric force, one which led to Boyle's law for air and not to the facts of electricity, and that consequently electricians would save themselves a "good deal of time and trouble" by not bothering with it.[54] Priestley's several revisions of his book left unchanged his erroneous discussion of Aepinus's theory,[55] suggesting that his electrical colleagues were insufficiently knowledgeable about the theory to point out his error. If Cavendish acted on what Priestley said, that Aepinus's force varies as the inverse power of the distance leading to Boyle's law, he could safely ignore it since he knew that that law was wrong. Compatible with this explanation is Cavendish's proof in his paper of 1771 that the law of force of the particles of air responsible for Boyle's law could not be the law of force of electrical particles. The first explanation is the more likely of the two, though the two are not incompatible. Aepinus's theory was first discussed extensively in print in English only a half century later, by John Robison. Because of its mathematics, Robison said, Aepinus's theory was the first to tread in Newton's footsteps. Robison admired Cavendish's electrical theory, which he considered an application of Aepinus's, only going much beyond it, especially in its "explanation of all the phenomena" of the Leiden jar, "examined, with the patience, and much the address of a Newton." Robison's warm appreciation came too late to make any difference to Cavendish, Aepinus, or the science of electricity.[56]

Experiments on Capacity

More completely than other fields, electricity allowed Cavendish to make full use of his skills as experimenter and mathematical theorist. In the last section we considered the electrical theory he published; in this section we consider the electrical experiments he did not publish. In his account of them, he referred to two rooms, a back and front room, one of which he compared to a sphere sixteen feet across, "about its real size." The rooms contained assorted electrical instruments, some delicate like Lane's and Henly's electrometers, some massive like Cavendish's battery of forty-nine Leiden jars, which was similar to Priestley's in 1767, the first large battery.[57] There was a seven-foot-high horizontal bar, from which bodies to be tested were suspended by silk strings. Occasionally a second person was present, an assistant "Richard," who lifted and lowered strings passed over pulleys or turned the electrical machine or felt a shock.[58]

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[52] This suggestion was made by Home in a private communication. Home also thinks that Cavendish may have been discouraged by the language in which Aepinus's book was written, Latin. "Aepinus and the British Electricians," 196.

[53] Priestley recommended electrical research because it required no "great stock" of knowledge, and "raw adventurers" like himself could make first-class discoveries. R.W. Home (1979, 136).

[54] Priestley (1767, 463).

[55] Personal communication from Robert E. Schofield.

[56] Robison (1822, 4:109–110).

[57] William D. Hackman (1978, 99–100).

[58] Maxwell in Cavendish (1879, xxix–xxx, xxxii).

Basic Electrical Apparatus

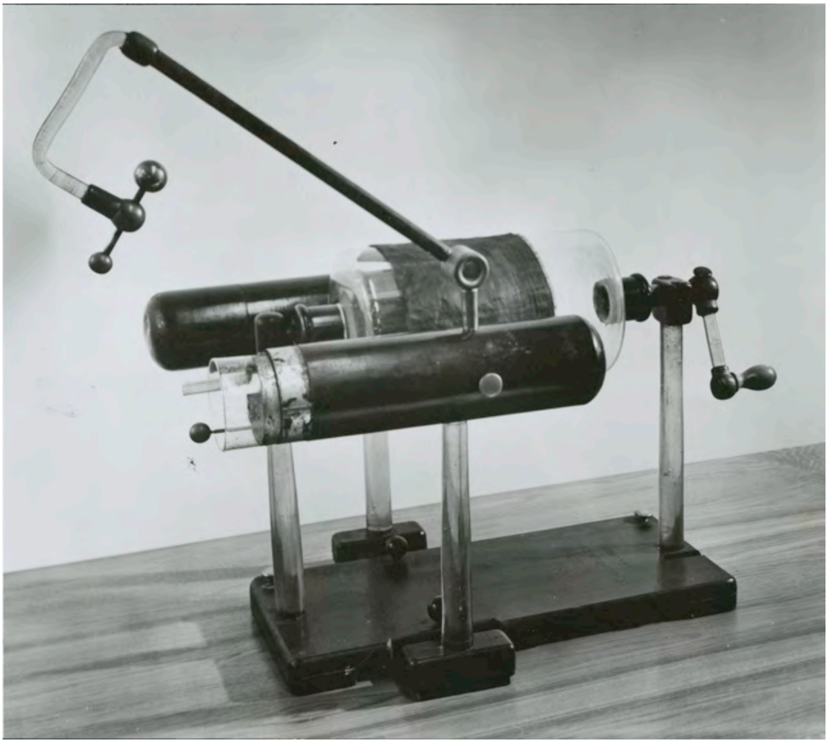

Figure 9.4: Electrical Machine. Made by Edward Nairne, stamped at the base "Nairne's/Patent/ Medico-Electrical/Machine," this instrument belonging to Henry Cavendish was presented to the Cavendish Laboratory at Cambridge by the duke of Devonshire around 1928. Its main parts are a glass cylinder with a turning handle and two metal cylinders, which contain Leiden jars. There are also a leather pad, a square of silk, and a brass discharging rod with a glass handle. Courtesy of the Whipple Museum of the History of Science, Cambridge, England.

Figure 9.5: Battery of Leiden Jars. The box is labeled JCM [James Clerk Maxwell], “Electrical Apparatus belonging to Henry Cavendish.” Photograph by the authors. Chatsworth. Reproduced by permission of the Chatsworth Settlement Trustees.

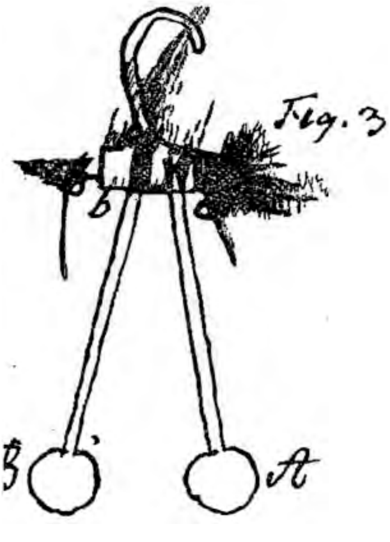

Figure 9.6: Cork-Ball Electrometer. This is the electrometer Cavendish used in his later experiments. It is made of two wheaten straws eleven inches long with cork balls at the bottom, each a third of an inch in diameter. At the top the straws are supported by steel pins on which they turn. The pins bear on notches in a brass plate, as shown. Cavendish (1879b, 120–121).

There existed only a few kinds of electrical instruments, and Cavendish did not add to them but adapted those then in use: an electrical machine for generating electricity, Leiden jars for storing it, and electrometers for measuring it (Figs. 9.4–9.6). He used several electrometers, variations of a general type. His first was a pair of pith balls about one fifth of an inch in diameter suspended by linen threads about nine inches long. The next was a pair of paper cylinders about three quarters of an inch in diameter and one inch in height suspended by linen threads about eight inches long. In later experiments he used a pair of gilded wheat straws about eleven inches in length terminating in cork balls about a third of an inch in diameter. Behind the electrometer he placed a piece of cardboard with black lines on it for judging the separation of the cork balls, and he used a guide for placing his eye thirty inches from the electrometer to ensure consistent readings. With this simple instrument, Cavendish said, he "could judge of the strength of the electricity to a considerable degree of exactness."[59] In the course of his experiments, he compared his electrometers one with the other and with Henly's and Lane's more exact electrometers. His last experiments were on electrical conduction, for which there did not yet exist a measuring instrument, a limitation he overcame, as we will see.

Several attempts had been made to determine the law of electric force by experiment, with inconclusive results.[60] In his published paper, Cavendish said that on the basis of experiments he had carried out, he thought that the electric force obeys the inverse square law, the same as gravity, but he had not made sufficient experiments to settle the matter. Two years passed before he made his decisive hollow-globe experiment. The apparatus was more complicated than it needed to be, he said, but because the experiment was of "great importance to my purpose, I was willing to try it in the most accurate manner." The relevant proposition from his theoretical paper states that if the intensity of the electric force falls off as the inverse square of the distance from the electric source, the redundant electric fluid on an electrified sphere lies entirely on its outer surface. Cavendish made two conducting globes of slightly different sizes, placing one inside the other, the inner globe measuring 12.1 inches in diameter, the outer globe standing from the inner globe by about 2/5th of an inch, the two globes connected by a wire, which could be withdrawn (Fig. 9.7). Upon electrifying the outer globe with a Leiden jar, he found that the inner one was not electrified, proof that electricity lies on the surface and that the electric force obeys the inverse-square law. The rough instrument he used for detecting electricity on the inner globe, a simple pair of pith balls, he made into an instrument of high accuracy by his method. By reducing the charge of the Leiden jar to 1/60th of its original strength and applying it to the globe, he found that the pith balls barely separated. With that measure of the sensitivity of his apparatus, he knew that the "quantity of redundant electricity communicated to the globe in this experiment was less than 1/60th part of that communicated to the hemispheres in the former experiment," from which he concluded that there was no reason to believe that the "inner globe is at all overcharged." He expressed this result in a more meaningful way: the electric force varies inversely as some power of the distance between 2 + 1/50 and 2 - 1/50, from which he concluded that there is "no reason to think that it differs at all from the inverse duplicate ratio."[61] That is, if the inverse power of the distance of the law of electric force were 2 +

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[59] Cavendish (1879b, 119, 121).

[60] For example: Stephen Gray, Cromwell Mortimer, Daniel Bernoulli, and John Robison. The latter two concluded that the electric force obeys the same law of distance as the force of gravity.

[61] Henry Cavendish (1879e, 104–113).

1/50 or 2–1/50, he would have detected a charge on the globe, if only just barely. To rule out his result as an artifact of the sphere, Cavendish repeated the experiment replacing the globe within a globe by a hollow box with a board inside.[62]

Blagden wrote to Heberden in 1787 that the French engineer and physicist Charles Augustine Coulomb had just demonstrated that the force of electricity acts "exactly according to the square of the distance."[63] Blagden, the colleague who knew Cavendish's work best, was obviously ignorant of Cavendish's earlier proof. It would seem that no one knew of it before Cavendish's unpublished papers were studied in the nineteenth century. Coulomb established the law directly using a torsion balance, and in due time the law went into history as "Coulomb's law."

The hollow-globe experiment has been discussed perhaps more than any other unpublished experiment in modern science. One reason for this interest is historical and philosophical, as is seen by the questions asked about it. Why did Cavendish assume that the law of electric force has the mathematical form of an inverse power of the distance, whether the power is 2 or any other number?[64] Do Cavendish's and Maxwell's claims for the accuracy of the experiment stand up?[65] How did Cavendish control the errors of the experiment?[66] Why did he not publish his experiments?[67] Another reason for the persistent interest is scientific, centering on the principle behind the experiment, which allows scientists to improve indefinitely on Cavendish's limits of accuracy. A century after Cavendish, at Cambridge his hollow-globe experiment was repeated with an electrometer capable of detecting a charge thousands of times smaller than Cavendish's electrometer could, showing that the electric force varies inversely as some power of the distance between 2 + 1/21600 and 2 - 1/21600. Maxwell showed that with Thomson's Quadrant electrometer, it was possible to "detect a deviation from the law of the inverse square not exceeding one in 72000." Cavendish's method is capable of far greater accuracy than Coulomb's. Since Cavendish's experiment, the electrification of concentric conducting shells "has been at the heart of the most sensitive tests" of that law.[68]

Cavendish was well satisfied with his experimental proof. The hollow-globe experiment not only determined the law of electric attraction and repulsion but also served "in some measure" to confirm the "truth" of the theory as a whole. The location of the redundant electric fluid on or extremely near the surface of a conducting globe would "by no means" have been expected without the theory. Cavendish's subsequent experiments based on the inverse square law of electric force and canals of incompressible electric fluid simulating wires provided "great confirmation" of the truth of the theory."[69]

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[62] Ibid., 112.

[63] Charles Blagden to William Heberden, 10 June 1787, draft, Blagden Letters, Royal Society 7:66.

[64] Laplace gave the first proof that for there to be no force inside a uniform hollow globe, the only possible function of the distance is the inverse square, as noted by Maxwell in Cavendish, *Electrical Researches*, 422. Laplace's proof still does not rule out other possible forces consistent with Cavendish's experiment, a point discussed in Jon Dorling (1974, 335–336).

[65] Ronald Laymon (1994).

[66] Cavendish's hollow-globe experiment and his subsidiary experiments have been likened to a "Russian doll with experiment inside of experiment." Jean A. Miller (1997, 71).

[67] Leonid Kryzhanovsky (1992).

[68] Ross L. Spencer (1990, 385). Maxwell in Cavendish (1879i, li).

[69] Cavendish (1879b, 142).

Mathematical Instruments

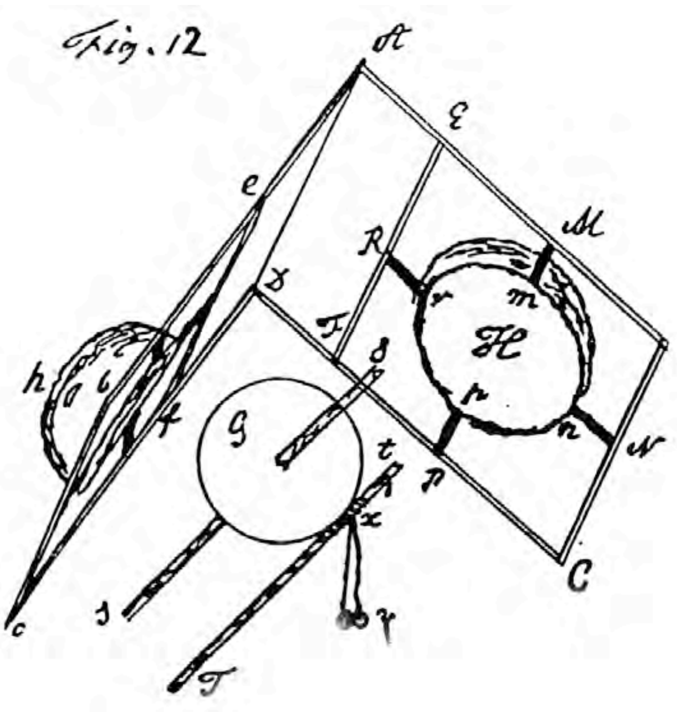

Figure 9.7: Apparatus for Determining the Electric Force. With this apparatus Cavendish demonstrated the distance dependency of the law of electric force. Upon closing, a hinged wooden frame brings together two hemispherical shells around but not touching an inner globe. The globe 12.1 inches in diameter is suspended by a stick of glass. The hemispheres and the inner globe are covered with metal foil, and a metal connection is made between the two. With the frame closed, the hemispheres are electrified with a Leiden jar. Then the metal connection is removed by a string from outside and the frame is opened. A pair of pith balls shown in the drawing is brought against the inner globe. Cavendish found that the pith balls do not separate, showing that no electricity was communicated to the inner globe. By a theorem from Newton's Principia, Cavendish concluded that the electric force obeys the inverse square law of distance. Cavendish (1879e, 104). Reproduced by permission of the Chatsworth Settlement Trustees.

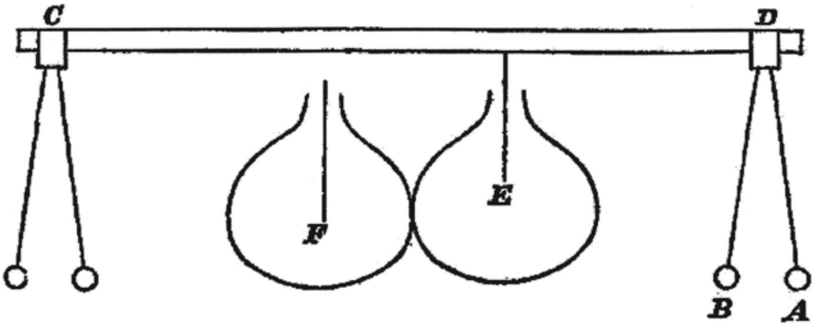

Figure 9.8: Apparatus for Determining the Electric Force. CD is a conducting rod. Electrometers suspended at C and D are similar except that the cork balls A and B can be made heavier by inserting weights. The experiment, which is described in the text, demonstrates that electric force between charged bodies depends on their charges in accordance with the theory. Cavendish (1879k, 189–193).

There is another part to the law of force. Cavendish proved experimentally that just as the law of gravitation depends not only on the distance between two bodies but also on the quantities of matter in them, the electric force between two bodies depends also on the quantities of redundant electric fluid (or of redundant matter) in them, completing the analogy between the electric force and the gravitational force. His skill in designing experiments is well illustrated by this proof, which is as inventive in its way as the complementary holllow-globe experiment. We will go through the steps, conscious that readers of biographies normally are not presented with technical arguments in detail. We justify our exception here, and again in another experimental proof in this section, by a reason we have discussed. Cavendish's life and its personal testimonies are deficient in the events that fill most biographies. We are left with knowing him as his contemporaries did, mainly through his scientific reasoning, and because of his extensive scientific manuscripts we can know him quite well, better even than his contemporaries could.

The apparatus is shown in Fig. 9.8. The object of the experiment is to prove that the electric force between two equally charged bodies varies as the product of the charge of each body, or the square of the charge of one body. At the ends of a conducting rod *C* and *D*, Cavendish attached identical pith-ball electrometers. He added weights to the pith balls of the electrometer at *D*, reducing its sensitivity to one quarter of what it was before (requiring four times the force to separate the pith balls the same distance as formerly). He electrified the bar with a Leiden jar *E* and observed the separation of the pith balls *B* and *A*. Then he connected an identical but uncharged Leiden jar *F* to the first Leiden jar *E*, dividing the latter charge equally between the two. The Leiden jar *E* was again connected to the rod. Cavendish observed that the pith balls at *C* separated by the same distance as did the weighted pith balls at *D*. The only difference was that the charge of each of the pith balls at *C* was one half of what it was formerly. The product of the charges on the two pith balls was one quarter, the same as the force. The complete law of electric force that Cavendish proved experimentally can be written m^2/d^2, where m is the charge of each body and d is their separation (Cavendish did not write it this way). Cavendish concluded that "the experiment agrees very well with the theory."[70] The experiment called only on the knowledge that charge is conserved and is shared equally between a pair of connected Leiden jars. The reasoning behind the steps of the experiment is transparent and the conclusion is convincing.

Cavendish's plan for the "work," as he called his manuscript, was to follow the proof of the law of electric force with experiments that confirmed his theory as a whole. For this purpose, he prepared a substantial paper of mathematical propositions and lemmas, numbered sequentially with those of the published theory, on the assumption that the electric force varies as the inverse square of the distance, as confirmed by the hollow globe experiment.[71] The object was to compare consequences of the law of force with measured charges of bodies of various sizes and shapes—spheres, cylinders, and circular, oblong, and square plates—connected by slender wires. He represented wires by canals of incompressible electric fluid, which he regarded as the weak point of his theory, and because he could not correct it, he was prepared to find substantial disagreement between the predicted and measured charges of bodies of various shapes and sizes. That the agreement turned out to be very close he took as a justification of his assumption and "also a strong confirmation of the truth of the theory."[72]

Cavendish's electrical theory made predictions about the electrical capacities of bodies of various sizes and shapes. Following is the second technical discussion in this section, which shows how Cavendish made electricity a measuring science. To compare the charges of two bodies B and b, he made use of a third body T, a "trial plate," which was a pair of flat tin squares that could be slid over one another to vary the area and with it the electrical capacity, as shown in Fig. 9.9. Fig. 9.10 shows how the method worked. To find if bodies B and b held the same charge, Cavendish charged two Leiden jars equally with an electrical machine. With one jar, he electrified B positively, and with the other jar he electrified the trial plate T negatively. He connected B and T by a wire and attached the electrometer to the wire. Generally the cork balls would separate, indicating either a net positive or negative charge. He would then adjust the size of the trial plate by sliding one leaf over the other until the cork balls no longer separated, indicating that the negative charge of the adjusted trial plate exactly saturated the positive charge of B. He followed the same procedure with the second body b. If the trial plate of the same size saturated b, he knew that B and b had the same charge. If however the surface area of the trial plate differed in B and b, he called on a result he had derived separately: the charge on a trial plate is proportional to the square root of its surface, so if the area of the trial plate in trying b was greater than that in trying B in the ratio of t^2 to T^2, the charge in b was different from that in B in the ratio of t to T.

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[70] Henry Cavendish (1879k, 189–193). R.J. Stephenson (1938, 58). He proved the law for bodies with the same charge m. The general law applies to bodies with different charges m_1 and m_2.

[71] Henry Cavendish (1879f, 64–94).

[72] Cavendish (1879b, 135, 142). Maxwell showed that Cavendish did not have to worry, for the result of his assumption of a canal of incompressible fluid agreed with the actual case. *Electrical Researches*, 375.

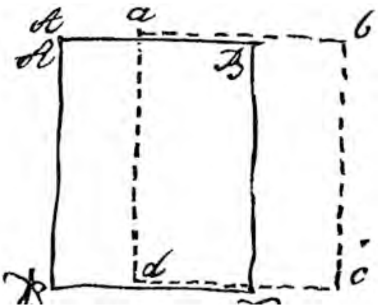

Figure 9.9: Trial Plate. Two flat tin plates, ABCD and abcd, slide over one another, increasing or decreasing the total size and with it the total charge. Cavendish (1879b, 1151–1216).

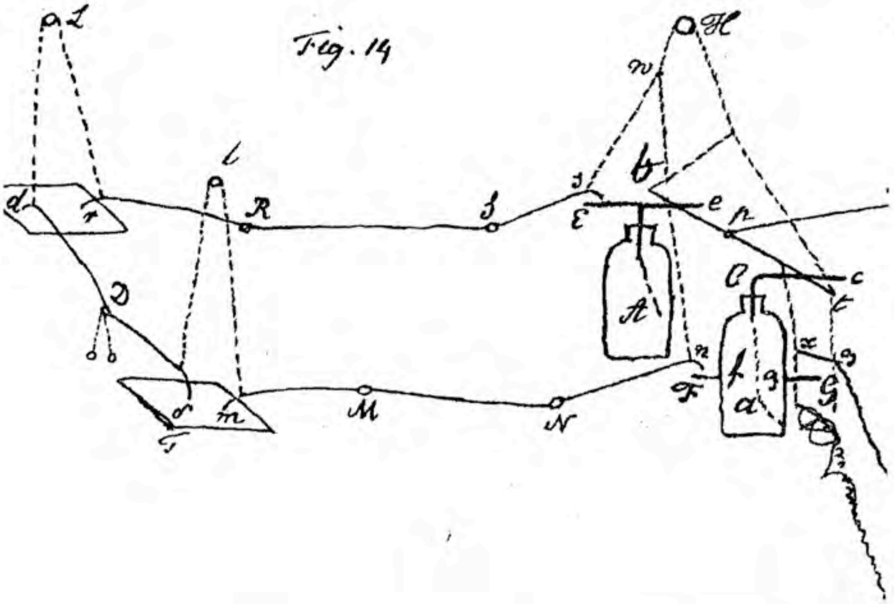

Figure 9.10: Apparatus for Determining Charges of Bodies. T is a trial plate. B is a body to be measured. It will be replaced by a second body b. The charges of the two bodies are compared, as explained in the text. A and a are Leiden jars, and D is an electrometer. Cavendish (1879b, 116–117).

As in the previous account of the experiment on the law of force, to perform this experiment Cavendish needed two Leiden jars, an electrometer, and an electrical machine, and he also needed a body with an adjustable capacity, a trial plate.[73]

Cavendish spoke of the "charges" of bodies rather than of their "capacities," our term. As a standard for measuring charges, he selected a conducting sphere of 12.1 inches diameter, the same one he used in the hollow-globe experiment. Having shown that the charges of similar bodies are as their linear dimensions, he expressed the charge on a given body as equivalent to the charge of a globe of a certain diameter when equally electrified, or as so-many "globular inches" or simply "inches if electricity."[74] We would say that the "capacity" of the given body is the same as that of a sphere of diameter of so-many "inches." It is usual to discuss Cavendish's experiments on the charges of bodies as experiments on the capacity of bodies.

For the measurement of capacities, Cavendish used another type of plate too, a glass plate coated with a conducting material in the manner of a Leiden jar. He prepared three sets of glass plates coated with circles of tinfoil, the plates of each set being of the same capacity, and each set having three times the capacity of the previous set; he prepared a tenth plate having a capacity equal to the total capacity of the the set with the largest capacity. With a selection from these ten plates, he could assemble a capacity from 1 to 64. The group of graduated capacitances was to become the principal tool in electrostatic measurements.[75]

The next "Part" of the work contained Cavendish's experiments on the charges of coated plates of glass and other nonconductors (Figs. 9.11–9.12). For these experiments, he introduced another version of "trial plates," glass plates with coatings of foil of the same size on both sides, the area of one of the coatings being adjustable by a sliding metal plate.[76] Before he began testing his theory of coated plates, he examined likely sources of errors. He found that the electricity spread onto the glass around the edges of the foil of the trial plates in two ways, one gradual and one instantaneous. The first could be minimized by making the measurement quickly; the second way could not be helped. The distance of the instantaneous spreading was very small, 0.07 inches on a thin glass plate, but it was significant, and he carried out experiments to determine how much the spreading affected the area of the coating, making a correction for it.[77] His theory explained the coated plate perfectly well in a qualitative way, as he had shown in his published paper of 1771, but when he measured the charge of a coated plate he found that it was eight times greater than the charge predicted by his theory, a discrepancy which could not be attributed to experimental error. "This is what I did not expect before I made the experiment," he wrote in the manner of understatement. Fearing that the "reader" might suspect that there was "some error in the theory," he made experiments in an attempt to account for the discrepancy. At this point he was helped by Aepinus, who in a paper in 1756 described experiments on the charge of a plate of air. Cavendish now carried out experiments of his own on plates of air, determining that the air was not charged. He then replaced the glass of a coated plate with air, and finding that this brought the computed and measured values close together, he concluded that the

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[73] Cavendish (1879b, 115, 122).

[74] Maxwell in Cavendish (1879, l–li). We say that the electrical "capacity" of a body is its charge when its potential is unity. This agrees with Cavendish's understanding. His unit of capacity is that of a sphere of 1-inch diameter, so that a body with a capacity of "*n* inches" has *n* times the capacity of a 1-inch sphere.

[75] Maxwell in Cavendish (1879, l). William Garnett (1885, 138–139).

[76] Henry Cavendish (1879a, 147–150).

[77] Ibid., 150–164

cause of the discrepency lay entirely in the material of the non-conductor, the glass itself. To explain the factor of eight, he supposed that glass has an electrical structure of nonconducting and conducting parts, arranged in alternating parallel layers, the thickness of any one conducting layer of glass being "infinitely small," and the total thickness of the nonconducting parts being 1/8th the thickness of the conducting parts. To support the explanation, he made an "analogy between this and the power by which a particle of light is alternately attracted and repelled many times in its approach towards the surface of any refracting or reflecting medium." He directed the reader to John Michell's explanation of Newton's fits of easy reflection and transmission of light, according to which each particle of a refractive or reflecting medium is surrounded by a great many equal intervals of attraction and repulsion alternately succeeding one another, as shown in Fig. 9.13. With the discrepancy between his theory and his experiments tentatively resolved, Cavendish proceeded with the experiments on coated plates. When he tried different kinds of glass and other nonconducting substances for the plates, he made a fundamental discovery, one which Michael Faraday would rediscover in the next century, that of specific inductive capacities.[78] Like the thermal properties of different substances—in the 1760s Cavendish investigated specific and latent heats of many substances—and like the gravitational properties of different substances—in the 1760s he determined the specific gravities of different air-like substances—and like the optical properties of different transparent substances—in the 1780s he determined their different refractive and dispersive powers—the electrical properties of different substances vary quantitatively and characteristically. In the course of testing the predictions of his electrical theory, his experimental technique itself proved to be a tool of discovery.

Cavendish went to lengths to decide which factors affected the accuracy of the tests of the theory. He measured the electrical capacity of every part of the apparatus and the room. He found that the capacity of his battery of forty-nine Leiden jars was 321,000 inches, or a globe five miles in diameter. To reduce the loss of electricity running into the air and over the surface of non-conductors, he charged the Leiden jars "extremely weakly." He calculated the inductive influence on his apparatus of the floor, ceiling, and walls, a precaution analogous to that of the astronomer who considers the disturbing gravitational influence on his instruments by nearby mountains. He studied the effect of the placement and the length of conducting wires and of the separation of the charged bodies. He did experiments to learn if the ratios of charges of bodies were affected by different degrees of electrification, by heat, by the plus or minus sign of electrification, by substance, and by time. To partially compensate for an "error" in the use of trial plates arising from unknown causes, "for greater security" he took multiple observations, comparing "each body with the trial plates 6 or 7 times." "For the sake of accuracy," in taking a measurement, he used two trial plates and took the mean of the result. In an experiment on a very weak Leiden jar constructed of air instead of glass, he placed his little finger on one of the plates, feeling a "small pulse," and upon varying the experiment, he was unable to "perceive any difference in the feel." His assistant was asked to try the experiment, and he also felt no difference, adding confirmation. He attended to the "error of the experiment," concerned that the differences between his results and the theory were not owing to an "error in the theory." That the differences were "so small" he regarded as a "strong sign that the theory is true."[79] By comparing his

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[78] Henry Cavendish, "Experiments on Coated Plates," *Sci. Pap.* 1:151–188, on 168, 172, 175–176, 179–181. Michell's account is reported in Joseph Priestley (1772c, 1:309–311).

[79] Cavendish (1879b, 127, 135; 1879d, 254); Maxwell in Cavendish (1879, vi).

measurements with modern ones, we see how successful he was. With his careful technique, he found the ratio of the capacity of a circular disk to that of a globe of the same diameter to be 1/1.57; the theoretically calculated value today is 1/1.571.[80]

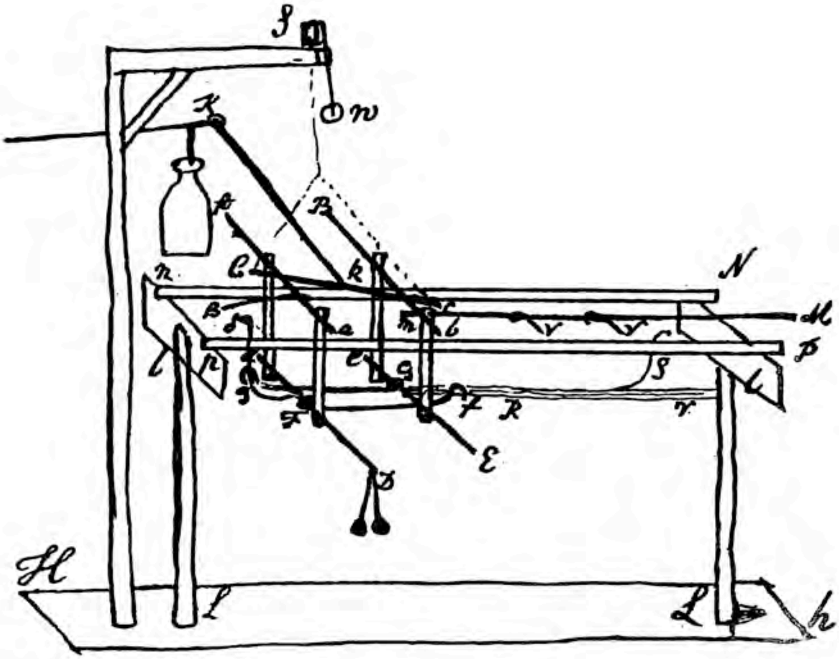

Figure 9.11: Apparatus for Determining Charges of Coated Plates. Standing on the floor, this seemingly rickety contrivance of wood and glass sticks, wires, and Leiden jar is actually portable and is described by Cavendish as compact. Two plates coated on both sides in the manner of a Leiden jar are electrified together, one plate serving as a standard; a communication is made between the upper coating of one plate and the lower coating of the other; if the original charges of the two plates are the same, the pith balls at D serving as an electrometer will not separate, but if the charges are different, they will. Cavendish (1879, 145) Reproduced by permission of the Chatsworth Settlement Trustees.

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[80] Cavendish (1879b, 114). Stephenson (1938, 56).

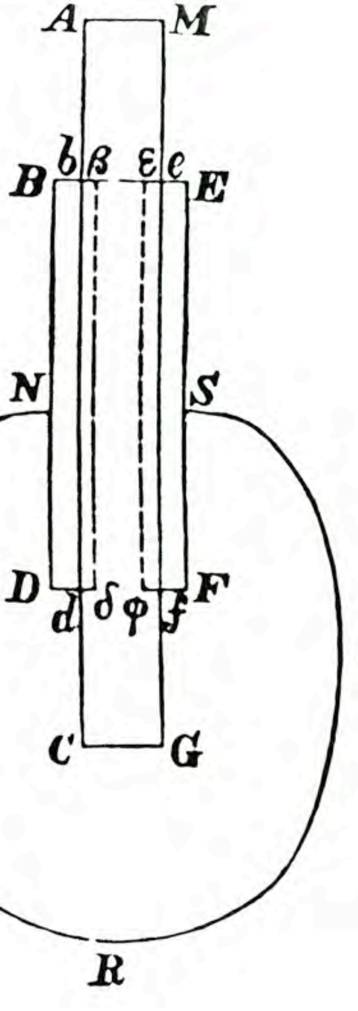

Figure 9.12: Leiden Jar. Cavendish analyzed the phenomena of the Leiden jar, or condenser, using this diagram. ACGM stands for a plate of glass seen edgeways, on either side of which are plates of conducting matter, such as metal foil. The dotted lines indicate the possible penetration of the electric fluid into the glass from the conducting plates. To charge the Leiden jar, one conducting plate is electrified, the other grounded. If a canal (wire) NRS is connected to the two conducting plates, the redundant electric fluid passes from one to the other. Cavendish (1771) in *Electrical Researches*, 57.

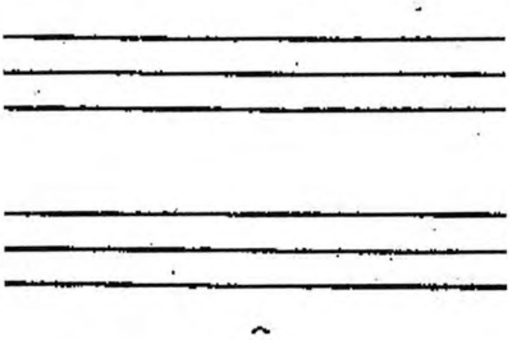

Figure 9.13: Electrical Structure of Glass. Cavendish found that the charges of glass plates coated with foil, which are simple Leiden jars, were eight times what they should have been by the theory. "There is a way of accounting for it," which he gave two versions of. Cross-section of the glass is shown. Franklin had proven that the charge of a Leiden jar is in the glass and not in the foil. In the drawing the electric fluid is free to move in the outside layers of the glass, and also in the interior, but not on the inside layers. If each of the inside layers is 1/16th of the thickness of the glass, together they make up an insulating layer of 1/8th, in agreement with the theory. Cavendish thought that it was more likely that conducting and insulating layers alternate throughout the glass, the sum of the thicknesses of the insulating layers being 1/8th of the thickness of the glass, as before. He justified this supposition by an analogy with John Michell's explanation of Newton's "easy reflection and transmission" of light: particles of light are alternately attracted and repelled many times in their approach to a surface. Cavendish (1879) in *Electrical Researches*, 172–175.

Conduction

In his paper on electrical theory in 1771, Cavendish did not include electrical conduction as one of the principal phenomena of electricity, though he touched on the subject: electric fluid flying through the air between the knobs of a Leiden jar, resistance to the motion of electric fluid in wires, penetration of electricity into glass, and dependence of the strength of shocks on the quantity of electric fluid and its velocity.[81] In late 1773, following his experiments on the charges of bodies, he turned his attention to conduction, and from then on, all of his electrical experiments were on this subject, obtaining results in close agreement with modern ones. Because he did not prepare a paper on it, we might conclude that he found his study of conduction less conclusive than his other electrical researches, but he gave no sign of dissatisfaction as far as he took it.

In general, in his experimental work Cavendish's depended heavily on sight, with assistance from the other standard senses: touch, hearing, and in his chemical researches smell

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[81] Cavendish (1771); in *Electrical Researches*, 57–61).

and taste. We have organs for other senses such as those for heat, which Cavendish also made use of. We lack a sense organ for electricity, but high voltages applied to different parts of the skin affect various sense organs, registering as pain, pressure, cold, heat, even taste. Because continuous current electricity was yet undiscovered, Cavendish relied on transient discharges of Leiden jars, the strength of which he measured by an electrically stimulated sensation in the skin of his hands and in the internal nerves of his wrists and elbows.

His initial object was to determine a mathematical relation between the resistance of conducting bodies and the velocity of the electric fluid moving through them, assuming that the resistance is proportional to some power of the velocity. His measures for the resistance were the heights and weights of columns of conducting solutions. By equalizing the shocks he felt by passing discharges through two such columns, he was able to determine the power of the velocity without having to know the velocity. His first experiment made the resistance vary as the 1.08 power of the velocity, his next experiment as the 1.03 power, which is where the matter stood at the end of 1773.[82]

A year and a half later Cavendish returned to experiments on electrical conduction. He began by deriving a formula that showed that in a divided circuit, where the discharge of a battery passed through both Cavendish and another conductor, the greater the resistance of the other conductor, the "more exact" the trial was, for more of the discharge passed through him. In this derivation he assumed that the power of the velocity is exactly 1, the value to which his previous experiments with conducting solutions pointed; that is, he assumed that the resistance is proportional to the velocity. If the velocity is identified with the strength of current, his conclusion is identical to the law Georg Simon Ohm arrived at in the next century, $V = IR$.[83]

Cavendish's use of the power 1.00 in the derivation above may have been convenience. In his experiment on the law of electric force, he concluded that the "force "must be inversely as some power of the distance between that of the 2 + 1/50th and that of 2–1/50th, and there is no reason to think that it differs at all from the inverse duplicate ratio."[84] He made no comparable statement about the power of the velocity of electric fluid. A difference in the two cases is that the law of electric force was the basis of a theory, and having reason to think that the power of the distance of the law is exactly 2, he designed an experiment to test that law. By contrast, the power of the velocity was not the basis of a theory, and he did not design an experiment to prove that it is exactly 1. In his published paper on the electric force, he began his comparison between the theory and experiments with a statement about the readiness of some bodies to allow the electric fluids to pass between their pores and not other bodies. What the difference between conducting and non-conducting bodies "is owing to I do not pretend to explain."[85] His theory did not take up electric conduction, as Maxwell recognized. After showing that if the power of velocity is 1, Cavendish's proportionality between velocity and resistance can be interpreted as Ohm"s law, Maxwell wrote: "The exactness of the proportionality between the electromotive force and the current in the same conductor seems, however, to have been admitted, rather because nothing else could account for the consistency of the measurements of resistance obtained by different methods, than

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[82] Maxwell in Cavendish (1879, lix). Henry Cavendish (1879d, 293–294; 1879g, 332–333; 1879h, 359).

[83] V is voltage, I is current, R is resistance. Cavendish did not write the equation. Henry Cavendish (1879c, 311–312). The first date of the new experiments is March 1775.

[84] Cavendish (1879e, 111–112).

[85] Cavendish (1771); in *Electrical Researches*, 44.

on the evidence of any direct experiments."[86] This was not research that Cavendish would have considered ready for publication.

The occasion for Cavendish's return to experiments on conduction was an interest in an electric fish. Long before Luigi Galvani at the end of the eighteenth century, animal shocks had been recognized and studied, but their identity with electricical discharge had yet to be experimentally demonstrated. With Cavendish's help, an electric fish was shown to be capable of delivering shocks with common electricity. By this indirect route Cavendish revealed to the public parts of his understanding of electrical conduction.

A number of species of fish belonging to more than one genus are known to use electricity as a defense. Early experiences of the human species with electricity may well have been by this means: Egyptian tombs portray fishermen with the electric eel of the Nile River, and the electric ray is depicted in the ruins of Pompeii. Pliny wrote of the ray that "from a considerable distance even, and if only touched with the end of a spear or staff, this fish has the property of benumbing even the most vigorous arm, and of riveting the feet of the runner, however swift he may be in the race." Its numbing property gave rise to its Greek name, "narke," having the same root as "narcotic," and its Roman name, "torpedo," from "torporific." Biology subsequently made distinctions between electrical fish, rays, eels, and so on, naming them accordingly.[87]

Known in antiquity and in the Renaissance as a magical fish, the torpedo retained its occult reputation into the eighteenth century but not beyond the experiments of the 1770s.[88] The fish enters the history of modern physics with the Dutch physicist Musschenbroek, who likened its shock to the one he felt upon discharging a Leiden jar through his body. He suggested that the torpedo is an electric fish, and the name stuck.[89] The torpedo is one of a number of fishes capable of delivering a shock, the most formidable of which is a South American eel, the *Electrophorus electricus*, called "Gymnotus." This large, almost blind, sluggish fish with small teeth and no spines or scales was said with some exaggeration to kill men and horses. From America the Royal Society received reports that the Gymnotus gives a "true electric shock," that its shock is "wholly electrical."[90] The identification of the singular power of the Gymnotus with electricity may be one reason why John Walsh, a fellow of the Royal Society, began to experiment on the torpedo.[91] From La Rochelle, France, where he went on a torpedo hunt, Walsh wrote to Franklin that the effect of the torpedo was "absolutely electrical."[92] The back and breast of the fish were found to have different electricities, like the sides of a Leiden jar, leading Walsh to wonder if its effect could be exactly imitated by one. To learn more about his fish he enlisted the services of the anatomist John Hunter, who upon dissecting a specimen was surprised by what he found: the torpedo has a pair of electrical organs, each of which has about 470 prismatic columns, and each column is divided by horizontal membranes, 150 to the inch, forming

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[86] Maxwell in Cavendish (1879, lix).

[87] R.T. Cox (1943, 13–14).

[88] Brian P. Copenhaver (1990, 278–279).

[89] Leonid N. Kryzhanovsky (1993, 119).

[90] Hugh Williamson, who had done experiments on the fish in Philadelphia in 1773, was then in London (1775). From Charleston, Alexander Garden wrote that several specimens of the fish were going to be sent to England (1775).

[91] Cox (1943, 14). W. Cameron Walker (1937, 88–90).

[92] John Walsh to Benjamin Franklin, 12 July 1772, quoted in John Walsh (1773, 462).

tiny spaces filled with fluid.[93] Hunter presented the Royal Society with male and female specimens of this intricately structured animal, and Walsh submitted a paper to the Society in which he said of the torpedo that "*the Leyden phial contains all his magic power.*"[94] In 1774 Walsh was awarded the Copley Medal for his experiments on the electrical nature of the fish, on the occasion of which the president of the Society John Pringle said that since "between lightning itself and the Leiden Phial there is no specific difference, nay scarcely a variety, as far as is known, why then should we unnecessarily multiply species and suppose the torpedo provided with one different from that which is everywhere else to be found?"[95] One of the rules of reasoning in natural philosophy was not to multiply causes, yet the case for the electrical nature of the torpedo had not been made to everyone's satisfaction. The electrician William Henly made an "artificial torpedo" of conducting materials, finding that it exhibited "no attraction or repulsion of light bodies, no snap, no light, nor indeed any sensation." He thought that the real torpedo was in the same predicament as the artificial one, incapable of delivering an "*electrical shock.*"[96] This is where the subject stood at when Cavendish took it up. In 1776 he published a second paper on electricity, on the shock of the torpedo.[97]

Walsh said that Cavendish was the "first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air."[98] Since Cavendish had not published his experiments on electrical conduction, Walsh probably received this information from him by request. A main objection to the claim that the torpedo possesses electricity was that its shock is delivered underwater where the electric fluid has easier channels than through the victim's (or experimenter's) body. The objection was based on the commonly held but incorrect view that all of the electric fluid flows along the "shortest and readiest path." Cavendish explained that the path it actually takes depends on the relative resistances of all the paths available to it. He gave an exact description of the flow of electricity through a divided circuit, a subject which entered physics at a much later date. From his knowledge that the length of spark from a battery of Leiden jars varies inversely as the number of jars in the battery, he reasoned that the electric organs of the torpedo were equivalent to a great number of Leiden jars connected like a battery. The analogs of Leiden jars were weakly electrified, but because of their great number, they could store a large quantity of electricity and deliver a strong shock with a charge unable to cross the least space of air. Cavendish answered another common objection with the observation that the discharge of the torpedo is completed so quickly that pith balls in contact with the animal do not have time to separate. To prove the correctness of his explanations, Cavendish built an artificial torpedo. His first version was cut out of wood in the shape of the fish, but because it did not conduct as well as he thought the real fish did, he built a second one by pressing together shaped pieces of thick leather like the "soles of shoes" to represent the body and attaching thin pewter plates to each side to imitate the electric organs (Fig. 9.14). With glass-insulated wires he connected the pewter plates to a battery, and encased the whole in sheepskin leather soaked in salt solution, the stand-in for the skin of the

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[93] John Hunter (1773, 484–485).

[94] John Walsh (1774, 473).

[95] John Pringle (1775b). Quoted in Dorothea Waley Singer (1950, 251).

[96] William Henly to William Campton, 14 Mar. 1775, Canton Papers, Royal Society, Correspondence 2:104.

[97] Henry Cavendish (1776a); in *Electrical Researches*, 194–215.

[98] Walsh (1773, 476).

torpedo in a salty sea. Discharging different numbers of Leiden jars through the artificial torpedo and placing his hands on or near it in the water, he found that the sensations agreed with descriptions of shock of the real torpedo.

To confirm his finding, Cavendish invited into his laboratory a number of interested persons: the torpedo anatomist Hunter; Lane, whose electrometer Cavendish was using; Nairne, whose battery and coated glass plates he was using; Priestley, who was in London on a visit; and Thomas Ronayne, a skeptic.[99] The latter said that he would have to "give up his reason" to believe that the tissues of the fish could accumulate enough electricity to deliver a shock. He left Cavendish's laboratory a believer, we presume, since Cavendish recorded in his notes of the visit, "Mr Ronayne felt a small shock."[100] From Hunter's observations, Cavendish calculated that the torpedo had nearly fourteen times the electrical capacity of his battery; powerful as his battery was, the battery of the real fish was superior to it. By experiment, he showed that the greater the capacity and the weaker the electrification of the source of the shock, the more the shock resembled that of the electric fish. He concluded that "there seems nothing in the phenomena of the torpedo at all incompatible with electricity."[101]

Cavendish's was not the final word on the subject. The voltaic battery provided a better model for the electric organs than the Leiden jar battery, and Davy, Faraday, and others would perform the definitive experiments on the electrical nature of the several kinds of electrical fish. Although Cavendish thought that it was likely that the electrical fish contains something "analogous" to the Leiden jar battery, he also considered that there might be no such thing, envisioning the possibility that the electric fluid is not stored but gradually transferred by a small "force" through the substance and over the surface of the body of the fish, anticipating the voltaic battery and the associated fundamental concept of electromotive force.[102] (We run the risk of becoming tiresome by mentioning Cavendish's "anticipation" of later discoveries. That he did so, however, has been a persistent reason for the interest the world has come to take in him.)

In his paper on the torpedo, Cavendish said that he intended to lay before the Royal Society some experiments on conduction. He never did, but he gave a result that would have built anticipation: "iron wire conducts about 400 million times better than rain or distilled water; that is, the electricity meets with no more resistance in passing through a piece of iron wire 400,000,000 inches long, than through a column of water of the same diameter only one inch long."[103] Cavendish did not say how he came by these numbers, but his reputation for accuracy was such that they were repeated by others without question. From an unpublished experiment, we know in general how he got it. It is the only experiment on iron wire and a salt solution in his surviving papers, and it is not the same as the one he reported in his published paper on an electric fish, but the method would have been the same.

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[99] The guests are named in Cavendish's laboratory notes for 27 May 1775. *Electrical Researches*, 313.

[100] Ibid. Letter from William Henly to John Canton, 21 May 1775, Canton Papers, Royal Society; quoted by Maxwell in Cavendish (1879, xxxvii).

[101] Cavendish (1776a); in *Electrical Researches*, 213.

[102] Cox (1943, 21–22).

[103] Cavendish (1776a); in *Electrical Researches*, 195.

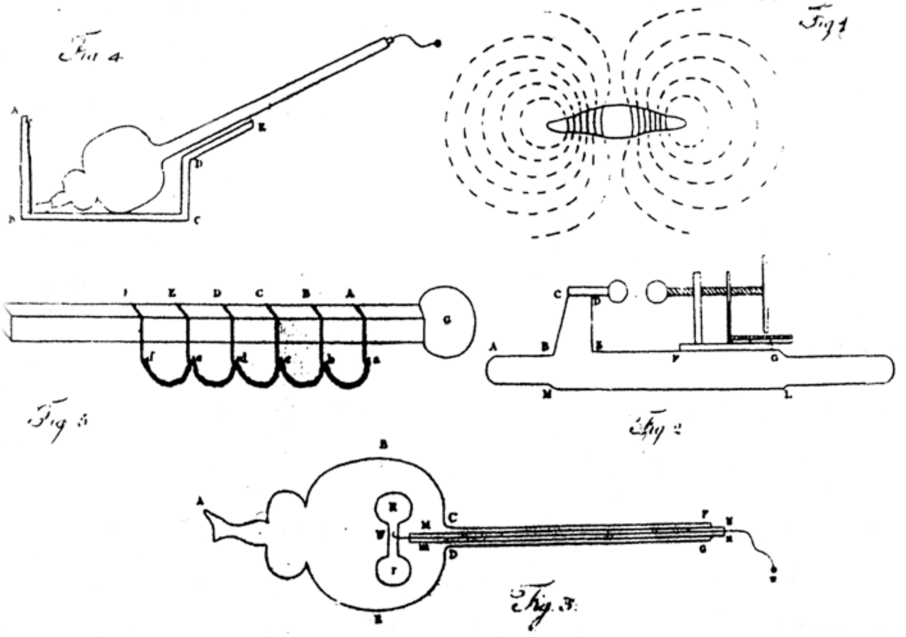

Figure 9.14: Artificial Electric Fish. In Figure 1, the solid line is the outline of an electric fish, or "torpedo," immersed in water. The dotted lines are the direction of flow of the electric fluid. When a person places his hands on the top and bottom of the fish or even only in water in the vicinity of the top and bottom, some fluid will flow through him. Cavendish's use here of the idea of lines of current did not become established until the next century. Figure 2 is Cavendish's handheld modified version of Timothy Lane's electrometer, made of brass and wood, indicating the distance a spark flies. Not shown is the pith-ball electrometer he used to estimate the strength of the charge. Resembling a stringed musical instrument, the drawing in Figure 3 is the artificial torpedo. Cut to the shape of the fish, a piece of wood 16¾ inches long and 10¾ inches wide with a handle 40 inches long is fitted with a glass tube MNmn. A wire passing through the tube is soldered at W to a strip of pewter, which represents the electric organs. The other side of the apparatus is fitted exactly the same way, with tube, wire, and pewter. With the exception of the handle, the whole is wrapped with a sheet of sheepskin. Later he replaced the wood with leather. Figure 4 shows the apparatus immersed in a vessel of salt water. Figure 5 shows a device for seeing if the shock of the artificial torpedo can pass through a chain. Through the wires and the body of the artificial fish, Cavendish discharged portions of his battery of 49 extremely thin-walled Leiden jars. The drawing appears in Henry Cavendish (1776a), Leonid Kryhanovsky (1993).

The account comes at the very beginning of his experiments on conduction, in 1773. Forming a divided circuit with the iron wire and his body, he compared the shock of a discharge through it with the shock of a discharge passing through a conducting salt solution. In his words, the shock of two Leiden jars “had its choice whether it would pass through 2540 inches of nealed iron wire, 12 feet of which weighed 14.2 grains, or through my body, each end of the iron wire being fastened to a pretty thick piece of brass wire which I grasped tight, one in one hand and the other in the other, and with them discharged the jars. It was found that when the straw electrometer separated to 1 + 0, I just felt a shock in my wrist, and when it separated to 2 + 0, I felt a pretty brisk one in them but not higher up. I then gave the shock its choice whether it would pass through my body, or 5.1 inches of a column of a saturated solution of sea salt contained in a glass tube ….” He found that the shock in the two cases was the same, and from the measures of the experiment, he calculated the resistance of the iron wire compared with that of the salt solution. Maxwell matched Cavendish’s experiment with a much later and very accurate comparison, remarking that “the coincidence with the best modern measurements is remarkable.”[104]

In his earlier experiments on the charges of bodies, Cavendish found that coated plates made of different nonconducting substances had different electrical capacities, and in his experiments on conduction, he measured the different resistances of different substances. To carry out the measurements, he placed the substances—solutions of table salt and other solutes of varying concentrations—in calibrated tubes about a yard long, with wires inserted at each end as electrodes. To vary the resistance of a solution, he simply slid one of the wires, changing the effective length of the solution. Because the wire has so little resistance compared with that of the solution, he could assume that when the current passing through the solution reached the sliding wire, all of it would flow through the wire. His technique was to insert himself in series with a solution and a Leiden jar, forming an electric circuit. Holding a piece of metal in each hand, he touched one piece to the knob of a Leiden jar and the other piece to one of the electrodes of a tube (the wire from the other electrode of the tube running to the other side of the Leiden jar), the discharge of the closed circuit passing through the solution and his body. For the purpose of comparing one conducting solution with another, he first prepared six equally charged Leiden jars. He then took shocks from six discharges passing alternately through one solution and then the other, judging whether the shock of the second solution was greater or less than that of the first, the solution causing the greater shock having the least resistance. To make a finer judgment, he adjusted the wire in one of the solutions to make their resistances more nearly equal and then repeated the experiment. By equalizing the shocks in this way, he was able to decide exactly what length of the second solution was equivalent to the length of the first solution. By designating a certain solution as a standard, he could compare the resistances of all of the solutions, in this way measuring them. Cavendish’s accuracy in this was “truly marvelous” according to Maxwell, who repeated the experiments in the Cavendish Laboratory, taking discharges through his body as Cavendish had. Cavendish’s resistances were consistent with one another and remarkably close to those obtained by experimenters using continuous currents and galvanometers, the instrument invented forty years later for the purpose.[105]

To see just how Cavendish could make an exact investigation of conductivities of substances on the basis of electric shocks, we look closer at a typical experiment. The method

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[104] Cavendish (1879d, 294–295). Maxwell in Cavendish (1879, 443–444).

[105] Maxwell in Cavendish (1879, lvii–lviii). Henry Cavendish (1879g, 321–343).

was the one just described. The object was to compare the conducting power of a saturated salt solution (tube 14) with that of a standard dilute salt solution (tube 15). Keeping the separation of the two wires in the saturated salt solution constant, Cavendish varied the separation of the wires in the other tube until he was satisfied that the shocks were nearly the same. At this point in the procedure he began keeping a record. Over several trials, he made fine adjustments, alternately slightly widening and lessening the separation of the wires (varying the effective length of the conducting solution in tube 15), experiencing slightly greater and lesser shocks, then estimating the separation that would make the resistance of the two tubes exactly equal by taking an average of the readings. The following table shows how he did this.

Distance of wires in tube 15	tube 14	shock in tube 15 than in tube 14
6.5 inches	40.7 inches	very sensibly less
5.8 inches		sensibly less
3.5 inches		sensibly greater
4.2 inches		scarce sensibly
5.3 inches		just sensibly less

The left-hand column shows that he narrowed the separation and then widened it again. He averaged the readings, obtaining 4.7 inches. The average of the five readings is a larger number than that, but Cavendish did not make a mistake. He clearly considered the first reading (“very sensibly“) to be too large for the comparison and left it out, and the average of the remaining four readings is as he said. The effective resistance of the solutions is proportional to the separation of the wires. Cavendish stated the result as a comparison of conductivity, which is inversely proportional to resistance: the saturated salt solution conducts 40.7/4.7, or 8.6, times better than the dilute salt solution. He repeated the experiment with the same two solutions using different tubes, obtaining a narrow range of values 8.94, 9.61, 9.02. He varied the experiments by changing the concentration of salt, by comparing a salt solution with distilled water, and by using different tubes.[106] From experiments with tubes of different diameters, he arrived at the important result that the resistance of conducting substances is independent of the strength of the current passing through them. The disagreeableness of his method, his experiencing numberless electrical sensations in the wrists and elbows, was more than compensated for, we think, by the bounty of new facts, which he could not have foreseen or have got any other way.

Cavendish’s investigation of conduction touched on his early chemical work. In one trial he compared the resistances of plain water and water impregnated with fixed air generated by dissolving marble in oil of vitriol, and in other trials he found the resistances of this acid and of alkaline solutions such as sal ammoniac. His investigation also touched on another major field, where he looked at the effect of heat on the conductivity of salt so-

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[106] Cavendish (1879g, 321).

lutions.[107] And as he had in his experiments on air, he sometimes made comparisons of resistances using sound, calling on a different sense.[108]

To discuss electrical conduction, Cavendish used terms from mechanics. He spoke of the degree of electrification as a "force," of the electric fluid's "velocity," of the fluid meeting "resistance" to its flow, and of the "strength of shock" as the product of the "quantity [of fluid] which passes through your body" and the "velocity with which it passes through your body," the electrical analog of momentum in mechanics, the Newtonian measure of the force of ordinary matter in motion: when the discharge of a Leiden jar "passes through the body of any animal, it will by the rapidity of its motion produce in it that sensation called a shock." When Cavendish discharged a Leiden jar through his body, the motion of the electric fluid was opposed by the resistance of his body, performing work.[109] To look ahead, in his paper on the mechanical theory of heat in the late 1780s, Cavendish stated his understanding of electric conduction. He questioned the common idea that the electric fluid moves with a very great velocity. When a Leiden jar is discharged through a very long wire that is cut in the middle and at the ends, the sparks in the middle and at the ends appear to be simultaneous, but that says nothing about the velocity of the electric fluid. The electric fluid that issues from the jar does not move from the positive electrode to the negative electrode; it does not move far at all, but instead it pushes the electric fluid in front of it, propagating "the motion through the wire, just as the motion of the particles of air propagate sound; & the swiftness with which the motion is propagated through the wire does not at all depend on the velocity of the electric fluid, any more than the velocity of sound depends on that with which the particles of air vibrate."[110] Cavendish's analog to electric conduction, the propagation of sound, is understood mechanically. In the same paper on the mechanical theory of heat he explained mechanically the heat generated by passing a discharge through a wire. At the time of his electrical experiments, he considered the effect of heat on conduction, but not the heat attending conduction. When later he explained the heat of conduction with help from his theory of heat and his electrical theory of 1771, he said that he was surprised, that he had thought that he could not explain the heat caused by an electrical discharge of a Leiden jar through a wire. He did not have an electrical theory of conduction, and what progress he made in understanding conduction came from mechanics.

In developing and presenting his electrical researches, Cavendish's model, as we have pointed out, was Newton's *Principia*, which suggests a partial motive behind his conduction experiments. Book II of the *Principia* "The Motion of Bodies (in Resisting Mediums)," the first section of which is about the motion of a body "resisted in the ratio of its velocity."[111] If the "body" is taken to be electric fluid, it is resisted "in the ratio of its velocity" when it is discharged through a conducting substance, as Cavendish determined by experiment. The main proposition in this section of the *Principia* is about the paths of bodies such as projectiles acted on by gravity moving through a resisting medium such as air, not about the resistance to the motion of the air, the analog to the resistance to the motion of the electric fluid. Yet Cavendish might have seen a rough parallel between his researches on conduction

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[107] Ibid., 324.

[108] Ibid., 341.

[109] Cavendish (1771); in *Electrical Researches*, 58; (1776a); ibid., 199; (1879c, 311). Maxwell in Cavendish (1879), 437–438.

[110] Russell McCormmach (2004, 190).

[111] *Sir Isaac Newton* (1962, 1:235).

and Newton's in Book II and, in general, between the mechanics of ordinary matter, which is divided into statics and dynamics, and the mechanics of electric matter. He had completed the statical part of electricity, and his conduction experiments were the beginning of the dynamical part. In light of his model and given his mechanical description of the flow of electric fluid, we might expect Cavendish to have carried the parallel further than he did, but beyond the statements of "Ohm's law" and the law of divided circuits, he did not develop the subject of electrical conduction mathematically, as he had that of charged bodies. Working with discharges and the instruments and concepts at hand, it is hard to see how he could have developed testable theoretical properties of the flow of electricity. Following his paper on the torpedo through early 1777, Cavendish continued to experiment on the conductivity of solutions. Five years later, in 1781, he returned to them, but without having arrived at a new direction, he had no reason for carrying them further. In the same year, after an absence of fifteen years, he returned to experiments in pneumatic chemistry, which would require his full attention. From then on, his only consequential electrical experiments were to detonate airs.

We will consider briefly other possible reasons why Cavendish took up experiments on conduction, starting with lightning, which had been found to be an instance of electrical discharge in nature. After his paper on electrical theory was read to the Royal Society in 1771, Cavendish was immediately recognized as an authority on electricity. The following year the government requested advice on how to protect the powder magazines at Purfleet from destruction by lightning, and the Royal Society formed a committee of its best local electricians, who included Cavendish alongside Franklin, Watson, Wilson, and Robertson. The committee recommended installing lightning conductors, Franklin's invention, at Purfleet, but there was a disagreement over the shape of the end of the conductor, whether pointed or blunt. Wilson's opinion was that blunt conductors work best, since pointed conductors invite and magnify lightning strokes, contributing to the danger rather than defending against it, sometimes resulting in violent explosions. The opinion of the majority was that pointed conductors are the most effective. In 1773 the committee, without Wilson, paid a visit to Purfleet to see if the lightning conductors were erected according to their instructions.[112] Cavendish's study of this version of the flow of electric fluid conceivably interested him in learning about the ordinary forms of electric conduction by a regular course of experiments.

Because Cavendish was not finished with lightning, we continue the account. Despite being protected by lightning conductors, Purfleet was struck by lightning in 1777, and the Board of Ordnance asked the Royal Society for help. A committee was formed of specialists on electrical instruments, Nairne, Henly, and Lane, who reaffirmed the earlier committee's recommendation for pointed lightning conductors. Wilson sent the Board a report with his contrary recommendation for blunt rods, which was referred back to the Royal Society. To consider Wilson's report, Cavendish, Priestley, Stanhope, and the president and secretaries were added to the committee, which again decided in favor of pointed conductors. Wilson

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[112] This was the second committee on lightning conductors; the first, in 1769, was without Cavendish, who had not yet published on electricity. 20 Aug. 1772, Minutes of Council, Royal Society 6:144. The committee gave a report with recommendations, 21 Aug. 1772. Cavendish's name appears first on the list of committee members, "A Report of the Committee Appointed by the Royal Society, to Consider of a Method for Securing the Powder Magazines at Purfleet," *PT* 63 (1773): 42–47. One member of the committee did not sign the report, Wilson, whose dissenting opinion follows on p. 48. He gave a fuller account: Benjamin Wilson (1773). On 14 Sep. 1773, Cavendish with three members of the committee visited Purfleet, reporting on 22 Nov. 1773, Minutes of Council, Royal Society 6:195–196.

did not quit, but about this time the issue passed from science to politics. Britain was at war with the American colonies, and the patriot Franklin was a champion of pointed conductors. King George took Wilson's side, ordering rounded conductors installed at the palace. John Pringle apparently was forced to resign his presidency of the Royal Society because of his opposition to George III's preference for rounded conductors, and he also lost his appointment as royal physician.[113]

In 1796 the Board of Ordnance again called on the Royal Society, which appointed Cavendish and Blagden to re-examine the state of the conductors at Purfleet.[114] In 1801 the Board returned with a related request of determining the proper floor covering to reduce frictional electricity at powder magazines and works, and Cavendish was appointed to a committee to look into this.[115] The electrician Cavendish was repeatedly enlisted in the defense of the nation.

Cavendish might have made experiments on conduction simply because he was curious and could spare the time. In March 1773, he completed his investigation of coated plates, bringing to a close the experiments he had promised in his paper of 1771. He could have effectively ended his electrical researches here and made the additions and changes needed to ready his book for publication, and this may have been the plan for a time. In January 1773, he carried out the experiments that completed the law of electric force by proving that the force is proportional to the product of the charges, and in April he repeated the hollow globe experiment that proved that the electric force is proportional to the inverse square of the distance. Beginning in January and extending to late summer, he made trials of Lane's electrometer and Henly's new electrometer, the latter described in the *Philosophical Transactions* the previous year, comparing them with his usual straw and pith-ball electrometers. This could be seen as tying up loose ends. However, at the conclusion of the trials of electrometers, in late 1773, Cavendish made an experiment that was unlike any up to this point, in which the electrometer was replaced by a new instrument, his body. The experiment was to compare the "strength of shocks by points and blunt bodies" by taking discharges through his body, alternately touching a terminal with a piece of brass wire with a needle fastened to the end and with a similar brass wire with a round knob at the end. To keep the shock from being too great, he gave it the "choice whether it would pass through my body or some salt water."[116] This was the first of his experiments on electric conduction through columns of solutions using his body for deciding its strength. The experiment comparing the shocks of pointed and blunt conductors coincided with his work with the Royal Society committee on Purfleet, comparing the conducting properties of pointed and blunt lightning rods.[117] Given the experiment's place in the sequence of his electrical experiments, it would seem to be

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[113] The controversy was suited for the talents of Swift, had he been around. It turns out that the shape, pointed or rounded, makes no difference, an opinion that was considered at the time, but which was overridden. Henry Lyons (1944, 193). J.S.G. Blair, "Pringle, Sir John,"*DNB* (http://www.oxforddnb.com/view/article/22805?docPos=1).

[114] 17 Mar. 1796, Minutes of Council, Royal Society 7:314. Their report was read on 23 June 1796.

[115] 11 June and 12 Nov. 1801, Minutes of Council, Royal Society 7:408–10, 414–415. The other members were Blagden, Rumford, and Hatchett.

[116] Cavendish (1879d, 292–293).

[117] Cavendish had discussed the rapid discharge of electricity from points and from the ends of long slender cylinders in his paper on electrical theory in 1771. His new experiments would seem to have been related to a question he raised in that paper, whether the electric fluid escapes faster from a small body or from an equal surface of a larger body (from a pointed or a blunt end), only now he was concerned with the shock rather than with the escape of electric fluid; he said that the answer was impossible to "determine positively from this theory." Cavendish (1771); in *Electrical Researches*, 52–56.

the start of a plan for measuring conductivities, and his work for the Royal Society might have been an imptus. For completeness, we should consider one more possible reason why Cavendish extended his electrical researches to include conduction. This was his father, who had made important experiments on electrical conduction across a vacuum and through heated glass; Cavendish extended other researches his father began, and electric conduction might have been another instance. Even if we lack the information to decide with much confidence between the possible reasons, by considering them we see that Cavendish's interest in electrical conduction is not surprising.

The Work

We close this account of Cavendish's electrical experiments with observations on the "work"[118] he intended to publish, and on the response to the part he did publish. The material on his experiments and the corresponding mathematical propositions would have made a very long paper. It occupies 104 pages of the Maxwell edition of Cavendish's electrical researches, and it would have expanded into nearly twice that number of pages in the *Philosophical Transactions*. The 1771 paper was itself long, occupying forty-nine pages in the Maxwell edition and ninety-four in the *Philosophical Transactions*, Cavendish's longest publication. It is likely that at some point he abandoned his original idea of publishing the experiments in the journal and reserved them for a book. Maxwell was certain that Cavendish was working on a book.

While Cavendish's electrical theory drew the attention of the Royal Society, it generated no evident interest among electrical researchers. The next paper on electricity to appear in the *Philosophical Transactions* after Cavendish's was about William Henly's new electrometer; Priestley, the author of the paper, said that the electrometer was capable of measuring "both the precise degree of the electrification of any body and also the exact quantity of a charge before the explosion."[119] As an accurate measurer of the two quantities that enter Cavendish's theory, Henly's electrometer was a proper instrument for investigating its experimental predictions, and Cavendish brought it into his electrical researches, but no one else thought to use it for that purpose. In 1812, the year of Simon Denis Poisson's impressive mathematical theory of electricity, Thomas Thomson wrote in his *History of the Royal Society*:

> The most rigid and satisfactory explanation of the phenomena of electricity, which has hitherto appeared in any language, is contained in a very long, but most masterly paper of Mr. Cavendish, published in the Philosophical Transactions for 1771. It is very remarkable, and to me an unaccountable circumstance, that notwithstanding the great number of treatises on electricity which have appeared since the publication of this paper, which is, beyond dispute, the most important treatise on the subject that has ever been published, no one, so far as I recollect, has ever taken the least notice of Mr. Cavendish's labours, far less given a detailed account of his theory. Whether this be owing to the mathematical dress in which Mr. Cavendish was obliged to clothe his theory, or to the popular and elementary nature of the treatises which have been published,

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[118] Henry Cavendish (1879a, 172).

[119] Joseph Priestley (1772a, 359); read 28 May 1772.

> I shall not pretend to determine; but at all events it is a thing very much to be regretted.[120]

Thomson's impression was confirmed by George Green, who came across Cavendish's "excellent paper" in a search of the literature after finishing his influential essay of 1828 on electrical potential functions, commenting that Cavendish's theory "appears to have attracted little attention."[121]

We recall that Newton urged the readers of his *Principia* to determine the forces of nature the way he had determined the law of gravitation, and to explore their experimental consequences. The next forces proved hard to work out; Newton himself tried without success. Then, without any early notice, Cavendish made public a mathematical theory of the electric force, realizing Newton's expectation. If he had belonged to a Continental scientific academy instead of to the British Royal Society, he might have had a competent audience,[122] but British electricians lacked the mathematical training to appreciate what he had done, let alone use it. The first work to have the substance of a successor to Newton's *Principia*, Cavendish's paper of 1771 was passed over almost without comment. His experimental paper on the torpedo received more notice. In the early eighteenth century, there had been a British circle of ardent admirers of Newton's mathematical philosophy, Roger Cotes, Colin Maclaurin, and others, who had not been replaced. That an excellent mathematical theory of a force of nature was for so long almost totally ignored is a comment on the decay of the mathematical tradition in late eighteenth-century Britain.

Apart from the mathematical limitations of British electrical experimenters, the likely main reason why Cavendish's theory received little attention was that he did not publish his experiments based on it. He said that he was going to, and it would have been expected; more than anyone, it was up to him to show what his theory could do. A secondary reason for the neglect is that at the time of his publication, electricity was not at the forefront of research, as it had been fifteen years before, and the same can be said of the topics that he addressed in his published paper. His "principal phaenomena" there—the attraction and repulsion of charged bodies, electric induction, Leiden jar, and electrification of air—were thought to have adequate explanations already. Priestley's *History of Electricity* contained investigations of his own on phenomena that were not well understood, and the queries in that book suggested the kind of problems that interested Cavendish's contemporaries, these having mainly to do with connections between electricity and light, sound, heat, and chemistry. Typical of a direction of thought at the time was Henly's belief that electricity, light, fire, and phlogiston were "only different modifications of one and the same principle."[123] Although Cavendish's natural philosophy could accommodate connections between electricity and other fields, his work was not directed to them.

The reasons why Cavendish did not publish his electrical experiments are unknown. What had begun as a second paper for the *Philosophical Transactions* became the second part of a book on electricity. He completed several series of electrical experiments to his satisfaction, but he may not have been satisfied with the book. If his idea of the book was to

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[120] Thomas Thomson (1812, 455).

[121] George Green (1828, v).

[122] Thomas S. Kuhn's comparison of the classical mathematical sciences and the Baconian experimental sciences would suggest that had Cavendish been born a European instead of an Englishman, he would have found knowledgeable colleagues in an academy of sciences for his mathematical theory of electricity (1977, 58).

[123] William Henly (1777, 135).

present a theory of electricity, and not just of a part of electricity, it had to include conduction, and just how his experiments on conduction relate to the theory is unclear. His explanation of the effect of glass on the capacity of Leiden jars was speculative, but there at least he had a theory with which to compare the experiments. Lacking a comparable theory of conduction, he had no reason to try to explain the effect of substances on the resistance to the flow of the electric fluid.

Cavendish began his electrical researches around the time of his initial publication on factitious air, which earned him a Copley Medal. After the publication of his electrical theory in 1771, he never again published a theoretical paper. It would be ten years after he had given up the plan of publishing his electrical experiments before he appeared in print again with original research. When he did, it was with the approach and subject of his original success, the experimental study of airs.

Chapter 10
Learned Organizations

Royal Society

At the time Cavendish entered the Royal Society, its membership was stable, as it had not been before and would not be after. During the twenty years centering on 1760, the average number of ordinary members was practically constant, around 350, whereas it had grown by nearly one quarter in the thirty years after Cavendish's father had joined. The foreign membership was now at its maximum, around 160, forty percent larger than it had been thirty years before; thereafter it slowly declined owing to a deliberate policy of the Society to stop the escalation of the honorary segment of its membership.[1]

Beginning in 1753, candidates for membership had to be known "personally" to their recommenders. Throughout his fifty years in the Society, Cavendish recommended a new member every year or two, somewhat over thirty all told. The first time he signed a certificate proposing a new member, he did so with his father, whose name appears first; that was the only time the two made a recommendation together, his father naming only four more recommendations. Four of the first five candidates Cavendish recommended were Cambridge men, and because he knew them "personally," he probably had met them in Cambridge. The first was Anthony Shepherd, recently appointed Plumian Professor of Astronomy and Experimental Philosophy at Cambridge. Shepherd was ten years older than Cavendish, but the other three had been fellow students: the mathematician and barrister Francis Maseres, the astronomer and cleric Francis Wollaston, and the antiquarian and diplomat John Strange. In the cases of Maseres and Wollaston, Cavendish was first to sign their certificates. At this time, persons Cavendish wanted in the Society were associated with the physical sciences, with exceptions. John Strange was a member of foreign botanical societies and John Cuthbert, the one candidate Cavendish recommended who was not from Cambridge, was an attorney, whose certificate read, "well versed in polite Literature."[2]

A further indication of the continuity of his years in Cambridge is the list of guests he brought to the Royal Society Club. Starting in 1766, six years after he became a member, the Club identified guests with the persons who brought them. We see that Cavendish's first five guests after that year were Cambridge men, all either about to leave Cambridge or had already left. William Ludlam was a little older than Shepherd and then a fellow of St. John's College, but soon to vacate his fellowship to accept a rectory. He published a book of astronomical observations made at St. John's in 1767–68, including an account of several astronomical instruments and calculations made for him by Charles Cavendish; both Henry

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[1] 19 Dec. 1765, 6 Feb. 1766, Minutes of Council, Royal Society (UPA film ed.) 5:146–148, 153–154. It was resolved that no more than two foreigners a year would be admitted until their number fell to eighty.

[2] Certificates, Royal Society. Dates of proposal: Anthony Shepherd, 2:242 (19 Jan. 1763); John Strange, 2:343 (early Jan. 1766) ; Francis Wollaston, 3:65 (3 Jan. 1769); Francis Maseres, 3:104 (31 Jan. 1771); John Cuthbert, 3:312 (7 Mar. 1765).

and Charles invited Ludlam to the Club as their guest in 1767.[3] Another guest of Henry's was John Michell, formerly the Woodwardian Professor of Geology at Cambridge, who in the year Henry invited him to the Club, 1767, became rector of Thornhill in Yorkshire. Henry's three other guests were his age and had been at Cambridge when he was: John Strange again, Henry Boult Cay, a fellow of Clare College, Cambridge, who was soon to vacate his fellowship to practice as a barrister of the Middle Temple,[4] and Wilkinson Blanchard, a fellow of the College of Physicians and a physician to St. George's Hospital in London. Cavendish also brought guests to meetings of the Royal Society, and again there was a Cambridge connection: around this time, in 1767 and 1768 he invited Francis Wollaston, and in 1769 he invited Ludlam.[5]

For further information about Cavendish's associations we return to the book of certificates recommending candidates for fellowship in the Royal Society. His recommendations reflected his current scientific activities. After his first recommendations of candidates from Cambridge mentioned above, his next, in 1769, was of Timothy Lane, who was then working in electricity and chemistry, the same as Cavendish. Cavendish's first foreign candidate was the electrical researcher Jean-Baptiste Le Roy in 1772, the year after Cavendish's published his electrical theory.[6] In the mid-1780s Cavendish undertook several tours of Britain, making industrial and geological observations and investigating specimens from furnaces and minerals from the Earth. The candidates he recommended then included James Watt, who is identified on the certificate as the inventor of the new steam engine and the author of a paper on chemistry; James Keir, a former glass and now alkali manufacturer, who is identified as the author of a paper on the crystallization of glass and the editor of a dictionary of chemistry; and James Lewis Macie (James Smithson) and Philip Rashleigh, both identified with chemistry and mineralogy. Cavendish's recommendation of the foreign geologist Horace Bénédict de Saussure belongs to this group too.[7] In the late 1780s, when Cavendish's chemical publications came to an end and he abandoned the phlogiston theory of chemistry, he welcomed into the Royal Society as foreign members the leaders of the new anti-phlogistic chemistry: its inventor Antoine Laurent Lavoisier, and his colleagues L.B. Guyton de Morveau and Claude Louis Berthollet. In the same period, when Cavendish brought together his wide-ranging experimental and theoretical work in heat, he recommended the Swedish master of the subject of heat, Johan Carl Wilcke.[8] In 1789 Cavendish recommended Pierre Simon de Laplace for his work in mathematics and astronomy, and every foreign member after that, with one possible exception, ten all told, were likewise known for their work in mathematics and astronomy, the fields that Cavendish was then pursuing. This sizable foreign group consisted of Joseph Louis Lagrange, Jean-Baptiste Joseph Delambre, Joseph Mendoza y Rios, Gregorio Fontana, David Rittenhouse, J.H. Schroeter,

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[3] 14 May 1767, Minute Book of the Royal Society Club, Royal Society, 5. William Ludlam (1769). "Ludlam, William," *DNB*, 1st ed. 12:254–255.

[4] 14 May 1767, 30 June 1768, and 16 Feb. 1769, Minute Book of the Royal Society Club, Royal Society, 5. Archibald Geikie (1917, 91, 100).

[5] 26 Feb. 1767, 8 Dec. 1768, and 9 Feb. 1769, JB, Royal Society 26.

[6] Certificates, Royal Society. Proposed: Timothy Lane, 3:73 (6 May 1769); Jean-Baptiste Le Roy, 3:161 (5 Sep. 1772).

[7] Certificates, Royal Society. Elected: James Watt, 5 (24 Nov. 1785); James Keir, 5 (8 Dec. 1785); James Lewis Macie (James Smithson), 5 (19 Apr. 1787); H.B. de Saussure, 5 (3 Apr. 1788); Philip Rashleigh, 5 (29 May 1788).

[8] 30 Apr. 1789, Certificates, Royal Society 5.

Joseph Piazzi, Franz Xaver von Zach, W. Obers, and Carl Friedrich Gauss.[9] We postpone to the end of this section our discussion of a large group of world travelers recommended by Cavendish.

Of the almost one hundred fellows of the Royal Society who joined Cavendish in recommending candidates, only a few appear with him on more than one certificate. Nevil Maskelyne appears on half of the certificates, and after him, in decreasing frequency, come the keeper of the natural history department of the British Museum Daniel Solander, William Watson, James Burrow, and William Heberden. Several of these persons were cosigners with Cavendish's father. From this record, we might conclude that he was not part of a faction. His frequent appearance with Maskelyne reflects their common Cambridge education, with its emphasis on mathematics, and their common interest in the physical sciences, especially astronomy.

In 1765 Cavendish was elected to the Council of the Royal Society,[10] the first of thirty-four times. We get some idea of what this involved from the frequency of Council meetings and the record of his attendance. Over the first twenty years after he became a member of the Royal Society, 1761 to 1780, the average number of Council meetings per year was seventeen, the number falling to eleven or twelve over the next twenty years, 1781 to 1800. The four officers of the Society—president, treasurer, and two secretaries—came to most of the Council meetings, but on the average fewer than seven of the twenty-one members attended. In his first year on the Council, other than for the two secretaries, Cavendish attended with greater regularity than anyone, and this became his pattern; like his father, when he was on the Council, he rarely missed a meeting. After his first term on the Council, for the next twenty years he was on it about half the time. A historian of the Royal Society lists the longest-serving members of the Council over a period of forty-two years, beginning twelve years after Cavendish first served. In the first half of the period, 1778–1800, a total of 171 members of the Society were elected to the Council; the great majority, eighty-eight percent, were elected for only one or two years; nine served three years; five served four or five years; and only four served more than ten years:[11]

W. Musgrave	22 years
N. Maskelyne	20 years
H. Cavendish	17 years
Lord Mulgrave	14 years

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[9] Royal Society, Certificates. Elected: Pierre Simon de Laplace, 5 (30 Apr. 1789); Joseph Louis Lagrange, 5 (5 May 1791); Joseph Delambre, 5 (5 May 1791); Joseph Mendoza y Rios, 5 (11 Apr. 1793): Gregorio Fontana, 5 (10 July 1794); David Rittenhouse, 5 (6 Nov. 1794); J.H. Schroeter, 5 (19 Apr. 1798); Joseph Piazzi, 6 (11 Apr. 1803). Proposed: Franz Xaver von Zach, 6 (17 Nov. 1803); W. Obers, 6 (17 Nov. 1803); Carl Friedrich Gauss, 6 (17 Nov. 1803).

[10] 30 Nov. 1765, JB, Royal Society 25:663.

[11] Lyons (1944, 197–204).

For 1801–1820, the years are:

C. Blagden	19 years
Lord Morton	18 years
N. Maskelyne	11years
H. Cavendish	10 years

Cavendish died in 1810, halfway through the second span. If we look at the last twenty-five full years of Cavendish's life, 1785–1809, we find that Cavendish's record was unsurpassed: he served on the Council every one of those years.[12]

We have an idea of the scientific company Cavendish kept on the Council: it is estimated that the average number of scientifically active members on the Council over the twenty years 1761–1781 was between nine and ten, and over the next twenty years 1781 to 1800 it was under seven. Because the activities of the Royal Society constituted a substantial part of Cavendish's working life, we should have an idea of what that work consisted of. We begin with his first year on the Council, dating from the end of 1765, when he took his oath along with other new members. The year's activity started with a courtesy related to the "Royal" in the name Royal Society, a gift to the king of bound volumes of the *Philosophical Transactions* for the last fifteen years. Through Cavendish's first year, the journal came to the attention of the Council in a number of ways: rules for authors' corrections of their papers, sales of the journal, payment for stocking future volumes, payment to printers, engravers, and stationers, and orders of copies of the journal printed that year. The membership of the Society came up in Council meetings. Through several resolutions, the Council in addition to limiting the number of new foreign members specified the procedure of their nomination and the conditions of their election, exempting from restrictions sovereign princes and their sons, ambassadors, foreigners living in England, and presidents of foreign academies of sciences. While revising the Society's practice of admitting foreigners, the Council ordered 1000 copies of its charter for distribution to the members. Other business included salaries paid to the two secretaries, the assistant secretary for foreign correspondence and translation, and the clerk. Bills were ordered to be paid by the treasurer for sundry purposes, principally to instrument makers, in particular for the instruments acquired to confirm John Canton's experimental proof of the compressibility of water. In addition to statutes, membership, journal, and bills and revenue, the Council took up a range of scientific matters. That spring Cavendish reported on his first project for the Society, a determination of the best method of fixing the boiling point on thermometer scales. The summer of 1766 saw the beginning of the Society's long preoccupation with the transit of Venus in 1769. In a letter to the president, Cavendish brought before the Council his recommendations of proper places in the world for observating the transit of Venus. The Council resolved that one or more astronomical observers be selected and that Roger Joseph Boscovich, a foreign member of the Royal Society and professor of mathematics at Pavia, be approached. For several years, the Society's Copley Medals for the best research in a given year had not been disposed, though there had been good papers during that time. To make up for this, the president proposed that three medals be given that year, one of which went to Cavendish

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[12] From a survey of the Minutes of Council, Royal Society 5–8.

for his first paper. The final item of business that year was an audit of the Society's income and expenses, the treasurer's account. Cavendish was named one of the auditors along with James Burrow and George Lewis Scott; when they were finished, Cavendish reported to the Council in the name of the three auditors, a degree of prominence he could accept. The balances were small, but that did not diminish the responsibility of the auditors; Cavendish was joined on the committee of auditors in subsequent years by Maskelyne, Franklin, and other stalwart members. Five years after his election, Cavendish was clearly an important member of the Society. Moreover, by demonstrating his knowledge and skill in astronomy, instruments, heat, and chemistry, he was recognized as a natural philosopher of broad competence, a valuable asset in the Society's wide-ranging activities.[13]

We look ahead. Cavendish was extensively engaged in two major projects initiated by the Society during his time, the one just mentioned, observations of the transit of Venus in 1769, the second an experiment on the attraction of mountains in 1774. He drew up plans for a voyage of discovery to the Arctic; he worked on changes in the statutes of the Society and in the printing of the *Philosophical Transactions*; he was appointed to committees concerned with meteorological instruments of the Royal Society and astronomical instruments of the Royal Observatory; and he served on committees called into being by requests of the government. He was appointed to twenty-three committees, more or less,[14] and he took on assignments for the Society that did not involve a committee but at most an instrument maker to work with him. Altogether, Cavendish worked with about sixty fellows on special committees. Since the work of the Society was spread around, usually other fellows appeared on only one committee with him, the exceptions being Maskelyne, the astronomer royal, and the astronomer Aubert, who was an expert on meteorological it as well as astronomical instruments.[15]

Like his father, Cavendish served regularly on the committee of papers,[16] which attracted able men regardless of their habits of publication; some of them, such as Maskelyne and William Herschel and Cavendish himself, were themselves authors of many papers in the *Philosophical Transactions*, but others, such as Aubert, published next to nothing. In addition to attending the meetings of the committee, which took place monthly as needed, the members had homework. On any particular paper, the committee could make one of several decisions: to print, not to print, to withdraw, or to postpone. If postponed, the paper might be referred to one or two members. In the case of strong disagreement over a given paper, the matter could be taken up by the Council of the Society. That was done in 1789: Cavendish gave the Council his reasons why a paper that the committee had ordered printed in the *Philosophical Transactions* should not be printed; the Council then recommended to the committee that it "reconsider their former vote on the subject of the said paper."[17]

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[13] Entries from 19 Dec. 1765 to 30 Nov. 1766: Minutes of Council, Royal Society 5: 143, 145–153, 155–158, 160–161, 163–164, 167, 169. Henry Cavendish (1921a). Henry Cavendish to James Douglas, earl of Morton, [9 June 1766], draft, in Jungnickel and McCormmach (1999, 531–533). Cavendish would later be appointed to a committee of eight to consider places for observing the transit. 12 Nov. 1767, Minutes of Council, Royal Society 5: 184.

[14] It depends on how one counts. Committees were often renewed, sometimes becoming virtually new committees with the same or a redefined task.

[15] Cavendish served on eight committees with Maskelyne and as many with Aubert.

[16] From a survey of the bound volume of minutes of the Royal Society's committee of papers, 1 (1780–1828).

[17] The paper proposed a new and easy method for determining the difference of longitude. 19 Feb. 1789, Minutes of Council, Royal Society 7:201.

Following Cavendish's report in 1766, the Council undertook painstaking preparations for observing the transit of Venus in 1769, which was the second of these rare, paired crossings of the Sun's disk, affording an accurate measure of the Sun's distance (Fig. 10.1). The Society had completed its work on the 1761 transit, with rather disappointing results. At the time, the secretary of the Society Thomas Birch wrote to Philip Yorke that the observations of the transit "differ so considerably from each other [...] that it is question'd whether the Credit of the Conclusions to be drawn from them will not be much weaken'd: and I am apprehensive that our Astronomers, if not Astronomy itself, will suffer a little in Reputation." Pride as well as science called for a repetition of the measurement. Having learned from their errors in 1761, astronomers planned their observations for 1769 with meticulous care.[18] Charles Cavendish, as we have seen, did considerable work on the first transit of Venus; beginning to end, Henry Cavendish did the same on the second.

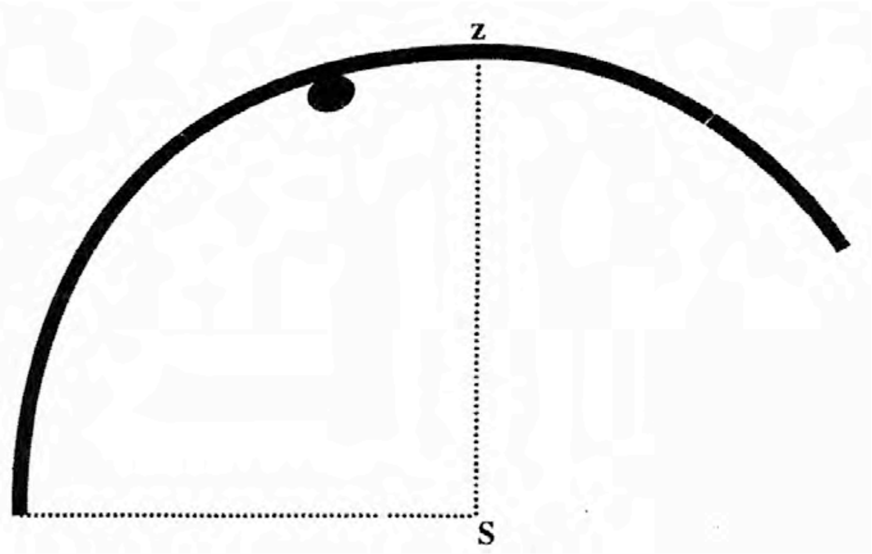

Figure 10.1: Transit of Venus. On the Island of Maggeroe, on the North Cape of Europe, the transit of Venus of 1769 was observed by William Bailey, who was sent there by the Royal Society. The event was partly obscured by clouds but not completely, as shown by his drawing (which has been redrawn for this book). "Astronomical Observations Made at the North Cape, for the Royal Society," *Philosophical Transactions* 59 (1769): 262–66, on 266.

Cavendish studied the observations of the earlier transit of Venus of 1761 at a time when he was carrying out chemical experiments on air. There was a connection of sorts. During the first transit, the effect of the air of Venus was not considered, with the result that the reported times of contact of Venus and the Sun were discordant.[19] By making different assumptions about the elastic fluid constituting the atmosphere of Venus, Cavendish com-

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[18] Thomas Birch to Philip Yorke, 13 June 1761, BL Add Mss 35399, f. 202.

[19] H. Spencer-Jones (1948, 16).

puted the errors of observation owing to the refraction of light passing through it from the Sun to the observers on Earth.[20] Before Cavendish was done with his work on the transit of Venus of 1769, he had written over 150 pages.[21] As it turned out, the observations of the second transit did not result in an unambiguous figure for the distance of the Sun, but the accuracy of the estimate was markedly improved, and the project could be counted as a respectable achievement of measuring science.

In a letter in 1771 from Maskelyne to Cavendish, we first hear of Cavendish's participation in the other major scientific project of the Royal Society in the second half of the eighteenth century, the experiment on the attraction of mountains to determine the average density of the Earth.[22]

While at St. Helena to observe the transit of Venus in 1761, Maskelyne made an experiment with a pendulum clock to compare the force of gravity there with that at the observatory at Greenwich. He drew no conclusions from the comparison about the figure of the Earth or the law of change in the force of gravity with latitude, since there was reason to think that the Earth is not homogeneous, in which case the force of gravity depends not only on the external figure of the Earth but also on its internal constitution and density. In a paper written as a letter to Charles Cavendish, Maskelyne said that other kinds of experiments than those with pendulum clocks would have to be made to "be able to infer any thing with certainty, concerning the internal constitution of the Earth, or even to determine its external figure."[23] For the same reason, Henry Cavendish told Maskelyne that the attraction of a mountain was preferable to a pendulum clock for determining the average density of the Earth, being less affected by any inhomogeneity.[24] A few years earlier, Cavendish had been concerned with the deviation of a plumb line by the attraction of mountains in connection with errors in measuring degrees of latitude, and he calculated errors for a number of hilly places around the world, including the Allegheny Mountains.[25] He gave his paper on rules for computing such errors to Maskelyne who made use of it in a publication in 1768 about Charles Mason and Jeremiah Dixon's determination of the length of the degree of latitude in Pennsylvania and Maryland. Cavendish was thoroughly familiar with calculations of the attraction of mountains.[26]

The Royal Society's experiment had a history. In 1738 French observers measured the deflection of a plumb line on a mountain in South America. They made use of two stations, one on one side of the mountain, and one on the other side several miles away on the same latitude, sufficiently removed from the gravity of the mountain. The same star was viewed from both stations, directly overhead as determined by a plumb line at the distant station

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[20] Henry Cavendish, "On the Effects Which Will Be Produced in the Transit of Venus by an Atmosphere Surrounding the Body of Venus," Cavendish Mss VIII, 27.

[21] In addition to "Thoughts on the Proper Places for Observing the Transit of Venus in 1769," letter to Morton, and "On the Effects […] by an Atmosphere," Cavendish wrote these studies: "Computation of Transit of Venus 1761, 1769," "Method of Finding in What Year a Transit of Venus Will Happen," "Computation of Transit of 1769 Correct," and "Computation for 1769 Transit," Cavendish Mss VIII, 30–33.

[22] Nevil Maskelyne to Henry Cavendish, 10 Apr. 1771; in Jungnickel and McCormmach (1999, 535). The discussion of the attraction of mountains is based on Russell McCormmach (1995).

[23] Nevil Maskelyne (1762, 442).

[24] Henry Cavendish, "Paper Given to Maskelyne Relating to Attraction & Form of Earth," Cavendish Mss VI(b), 1:20.

[25] Henry Cavendish, "Rules for Computing the Error Caused in Measuring Degrees of Latitude by the Attraction of Hilly Countries," Cavendish Mss XI, Misc.

[26] Henry Cavendish, "Attraction of a Solid on a Point in Its Surface," Cavendish Mss VI(b), 11.

and forming a small angle with a plumb line at the other station owing to the attraction of the mountain. The measurements were found to be inexact, and the French hoped that other observers would succeed on a better mountain.

In the middle of 1772, Maskelyne proposed to the Royal Society the mountain experiment that he and Cavendish had discussed. The Council appointed a committee with Maskelyne and Cavendish on it to prepare the experiment and to call on the treasurer as needed.[27] Cavendish worked out rules for the attraction of mountains, which Maskelyne found "well calculated to procure us the information that is wanted."[28] In a paper he wrote for Franklin, who was on the committee, Cavendish explained that the meridian altitudes of stars were to be observed at both the north and the south feet of a mountain capable of exerting a sensible attraction, giving the relative inclinations from the vertical of the plumb line at those two locations. The chief criterion for the choice of a mountain was that the relative inclinations should be as great as possible, for which purpose the want of attraction of a deep valley was as good as the attraction of a mountain and perhaps better.[29] (Fig. 10.2). As the one who did the extensive planning, Cavendish reported to the Council on the committee's resolutions in the middle of 1773.[30] The surveyor and astronomer Charles Mason was directed by the Council to ride horseback into the Scottish Highlands to survey mountains suitable for the experiment, and on his return to survey further mountains on the borders of Yorkshire and Lancashire. In early 1774, the committee decided on Schehallien (the usual spelling of the time), a 3547–foot mountain in Perthshire in Scotland[31] made to Cavendish's order: big, regular, detached, with a narrow base in the north-south direction (Fig. 10.3). The committee selected Maskelyne to make the experiment. His Greenwich assistant Reuben Burrow and a local surveyor determined the size and shape of the mountain, while Maskelyne observed forty-three stars from it.

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[27] Nevil Maskelyne (1775a); read in 1772. 23 July 1772, Minutes of Council, Royal Society 6:145.

[28] Nevil Maskelyne to Henry Cavendish, 5 Jan. 1773; in Jungnickel and McCormmach (1999, 538). Having made a copy, Maskelyne returned Cavendish's "Rules for Computing the Attraction of Hills." The preliminary version of that paper is Henry Cavendish, "Thoughts on the Method of Finding the Density of the Earth by Observing the Attraction of Hills," Cavendish Mss VI(b), 2, 6.

[29] The Royal Society's experiment would differ from the French one in that the two stations were both on the mountain, one on the north side and one on the south side. Henry Cavendish, "On the Choice of Hills Proper for Observing Attraction Given to Dr Franklin," Cavendish Mss VI(b), 3:1, 5. Among his manuscripts are extensive calculations of the attraction of conical hills with circular and elliptical bases. Other manuscripts treat specific mountains in Scotland, candidates for the experiment: Skidda, supposed to be a cone with a circular base, Maidens Pap, Ben Laas, and others. Cavendish Mss XI, Misc.

[30] 29 July 1773, Minutes of Council, Royal Society 6:185–186. The committee which met on 18 July to approve the resolution consisted of Cavendish, Barrington, Horsley, Maskelyne, Watson, and the secretaries Maty and Morton.

[31] 27 Jan. 1774, Minutes of Council, Royal Society 6:210–211. The spelling varied. In this entry of the Minutes, the mountain is written "Sheehalian Maidens Pap."

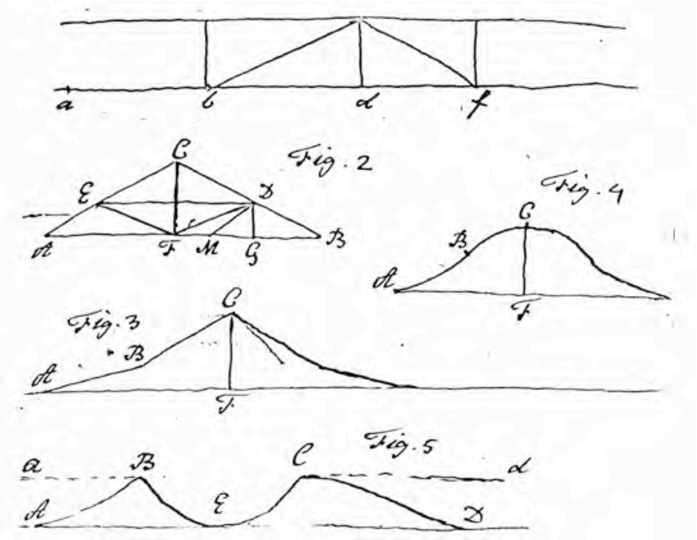

Figure 10.2: Cavendish's Drawings of Mountains. For the experiment by the Royal Society to measure the gravitational attraction of a mountain as a means for determining the average density of the Earth, Cavendish drew up rules for selecting the mountain for the purpose. He considered a number of shapes. "Mr.Cavendish's Rules for Computing the Attraction of Mountains on Plumblines," Cavendish Scientific Manuscripts VI (b), 2. Courtesy of the Chatsworth Settlement Trustees.

Figure 10.3: Schehallien. Photograph of the mountain showing its advantageous geometry for determining the average density of the Earth. Wikimedia Commons.

The meridian distance between the north and south observation stations was measured too in order that the change in the zenith distances of the stars owing to the slightly different latitudes of the two stations could be corrected for. The main instruments for measuring were a theodolite and rods. The main instrument for taking observations was a zenith sector, a telescope designed to observe stars directly overhead, as determined by a plumb line. When the experiment was done, Cavendish and C.J. Phipps went over Burrow's scarcely legible papers from the field.[32] The mean sum of the gravitational attractions on the two sides of the mountain produced an angle of 11.6 seconds, small but large enough to work with.

On the basis of the experiment and Newton's "rules of philosophizing," Maskelyne told the Royal Society in July 1775 that "we are to conclude, that every mountain, and indeed every particle of the Earth, is imbued with the same property [attraction], in proportion to its quantity of matter," and further that the "law of the variation of this force, in the inverse ratio of the squares of the distances, as laid down by Sir Isaac Newton, is also confirmed."[33] For this work, Maskelyne was awarded the Copley Medal in 1775. In his address on the occasion, the president of the Society John Pringle said that the Newtonian system was "finished" and that every man now must become a Newtonian.[34] The quantity the experiment addressed, the mean density of the Earth, had to wait for the calculations of the mathematician Charles Hutton, who had been hired by Maskelyne for the task. In 1778 Hutton finished his paper, some hundred pages of "long and tedious" figuring, arriving at a value for the attraction of the mountain in the north-south direction. To explain why it took him so long, he said that in dividing the mountain into manageable sections and assigning elevations, several thousand triangles had to be calculated, and to find the attraction of the mountain on the plumb line the attractions of around 2000 small parts of the mountain, contained within concentric circles and progressive radii centered on each of the two observational stations, had to be calculated, requiring many hundreds of long divisions in constructing the necessary trigonometric sines. The calculations were an enormous labor, which would have been even far greater if in both cases Cavendish had not proposed laborsaving methods, which Hutton acknowledged. The ratio of the attraction of the Earth to the attraction of the mountain, the quantity sought, was computed two ways. One was a theoretical calculation based on Hutton's configuration of the mountain: by assuming that the density of the mountain is the same as the density of the Earth, the ratio came out to be 9933 to 1. The other was by using Maskelyne's observations of the plumb line, which after making allowance for the centrifugal force of the rotating Earth came out to be 17,804 to 1. The quotient, 17,804 to 9933, approximately 9 to 5, is the quantity by which the mean density of the Earth exceeds that of the mountain. Hutton pointed out that the density of the mountain was unknown, but by assuming that the mountain is "common stone," the density of which is $2\frac{1}{2}$, he deduced that the "mean density of the whole earth is about $4\frac{1}{2}$ the density of water." Newton's best guess was that the density of the Earth is between 5 and 6 ("so much justice was even in the surmises of this wonderful man!"). Reminding his readers that this experiment was the first of its kind, Hutton hoped that it would be repeated in other places.[35]

The experiment on the mountain and the Society's recent concern with the transit of Venus had a common goal: the distance of the Earth from the Sun and the density of the

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[32] 6 and 27 Apr. 1775, Minutes of Council, Royal Society 6:267–269.

[33] Nevil Maskelyne (1775b, 532).

[34] John Pringle (1775a); the remark on the Newtonian system comes at the end of the discourse.

[35] Hutton (1778, 689–690, 717, 749–750, 766, 781–783, 785).

Earth were both standard measures of the solar system. At the end of his paper, Hutton related his calculation to the a physical measure of the Sun, Moon, and planets:

> Knowing then the mean density of the Earth in comparison with water, and the densities of all the planets relatively to the Earth, we can now assign the proportions of the densities of all of them as compared to water, after the manner of a common table of specific gravities. And the numbers expressing their relative densities, in respect of water, will be as below, supposing the densities of the planets, as compared to each other, to be as laid down in Mr. de la Lande's astronomy.

Water	1
The Sun	$1\frac{2}{15}$
Mercury	$9\frac{1}{6}$
Venus	$5\frac{11}{15}$
The Earth	$4\frac{1}{2}$
Mars	$3\frac{2}{7}$
The Moon	$3\frac{1}{11}$
Jupiter	$1\frac{1}{14}$
Saturn	$\frac{13}{32}$

Table 10.1: Densities of the Solar System

> Thus then we have brought to a conclusion the computation of this important experiment, and, it is hoped, with no inconsiderable degree of accuracy.[36]

There is a legend that Maskelyne threw a bacchanalian feast for the inhabitants of the region near Schehallien.[37] It is hard to picture the proper Maskelyne taking part in this affair and impossible to picture Cavendish, but Cavendish was not there. Just as he did not travel to observe the transit of Venus, he did not go to Scotland to observe stars from a mountain; he planned the experiment from his study on Great Marlborough Street in London.

A related activity of the Royal Society from which Cavendish likewise stayed home was voyages of discovery, although again he took part in the scientific preparation for them. The world was still incompletely explored by Europeans. In the wake of James Cook's southern voyages, the Royal Society proposed, and the king agreed to, a voyage to the far north, the primary object of which was to settle the practical question of the existence of a shorter route to the East Indies across the North Pole, the hopefully named Northwest Passage. The Society anticipated that such a voyage would also be of service in the "promotion of natural knowledge," the "proper object" of the Society.[38] C.J. Phipps was put in com-

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[36] Charles Hutton (1778, 784). B.E. Clotfelter (1987, 211). A second goal of the mountain experiment was to learn about the composition of the interior of the Earth. From the results of the experiment, Hutton concluded that the interior contains great quantities of heavy metals.

[37] Derek Howse (1989, 137–138).

[38] After Daines Barrington, F.R.S., had spoken with the secretary Lord Sandwich, the Council of the Royal Society ordered the secretary of the Society to write to him proposing a northern voyage with practical and scientific ends. 19 Jan. 1773, Minutes of Council, Royal Society 6:160–161.

mand of two frigates, joined by the astronomer Israel Lyons. On this voyage no opportunity for advancing the Royal Society's knowledge of the Earth was overlooked. The president of the Royal Society Joseph Banks provided Phipps with instructions on how to draw up an account of the natural history of the North. From the side of the physical sciences, Phipps and his crew were given multiple assignments, which they carried out while passing through perilous waters, observing a variety of natural objects: refraction of light, height of mountains using a barometer and a theodolite, icebergs, specific gravity of ice, magnetic variation and dip, temperature, pressure, and humidity of the air, and acceleration of a pendulum. They surveyed coasts using a megameter, distilled seawater, compared timekeepers, and made astronomical observations. To improve the art of navigation the Board of Longitude provided Phipps with instruments for experiments. As a member of the Royal Society's committee for this voyage, Cavendish drafted instructions for the use of his father's self-registering thermometer for taking the temperature of the sea at various depths, working out the corrections required to bring the accuracy of the thermometer up to date.[39] Phipps's expedition was a traveling observatory and laboratory of the Earth, or as Cavendish might have pictured it, the Royal Society under sail.

Around the time of Phipps's journey, there was keen interest in the Royal Society in the far north. During the transit of Venus in 1769, the Society sent the astronomer William Bayley to the northernmost projection of Norway, the North Cape. The Society also sent the astronomer and mathematician William Wales, who was a sailing companion of Bayley's, and the astronomer Joseph Dymond to Hudson's Bay in Canada, where they made meteorological as well as astronomical observations of the transit. In 1776, Cook made a journey north, with Bayley aboard, carrying Cavendish's instructions. The same year Richard Pickersgill made a northern journey also carrying Cavendish's instructions.[40]

Beginning in 1773, if not earlier, Cavendish incorporated the Hudson's Bay Company into his network of sources, its northern remoteness affording an opportunity to study nature in a frozen state. In December of that year, as an acknowledgment of its "considerable and repeated benefaction's," the Council of the Royal Society moved to send the Company a collection of meteorological instruments with instructions for its officers to measure the weather and report back to the Society, the secretary of the Society Maty to serve as intermediary.[41] Three days after the motion, Maty wrote to Cavendish to acknowledge his "hints" about observations to be made at Hudson's Bay, and to ask him where the instruments were to be placed in that climate. Because the rain gauge, in particular, could only be used in summer, Maskelyne had proposed that snow be collected on the frozen river, and Maty wanted to know what Cavendish thought about the suggestion.[42] Ten years later Cavendish would carry out researches on the mercury thermometer and on freezing solutions with the aid of personnel of the Hudson's Bay Company. The great trading companies together with

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[39] 22 and 29 Apr. 1773, Minutes of Council, Royal Society 6:172–173. The instructions for Phipps's voyage were drawn up by Cavendish, Maskelyne, Horsley, Montaine, and Maty. Charles Richard Weld (1848, 2:72). Henry Cavendish, "Rules for Therm. for Heat of Sea," twenty-four numbered pages with many crossings-out, Cavendish Mss III(a), 7. "To Make the Same Observations on the Flat Ice or Fields of Ice as It Has Been Called," part of a ten-page manuscript, ibid., Misc. There is a second draft of the instructions about ice fields among Cavendish's journals, ibid., X(a). Cavendish's instructions for the use of his father's thermometer are quoted in Constantine John Phipps (1774, 27, 32–33, 142, 145).

[40] Cavendish Mss IX, 41, 43.

[41] 23 Dec. 1773 and 20 Jan. 1774, Minutes of Council, Royal Society 6:205, 208.

[42] Matthew Maty to Henry Cavendish, 26 Dec. 1773; in Jungnickel and McCormmach (1999, 541–542).

the admiralty were, in effect, a part of the method of science in eighteenth-century Britain. Cavendish received observations from voyages around the world.[43]

One of Cavendish's close colleagues in the Royal Society was a professional voyager, the first hydrographer for the East India Company and later the first hydrographer for the admiralty, Alexander Dalrymple (Fig. 12.2). A man of great energy and versatility, Dalrymple was an explorer, chart maker, navigator, surveyor, commander, geographer, author of the first English book on nautical surveying, and the moving spirit behind the "second British Empire." His hypotheses inspired major voyages of discovery to test them. Thoroughly scientific in his approach to oceanic exploration, he had a keen interest in scientific instruments, especially chronometers. He was stubborn, difficult to work with, short tempered, and he has said of himself, "humour was not his talent!" It is unlikely that Cavendish was distracted by these traits, appreciating Dalrymple's insistence on "accurate precision," his constant "investigation of Hydrographic Truth, amidst the variety of discordant authorities," and his "unstinting [...] loyalty towards those who have earned his confidence."[44] Warmly greeted by Dalrymple in letters, Cavendish named him a trustee of his property, left him a legacy in his will, and repeatedly lent him money.[45] Cavendish no doubt thought he was amply rewarded in the news of the world that Dalrymple regularly brought him.

Voyagers held a special interest for Cavendish, who invited them as his guest at the Royal Society and the Royal Society Club. In the certificates book of the Society we find that he recommended at least ten men who were known for their wide travels as well as their learning. One of them was the ship's captain James Horsburgh, who like Dalrymple would be appointed hydrographer to the East India Company. Dalrymple met Horsburgh in London, where he introduced him to Cavendish and other men of science in 1801. From Bombay in 1805, Horsburgh sent Cavendish a paper on meteorological readings to communicate to the Royal Society. That year Dalrymple asked Cavendish, Maskelyne, and Aubert to join him in recommending Horsburgh as a fellow, as they did, only Aubert did not sign the certificate since he died that year.[46] Other world travelers recommended by Cavendish included Josias Dupré, who as secretary for the East India Company at Fort St. George at Madras had appointed Dalrymple as his deputy, preparing the way for the latter's career; Robert Barker, a Member of Parliament, who formerly was in the service of the East India Company as "Commander in Chief in Bengal, being curious in natural History";[47] Samuel Davis, "of Bhagalpur in the East Indies," who as a civil servant in Benares was an active member of the Asiatic Society, publishing in its journal on the "astronomical computation of the Hindus"; James Cook, who was the "successful conductor of two important voyages for the discovery of unknown countries by which geography & natural history have been greatly advantaged & improved"; James King, who was "Captain in the Royal Navy, lately

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[43] Robert Barker (1775). Alexander Dalrymple (1778). Dalrymple took observations with thermometers, barometers, and a dipping needle, and in his report he gave a long extract on the latter instrument by Cavendish (390).

[44] W.A. Spray (1970, 200–201). Howard T. Fry (1970, xiii–xvi, xx–xxi, 235).

[45] Cavendish loaned Dalrymple £500 in each of several years, 1783, 1799, 1800, and 1807. Dalrymple needed money to pay debts due immediately. Upon his death, his administrator asked Cavendish to tell him how much was owed him. The matter was still pending a few years later when Cavendish died. "27 December 1811 Principal Money and Interest This Day Received of Alex. Dalrymple Esq. Exctr. £ 2873.3.5," Devon. Coll., L/31/64 and 34/64.

[46] James Horsburgh (1805). "Horsburgh, James," *DNB*, 1st ed. 9:1270–71. Fry (1970, 253–255). Certificates, Royal Society 6: James Horsburgh (proposed 21 Nov. 1805).

[47] Certificates, Royal Society 3:209, Robert Barker (proposed 15 Dec. 1774); ibid. 4:23, Josias Dupré (proposed 25 Feb. 1779). "Barker, Sir Robert," *DNB*, 1st ed. 1:1128–29.

returned from a Voyage of Discoveries in the South Seas"; Isaac Titsingh, who was "long resident in various parts of the East, particularly Japan, skilled in various branches of natural knowledge"; William Bligh, who was "Post Captain in H.M.'s Navy […] whose Voyages to the Pacific Ocean have established his character as an able Navigator, whilst they enriched our Westindian Colonies with the most valuable productions of the South Sea Island"; John Thomas Stanley, "who has lately made a voyage to Iceland for the improvement of natural knowledge"; and John Hunter, Cavendish's personal physician who had recently returned from Jamaica, where he served as superintendent of military hospitals, and was soon to bring out his book on the diseases of the army in Jamaica.[48] These widely traveled men Cavendish recommended as members of the Royal Society over a period of thirty years, evidence that he wanted the Society not to be limited to people like himself, Londoners who rarely left town. He welcomed as members men who had direct experience of the wider world, counteracting parochial tendencies, and insuring a vigorous scientific life in the metropolis.

Figure 10.4: Royal Society. Painting by Frederick William Fairholt, engraving by H. Melville. This is the meeting room of the Royal Society at Somerset House 1780–1857. Over the last thirty years of his life, Cavendish came regularly to meetings here. The president of the Society is at the center, and the two secretaries at either side. The paintings on the wall are of past distinguished members. Reproduced by permission of the President and Council of the Royal Society. Wikimedia Commons.

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[48] Certificates, Royal Society 3:237, James Cook (proposed 23 Nov. 1775); ibid. 4:56, James King (proposed 23 Nov. 1780); ibid. 5, John Hunter (elected 12 Jan. 1786); ibid. 5, John Thomas Stanley (elected 29 Apr. 1790); ibid. 5, Samuel Davis (elected 28 June 1792); ibid. 5, Isaac Titsingh (elected 22 June 1797); ibid. 6, William Bligh (elected 19 Feb. 1801).

The meeting place of the Royal Society at Crane Court was cramped, and when Joseph Banks became president in 1778, he approached the government for ampler quarters, a prospect which the Society had been considering for some years. Cavendish was appointed to a committee to meet with the architect about fitting up apartments in the new home of the Society, Somerset House (Fig. 10.4). Having examined the meteorological instruments of the Society a few years before and advising on their use at Crane Court, he was charged with their relocation.[49] In his report to the Council he was particularly concerned with the "error" of a thermometer, which he proposed setting some feet away from the sunlit wall, hardly "any eye sore," though he preferred a window of the room where the Society of Antiquaries met, if they would permit it.[50] Subsequently, he was appointed to a committee to oversee the meteorological journal at the Society's new home.[51] Although it was not exactly spacious, Somerset House had more room, and it was better located than Crane Court.[52] In the meeting room, the president sat on a high-backed chair, looking like a judge, well above the table at which the secretaries sat, while the ordinary members sat on benches with rail backs resembling pews. For the last thirty years of his life, Cavendish came regularly to this meeting room, where he sat beneath paintings of illustrious past members, crammed on the walls one above another. (By refusing to sit for a painting, he ensured that he would not be exhibited on those walls exposed to the eyes of strangers.) The next move of the Society was not until 1857, when its new home was Burlington House in Piccadilly, which had belonged to the Cavendishes.

In a manuscript in the British Library, an anonymously reported fragment of conversation reads, "Mr. Cavendish rather wished to have the Presidentship." This follows immediately after fragments attributed to Aubert and Smeaton, evidently part of the same conversation: "Aubert asked Russell how his mercantile character would be affected by being a Candidate for the Presidentship of the Royal Society. Smeaton said he should vote for him."[53] The subject dates the conversation. When in 1778 the president of the Royal Society Pringle resigned, Aubert was one of two candidates picked to replace him, the other being Banks. Because of his shyness, it seems unlikely that Cavendish would ever have wanted to be president, and at first we discounted the gossip, but it is not inconceivable. He worked with members of the Council constantly; he served on committees, sometimes heading them; and on occasion he presided over meetings. Somebody must have heard Cavendish express the desire to be president or heard someone who heard him, unless it was a joke. In his scientific work, Cavendish followed his father and went beyond him, and in his service to the Society, he followed his father again. Charles Cavendish had declined entreaties to become president, and perhaps Henry was prepared to go beyond his father in this way too. Perhaps he was also inspired by Newton, who was an energetic and conscientious president of the Royal Society. Serious about the scientific content of the meetings of the Royal Society, when Newton presided there was no laughter or inattentive whispering. Called a "grand administrator of science" and scientifically preeminent, Newton was in

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[49] 16 Mar., 6 July 1781, Minutes of Council, Royal Society 6:397, 439.

[50] 2 Aug. 1781, ibid. 6:440–442: "A Report from Mr. Cavendish Concerning the Meteorological Apparatus." The Society's concern with placing the meteorological instruments continued, leading to a committee formed of Cavendish, Aubert, Heberden, Deluc, Watson and Francis Wollaston: 12 Feb. 1784, ibid. 7:62.

[51] 19 Jan. 1786, Minutes of Council, Royal Society 7:138. This committee consisted of Cavendish, Chambers, Aubert, Kirwan, and Shuckburgh.

[52] D.C. Martin (1967, 16).

[53] "Notes of Conversations 1770–1790," BL Add Mss 35,258, f. 15.

these ways probably Cavendish's idea of a good president.[54] The Royal Society was by far the most important body in Cavendish's life just as natural philosophy was by far his most important work; it may have seemed natural to him to serve as its able president.

British Museum

In 1773 Cavendish joined his father as a trustee of the British Museum.[55] During his tenure, general meetings of the trustees were held two to five times a year, occasionally as many as six or seven, with attendance averaging about eight to ten, but often not enough for a quorum. The few other trustees who attended frequently were the Cavendishes and their acquaintances from the Royal Society and their relatives: Banks, Wray, Watson, Pringle, Yorke (now Lord Hardwicke), and Lord Bessborough.[56] The standing committee of the trustees took care of most of the business and prepared reports for the general meetings. The committee, which in effect was any trustees who cared to attend, met weekly until the 1780s, then fortnightly, and eventually monthly.[57] For ten years Cavendish came regularly to the meetings of the committee with his father, a commitment which was both substantial and unusual, since rarely as many as six attended the meetings. Those who came were usually the same as those who came to the general meetings.

The standing committee had a wide range of responsibilities, mostly having to do with routine matters, such as paying bills and performing audits, but there was also an unpredictable element. The committee routinely gave permission for visitors to copy documents and draw birds but also, on occasion, to examine human monsters under the inspection of an officer of the Museum. It heard standard complaints about the cold of the medals room and the damp of the reading room, but it also heard about the infighting of the staff, whom the committee ordered to stop quarreling and be amicable.[58] It laid out money to buy or to subscribe to important works of science for the library such as Robert Smith's *System of Opticks* and Samuel Horsley's edition of Newton's works.[59] It noted gifts of books and collectibles. Just before Cavendish was elected a trustee, John Walsh and John Hunter presented two specimens of the electric eel,[60] and two years later, just as Cavendish was beginning his experiments on an artificial electric eel, Walsh presented another electric eel whose organs had been laid open by Hunter, who presented a transverse section of an electric eel.[61] Occasionally gifts were substantial: in 1773 Banks presented his large collection of Icelandic sagas, and Lord Rockingham presented his large collection of animals preserved in spirit in seventy-two glasses, to which he added more glasses the next year. Most gifts, however,

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[54] Richard S. Westfall (1980, 630, 634–635). Frank E. Manuel (1968, 266, 281). The domineering side of Newton probably would not have been Cavendish's preference.

[55] Cavendish was elected trustee on 8 Dec. 1773. Minutes of the General Meeting of the Trustees, vol. 3. His record of attendance over the years is in the Minutes of the British Museum: Committee, vols. 5 to 9; General Meeting, vols. 3 to 5.

[56] Sometimes he attended all of the meetings, but often he came to only some. What was for him a less than exemplary attendance no doubt owed to the largely formal nature of its proceedings.

[57] P.R. Harris (1998, 11).

[58] The order for amicable relations was made on 9 May 1777. Committee Minutes of the British Museum, BL, 6.

[59] 31 July and 11 Sep. 1778, ibid.

[60] 23 Apr. 1773, ibid., vol. 5.

[61] Walsh's gift was in January or February 1775, and Hunter's was on 16 June 1775, Diary and Occurrence-Book of the British Museum, BL Add Mss 45875, 6.

were isolated curiosities of the sort that were written about in the *Philosophical Transactions*, a six-legged pig, a frog preserved in amber, the head of a seahorse, and, presented by Charles Cavendish, a "curious Specimen of a double Egg."[62] Stuffed birds from the Cape of Good Hope, serpents from the East Indies, shells from Labrador, insects from Jamaica, a gun and powderhorn from Bengal, Captain Cook's artificial curiosities from the South Sea islands, and much more from Britain's colonial extremities and seafaring way of life piled up in the British Museum. First Charles Cavendish, then Charles and Henry together, and then Henry gave conscientious attention to the affairs of the British Museum for over fifty years. Through this central, public institution for books and collections, they served the public and the cause of learning.

Society of Antiquaries

In the same year that he became a trustee of the British Museum, 1773, Cavendish was elected a fellow of the Society of Antiquaries of London. Described as a gentleman of "great Abilities, & extensive knowledge," but with no mention of any accomplishments in antiquarian scholarship, Cavendish was recommended by Heberden, Wray, Burrow, Colebrook, Barrington, and Jean Louis Petit, all of whom were also members of the Royal Society.[63] Macclesfield, Banks, Birch, and other colleagues of Cavendish's from the Royal Society were also members, indicative of the overlap in membership of the two societies.[64]

Originating with a group who met in a coffee house to discuss history and genealogy, the Society of Antiquaries was formally created, or re-created, in 1717. The leading spirit of the Society in its early years was the physician William Stukeley, a productive antiquarian, known as the "Archdruid of this age,"[65] who was also a prominent member of the Royal Society. Early on there was an attempt to merge the Antiquarian Society and the Royal Society, but the stronger desire was for separateness and equality. In 1751 Martin Folkes, who was at the same time president of the Society of Antiquaries and president of the Royal Society, pushed through a reform to establish a Council and officers for the Society of Antiquaries in imitation of those for the Royal Society, and in that year the Society was granted a royal charter.[66] In other ways too, it imitated the Royal Society, acquiring a dining club, a journal, and a committee of papers. Fellows of the Royal Society, it would seem, sometimes acted in concert in the politics of the other society, and it no doubt worked the other way too.[67] A large proportion of the officers and Council of the Society of Antiquaries were fellows of the Royal Society. At the time the Society of Antiquaries received its charter, a member wishing to make public new discoveries in antiquities might consider doing so through either the Royal Society or the Society of Antiquaries. Francis Drake, F.R.S. and F.S.A., told Charles Lyttleton, F.R.S. and future president of the Society of Antiquaries, that he had

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[62] Meeting on 13 Sep. 1776, Committee Minutes of the British Museum, BL, 6.

[63] Cavendish was proposed on 21 Jan. 1773 and elected on 25 Feb. 1773; on 18 Mar. he paid his admission fee and was admitted to the Society. Minute Book, Society of Antiquaries, 12:53, 580, 610.

[64] Of the twenty-one members of the Council of the Society of Antiquaries in 1760, eleven , including the president, were also fellows of the Royal Society, and of its ordinary membership forty-six were fellows of the Royal Society. "A List of the Society of Antiquaries of London, Apr. 23, MDCCLX," BL, Edgerton 2381, ff. 172–175.

[65] "William Stukeley, M.D.," in William Munk (1878, 74).

[66] Joan Evans (1956, 442).

[67] Peter Davall to Thomas Birch, 22 Apr. 1754, BL Add Mss 4304, vol. 5, f. 126. Daniel Wray to Thomas Birch, 7 Mar. 1753, BL Add Mss 4322, f. 111.

had better success communicating discoveries of antiquities to the Royal Society than to the Society of Antiquaries, and that he was inclined to follow that guide with his present subject, a Roman alter, as he did, publishing his paper in the *Philosophical Transactions*.[68] James Burrow, F.R.S. and F.S.A., sent a paper to Thomas Birch, F.R.S. and F.S.A., saying that he always intended it for the Society of Antiquaries and "never entertained the least thought of communicating it to the Royal Society, since it cannot pretend to be of any use towards the advancement of *natural* knowledge," but because of an opinion of the committee of papers, he was sending it to the Royal Society after all.[69] The division between topics belonging to the Royal Society and those belonging to the antiquaries was evidently clear in principle to Burrow, but in practice it was not sharply drawn. The Society of Antiquaries also had interests in common with the British Museum, an institution which drew support from the "antiquarian milieu."[70]

The duty of the Society of Antiquaries was to record "Antient Monuments," such as cities, roads, churches, statues, tombs, utensils, medals, deeds, letters, and whatever other ruins and writings belonged to the "History of Brittish Antiquitys."[71] The meaning to be derived from such objects was a matter of judgment and strong feeling. When Cavendish joined the Society, its minutes recorded long papers, which revealed contemporary views on the direction of the field. There was, for instance, a paper on the history of Manchester, written on a "rational plan," which promised to rise above the parochialism of the usual town histories to illuminate the "general polity" of towns and the "general antiquities" of the entire kingdom and to lay open the causes and circumstances of "any momentous events" affecting Manchester. Antiquaries could condemn antiquarianism in the pejorative meaning of the term.[72] Other papers from this time made a moral point; for example, a history of cockfighting corrected the "errors" of the modern writers, but its main purpose was to show the perversion of cockfighting from a religious and political institution for instilling valor to the present day pastime founded on cruelty, finding it offensive to humanity that "rational & civiliz'd minds" could take enjoyment in this spectacle.[73]

In 1770 the Society of Antiquaries introduced its own journal, *Archaeologia*, an occasion for a forceful statement of the purpose of the Society by its director Richard Gough. The chartered antiquaries have as their object not their "own entertainment" but the communication of their "researches to the public." Belonging to the modern "age wherein every part of science is advancing to perfection," antiquaries had a duty to make proper use of their facts: "*history*" was not a poetic narrative but a "regular" inquiry into the records and proofs of the past.[74] Apart from their common objectives, "science," "knowledge," and "truth," and their common membership, the Society of Antiquaries and the Royal Society had common work. Because science had its own antiquities, both societies had a concern with the history

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[68] Francis Drake to Charles Lyttleton, 26 Jan. 1756, Correspondence of C. Lyttleton, BL, Stowe Mss 753, ff. 288–89.

[69] The Royal Society's committee of papers sent Burrow's paper to the secretary of the Royal Society, having drawn red lines through the passages that Burrow had expressly addressed to the Society of Antiquaries. James Burrow to Thomas Birch, 18 June 1762, Birch Correspondence, BL Add Mss 4301, vol. 2, 363.

[70] David Philip Miller (1981, 46).

[71] In Stukeley's hand, in the first Minute Book of the Society, quoted in Evans (1956, 58).

[72] John Whitaker, "The History of Manchester," 6 Dec. 1770, Minute Book, Society of Antiquaries, 11.

[73] Samuel Pegge, "A Memoir on Cockfighting: wherein the Antiquity of It, as a Pastime, Is Examined & Stated; Some Errors of the Moderns Concerning It Are Corrected; & the Retention of It amongst Christians Absolutely Condemned & Proscribed," 11, 12 and 19 March 1772, Minute Book, Royal Society of Antiquaries, 11.

[74] Richard Gough on the purpose of the Society of Antiquaries' publication, in vol. 1, 1770, of *Archaeologia*.

and biography of science.[75] History and natural history were both collecting activities,[76] and history and astronomy were both dating activities, ensuring a lively interaction between these fields at times.[77] Antiquaries were interested in views of Pompeii and the like, and there was interest in the Gothic as well is in the Classic, but there was also interest in contemporary history, so strongly marked by science and technology; an example was a history of the Society of Arts, to which Charles and Henry Cavendish belonged.[78]

Cavendish became a member of the Society of Antiquaries at a time when the membership was rapidly growing, having nearly doubled in the ten years before his election.[79] Many of the new members came from the upper classes, including the nobility. Many also came from science: in the same year as Cavendish, Franklin and Pringle were elected. There is the suggestion that both wealthy and learned persons entered the Society to receive its new journal, *Archaeologia*.[80] Cavendish took considerable interest in papers in that journal having to do with India; his own paper on the Hindu calendar fitted either it or the *Philosophical Transactions*, which was where he published it.

Cavendish's membership in the Society of Antiquaries together with his membership in the Royal Society and his trusteeship in the British Museum was inscribed on the plate of his coffin, but to Cavendish the affiliations were not of equal importance. He applied himself to the affairs of the Royal Society and of the British Museum, whereas he took no responsibilities in the Society of Antiquaries. He entered the record only once and then as an intermediary, submitting drawings of an Indian pagoda in the name of his scientific colleague Alexander Dalrymple.[81]

There was a plan to bring together in the same meeting place the Society of Antiquaries, the Royal Society, the British Museum, and the Royal Academy of Painting, Sculpture and Architecture. It was only partly to be realized. In 1753 the Society of Antiquaries took over a former coffeehouse on Chancery Lane, and the British Museum moved into Montague house the following year. Twenty years later, the Royal Society began planning its apartments for its new location, Somerset House. Cavendish, who was much involved with that move, agreed with others on the Council that it would be a "great inconvenience" for the Royal Society to have any apartments in common with the Society of Antiquaries, or even

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[75] There are many letters from members of the Royal Society to John Ward, president of the Society of Antiquaries, professor of rhetoric at Gresham College, and F.R.S. He published frequently on antiquities in the *Philosophical Transactions*. He helped locate letters of the chemist Robert Boyle for the benefit of the Royal Society: Henry Miles, F.R.S., to John Ward, 10 Feb. 1742/41 and 13 June 1746, Letters of Learned Men to Professor Ward, BL Add Mss 6210, ff. 248–50.

[76] In connection with a natural history of fossils, Emanuel Mendes da Costa wrote to John Ward to ask if certain Roman vases were made of marble or porcelain. Letter of 13 Nov. 1754, Letters of Learned Men to Professor Ward.

[77] Concerned with Homer's placement of Troy, John Machin wrote to John Ward: "My whole time has been employed in tedious and irksome calculations to adjust and settle the moons mean motion, in order to make a proper use of the eclipse at the death of Patroculus." 23 Oct. 1745, ibid., ff, 230–231.

[78] This "history of the rise and progress of the Society for the Encouragement of Arts, Manufactures & Sciences" was read at the meetings of 1 and 8 June 1758. The paper was kept in a folio with the purpose of entering "occurrences of our own time." Emanuel da Costa, "Minutes of the Royal Society and the Society of Antiquaries," BL, Edgerton Mss 2381, ff. 57–58.

[79] Membership was 173 in 1764 and 290 in 1774. Evans (1956, 148).

[80] Between 1770 and 1775, sixteen upper-class members, including Cavendish, were elected. Ibid., 150.

[81] Henry Cavendish to William Norris, undated. This letter, which is in the library of the Society of Antiquaries, has to do with an extract by Alexander Dalrymple from a journal in the possession of the East India Company, evidently referring to an "Account of a Curious Pagoda near Bombay …," drawn up by Captain Pyke in 1712, and communicated to the Society of Antiquaries on 10 Feb. 1780 by Dalrymple; published in *Archaeologia* 7 (1785): 323–332.

a common staircase. The public apartments of the Royal Society "will be understood by all Europe, as meant to confer on them an external splendor, in some measure proportioned to the consideration in which they have been held for more than a century."[82] A week later the architect William Chambers informed the Royal Society that its wish could not be met: no space could be allotted to the Royal Society consistent with its "splendor" other than what it had in common with the Society of Antiquaries. Their common location said nothing about their interests, which were becoming more differentiated.[83]

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[82] 10 May 1776, Minutes of Council, Royal Society 6:302–303.

[83] 18 May 1776, ibid., 304–306. Evans (1956, 152).

Chapter 11
Places

Charles Cavendish was remarkably healthy. He experienced the almost universal malady of that time, "gout," but he was not crippled by it,[1] and to judge by his attendance at meetings, he did not suffer from any protracted illnesses. He came to a meeting of the standing committee of the British Museum as late as 7 February 1783.[2] He was nearly seventy-nine when he died, on 28 April 1783.[3] Not yet remembered as the father of Henry Cavendish, his obituary notice in *Gentleman's Magazine* identified him as the great uncle to the present duke of Devonshire, who but for his title was undistinguished. The obituary also said that Charles was ninety, but it got him right when it called him an "excellent philosopher."[4]

For a man so well off, Cavendish's will was extremely brief, as his son Henry's would be too. Unchanged since he made it nearly thirty years before, it left £4000 to Charles's youngest son, Frederick, compensation for what he had taken from Frederick's estate, and £1000 for charity. His personal estate went to his oldest son and sole executor, Henry.[5]

At some point, probably when he resettled after his father's death, Henry made an inventory of his and his father's papers labeled *Fathers papers* and *Mine*, which he kept in a tall walnut cabinet with an upper case. His father's personal papers have all been separated and evidently lost, but it was unlikely to have been Henry who lost them; rather he classified and stored them under lock and key. Papers that we do not have but that Henry did include letters of his father's, mother's, and brother's, Ruvigny papers, poetry, genealogy, mathematical papers, pocketbook of experiments, measurements (probably meteorological) taken at Chatsworth, and papers on meteorological instruments, refracting telescopes, crystals, artificial cold, and specific gravity. Papers of Charles's that have survived are mainly legal documents having to do with wills, annuities, titles, rents, dividends, lawsuits, and his marriage settlement. Henry's own papers in the combined classification have to do with much the same things as his father's, which came with their station, properties and lawyers.[6]

Upon the death of Lord Charles Cavendish, there was a small, almost imperceptible change in protocol. In his publications in the *Philosophical Transactions*, Henry Cavendish's name was no longer preceded by "*Hon*," a courtesy title once removed.[7] From

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[1] Charles Cavendish to Thomas Revill, draft, 2 Mar. 1765, Devon. Coll., L/31/20.

[2] 7 Feb. 1783, Committee Minutes, British Museum, 7.

[3] Devon. Coll., L/31/37.

[4] Anonymous obituary of Charles Cavendish (1783).

[5] Charles Cavendish's will was probated on 28 May 1783. "Special Probate of the Last Will and Testament of the Right Hon$^{\text{ble}}$ Charles Cavendish Esq. Commonly Called Lord Charles Cavendish Deceased," Devon. Coll., L/69/12.

[6] "Walnut Cabinet in Bed Chamber," "Papers in Walnut Cabinet," and "List of Papers Classed," Cavendish Mss Misc.

[7] "The Honourable" followed by a given name and surname was allowed the sons of earls and the children of viscounts and barons. Other than for a duke, who was called "His Grace," and a marquess, who was called "The Most Honourable," the title "The Right Honourable" was given to all peers as a courtesy. The son of a peer, Charles

1783 on, he was Henry Cavendish "Esquire" or simply Henry Cavendish. One year after Charles's death, Blagden commented that "no address is requisite to please Mr. [Henry] Cavendish."[8]

Following his father's death at the end of April, Henry was absent from the first two dinners of the Royal Society Club in May, the only dinners he missed that year.[9] Writing to Henry in late May, John Michell apologized for imposing on him "so soon after the loss of Ld Charles."[10] As to the meaning of the loss to Henry we can only speculate, but we believe that no one had been as important to him as his father; we base our belief on several considerations.

Cavendish was "educated and trained by his father from very early youth to scientific pursuits," according to a contemporary.[11] His father sent him to a secondary school with a modern curriculum and then to a university with a Newtonian curriculum, and at both places his father made social contact with the persons in charge. The year after he left the university, his father began to bring him to dinners with his friends from the Royal Society. Five years after that, Henry began attending meetings of the Royal Society as a guest of his father's. His first recommendation of a candidate at the Royal Society was made jointly with his father. His father was not on the Council during Henry's first term, but because the Council was elected, their separation perhaps could not be helped; they were on it together in 1769. Henry joined the same scientific clubs as his father. His father was present at Henry's early attendances at general meetings of trustees and at meetings of the standing committee of the British Museum. In his work at the Royal Society and the British Museum, Henry showed the same diligence as his father. His early scientific researches at home were done with his father's instruments, books, and journals, and he and his father made observations together. In his penchant for accuracy in his scientific work, he followed in his father's path. Henry had the example of his father before him, and he evidently approved of it, for he imitated it. This is the evidence of his father's importance to him in his life of science.

Despite Charles Cavendish's privileges, his life had a sad aspect. His wife died while he was still in his twenties, leaving him with two small boys to bring up. While in his teens, the youngest boy, Frederick, suffered an accident that left him impaired and dependent on his father, and his oldest son was socially impaired. Charles, it would seem, shepherded and sheltered Henry until he was ready to go into the world.

His life also had its gratifications. Within his family and in the wider society he took on strenuous duties, which he performed admirably. His scientific work was skillful and recognized. Of his achievements, the assistance he provided his intelligent and diffident son Henry was the most consequential. He died with the knowledge that Henry was in charge of his life and master of his chosen work, science.

Cavendish was called "The Right Honourable" or, more often, "Lord," and occasionally "The Right Honourable Lord," both parts of his title being by courtesy and proper. His son Henry was called "Honourable" by courtesy. *Treasures from Chatsworth, The Devonshire Inheritance.* A Loan Exhibition from the Devonshire Collection, by Permission of the Duke of Devonshire and the Trustees of the Chatsworth Settlement, Organized and Circulated by the International Exhibits Foundation, 1979–1980, 24.

[8] Charles Blagden to William Cullen, 17 June 1784, draft, Blagden Letterbook, Yale.

[9] They were dinners on 1 and 8 May 1783. Minute Books of the Royal Society Club, Royal Society.

[10] John Michell to Henry Cavendish, 26 May 1783; in Jungnickel and McCormmach (1999, 566).

[11] John Walker to James Edward Smith, 16 Mar. 1810, Smith (1832, 170–171).

Leaving Home

Charles Cavendish appears on the rate books for his house on Great Marlborough Street until his death in 1783, after which Henry is listed,[12] and for a time Henry evidently used it as his townhouse. In 1782 he rented a country house in Hampstead, located north of London.[13] William Thornton's guide to London and the surrounding countryside published in 1784 gives us an idea of Hampstead at the time Cavendish moved there: the village "is now of considerable extent. Many of the citizens of London have fine houses here, because the situation is not only delightful, but the air is esteemed exceeding wholesome.… At the north extremity of the village is a heath or common, which is adorned with many handsome buildings, and is so elevated, as to command one of the most extensive prospects of the kingdom."[14] Fashionable Hampstead offered Londoners a vista and an escape from city stench.

Hampstead

Figure 11.1: No. 34 Church Row, Hampstead. Between 1782 and 1785, Cavendish lived in a house at the end of this row next to the church. But for the automobiles, this street with its terraced houses and church looks much the same as it did then. Photograph by the authors.

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[12] 12 June 1783, Paving Rate Books, Great Marlborough Street/Marlborough Mews, Westminster Archive, D 1260.
[13] Cavendish first appears in the rate books on 3 Jan. 1782. "Hampstead Vestry. Poor Rate," Holborn Public Library, London.
[14] William Thornton (1784, 482).

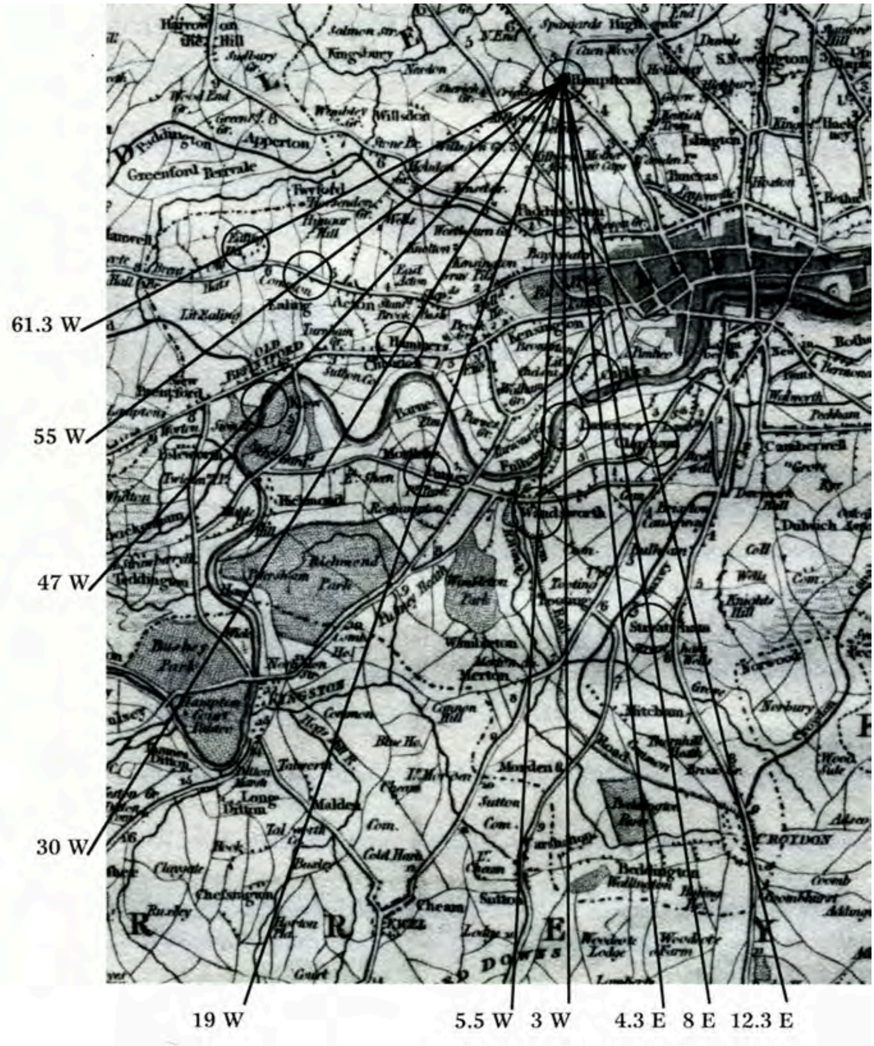
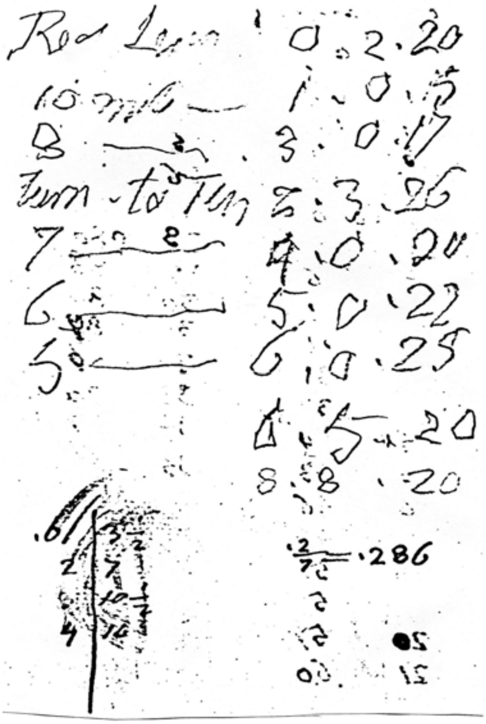


Figure 11.2: Hampstead Bearings. From his country house in Hampstead, Cavendish took bearings in the direction of London. With a theodolite, he recorded the angular position of tall objects through an arc of about sixty degrees. Prominent among the objects were steeples, as we would expect from the picture of Westminster Bridge above; the London skyline was marked by steeples. On the map of London and environs published by R. Phillips in 1808, I have drawn Cavendish's lines of sight for a number of steeples, labeling them with the angles he measured. From right to left: 1. New houses on the road to Clapham. 2. Streatham steeple. 3. Chelsea steeple. 4. Battersea steeple. 5. Wandsworth steeple. 6. Putney steeple. 7. Hammersmith steeple. 8. Kew Chapel. 9. Acton steeple. 10. Ealing steeple. "Bearings," Cavendish Mss, Misc.

Figure 11.3: Hampstead Environs. From his house at Hampstead, Cavendish made trips into the surrounding countryside, noting milestones and other markers, such as churches and villages, which we indicate by circles on this map of the portion of the County Middlesex directly north of London. Locations and mileages are from several miscellaneous sheets in Cavendish Mss.



Red Lyon 0.2.20
10 mile 1.0.15
9 3.0.17
Turn to Fin [illegible].3.26
7 4.0.20
6 5.0.22
5 6.0.25
[illegible].5.20
8.8.20
.2/7 = .286

Figure 11.4: Mileage Counter. This page was obviously written by Cavendish while moving, the unsteadiness of his hand giving an idea what travel was like then. The abbreviated place names are Red Lyon, about 8½ miles from his home, and Finchley Church, about 2 miles closer. Cavendish recorded several local journeys with a measurer, 35 revolutions equaling 1/10 of a mile. Between places marked on the map of the previous illustration, this table gives the distance in miles. We are not certain what his means of conveyance was when he took these measurements, but we know that he had an "odometer" attached to the wheels of his carriage. Such an instrument could be bought for 7 to 10 guineas, and it was thought to be accurate to within 1%. After Cavendish's death, his "way-wiser" passed to the instrument maker Newman, who presented it to the museum of King's College, London. It was there when Wilson wrote his biography of Cavendish, but according to our inquiry it no longer is. Benjamin Vaughan to Thomas Jefferson, 2 Aug. 1788, in Boyd (1956, 460). The sheet of distances is reproduced with permission of the Trustees of the Chatsworth Settlement.

In the late seventeenth century, Hampstead began to change from a rural to an urban village. A mineral spring was opened, earning the village a reputation for healthiness as well as a good income from its water, which was recommended by physicians who drank it themselves. A popular destination early in the eighteenth century, Hampstead remained a resort, while its continuing growth owed to prosperous Londoners such as Cavendish taking up residence. Cavendish's address was 34 Church Row, the street of choice in Hampstead, where visitors congregated and persons of "quality" promenaded. In appearance, the attractive, terraced houses have changed little since Cavendish's day (Fig. 11.1).[15]

Cavendish's activities were now divided between two locations, the exact separation of which was an astronomical datum: "Hampstead is 1,82 miles or 10.2 seconds of time west of Marlborough street," he recorded.[16] During the first spring at his new country house, he compared the good air of Hampstead with the foul air of the city,[17] assisted by the instrument maker Edward Nairne, who lived a few doors away, at 21 Church Row. During his first winter, he busied himself with experiments on the freezing temperature of mercury. From his house, he sighted on the weathercock of the parish church next door, and from the steeple he or an associate surveyed the countryside with a quadrant. The vista from Hampstead was broad. Cavendish took bearings of the duke of Devonshire's Palladian house at Chiswick; of temples, gazebos, and pagodas; and of the steeples at Walton, Battersea, Hammersmith, Stretham, Acton, Paddington, Chelsea, and Ealing, and of the steeple of the church at Clapham Common, on the far side of London, the location of his next country house (Fig. 11.9).[18] Cavendish's final appearance in the Hampstead rate books is on 17 September 1785. This stage of leaving home lasted three and a half years.

Bedford Square

For a time, Cavendish employed a young man Charles Cullen, exactly in what capacity is unclear, but it involved translating from the Swedish. Charles was a son of the Edinburgh professor of medicine William Cullen, Blagden's teacher and friend. In a letter to Blagden in May 1784, William Cullen spoke of Charles's "circumstances into which he had unluckily fallen," and of his gratitude to Blagden for referring him to Cavendish.[19] Blagden replied that his son had been "totally unacquainted both with the book & the subjects in Mr. Cavendish's line of studies," but that Cavendish had not expressed "any dissatisfaction with your son's conduct, & more cannot yet be expected."[20] In November Charles Cullen wrote to Blagden that he was about to part from Cavendish.[21] In a later undated letter to Blagden, he said that he "felt with much justice the force of the objection made to his deficiency in skill and acquaintance with books." He should have consulted with Blagden and with J.C. Dryander, Banks's Swedish botanist and librarian, "but the truth is the moving from Marlboro Street to Bedford Square had divided his attention from the object to which he

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[15] Alex J. Philip (1912, 45–46). F.M.L. Thompson (1974, 20–22, 24–26). Stabling could be had in the village, and coach service into London was convenient, there being between fourteen and eighteen return trips a day. Thomas J. Barrett (1912, 1:279–280). "Hampstead Vestry. Poor Rate."

[16] Cavendish Mss Misc.

[17] Henry Cavendish, minutes of experiments on air, 15 and 16 Mar. 1782, Cavendish Mss II, 5:189.

[18] We assume that this Edward Nairne was the instrument maker of that name. 17 Dec. 1782 and 15 Jan. 1783, Charles Blagden Diary, Royal Society, 1. Henry Cavendish to John Michell, 27 May 1783, draft; in Jungnickel and McCormmach (1999, 267–269). Cavendish had help with observations taken from the Hampstead church steeple, or he helped someone, as the angles are written in another hand, 23 and 25 July 1783. The unclassified papers in Cavendish's scientific manuscripts contain a great many sheets of observations of bearings, with dates falling between 1770 and 1792.

[19] William Cullen to Charles Blagden, 8 May 1784, Blagden Letters, Royal Society, C.70.

[20] Charles Blagden to William Cullen, 17 June 1784, draft, Blagden Letterbook, Yale.

[21] Charles Cullen to Charles Blagden, 7 Nov. 1784, Blagden Letters, Royal Society, C.62.

should have applied it and had sketched a plan to accomplish after the house was a little more settled."[22] In this section, we look at that unsettling move.

In 1784 Cavendish leased his father's house on Great Marlborough Street and the premises on Marlborough Mews behind it. Joshua Brookes, who lived in the house, continued the local scientific tradition in a bizarre fashion. Holding a "Theater of Anatomy" there in 1786–98, he lectured and exhibited bodies of notorious criminals, and in the garden behind the house, where Charles and Henry Cavendish had measured the Earth and the atmosphere with their delicate instruments, Brookes built a vivarium out of huge rocks, where he chained wild beasts.[23]

We do not know why Cavendish did not keep the house on Great Marlborough Street after his father died.[24] Perhaps the house he moved to on Bedford Square had better arrangements for the library he intended for it, or perhaps he preferred the location next to the British Museum, where he regularly attended meetings as a trustee. Bedford Square may have had an intrinsic attraction too as the first garden square in London to exhibit perfect uniformity and symmetry in its architecture, features which may have appealed to his mathematical side.

Exactly when he relocated can be clarified. The rate books for the house give the occupants: 1782–84 Dr. Tye, 1784–86 Hon. John Cavendish, and 1786—Hon. Henry Cavendish.[25] The second occupant, "Hon. John Cavendish" in 1784–86, would have been Henry Cavendish's first cousin the "Right Honourable," though commonly called "Lord," John Cavendish. However this identification is ruled out by the following exchange. In August 1785, John Cavendish wrote to Henry Cavendish, "The last time I came to Marlborough Street, & found your house so compleately shut up that I took it for granted you had quitted it." Henry Cavendish replied, "I am moved to the corner house of Be[dford] Sq[uare] & Gower street on the East side."[26] If Henry had bought the house from his cousin, his explanation would have been unnecessary. The rate books evidently were in error: John Cavendish is not among the occupants of the house. The original ninety-nine-year lease for the house in 1775 was to William Scott and Robert Grews, who in late 1783 leased it to the physician Dr. Michael Teighe for a period of eight years.[27] By an indenture between Dr. Teighe and Henry Cavendish, registered on 21 May 1784, Cavendish acquired the house, with an absolute purchase for £3250.[28] With this clarification, we see that he moved to Bed-

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[22] He asked for two or three months to remedy the defect, and if he failed he intended to resign. Charles Cullen to Charles Blagden, "Monday" [1784 or 1785], Blagden Letters, Royal Society, C.63. We assume that the following translation by Charles Cullen came out of his employment by Cavendish: Torbern Bergman, *A Chemical Analysis of Wolfram*, published in 1785. Cavendish was interested in wolfram, or tungsten.

[23] "Henry Cavendish to Mr. Joshua Brookes. Counterpart Lease of a Messuage or Tenement with the Apperts No. in Marlborough Street in the Parish of St James Westminster County Middlesex," 1788, Devon. Coll., L/38/35. London County Council, (1963, 256).

[24] In his will, Charles Cavendish left his personal estate to Henry; he said nothing about his real estate. He named Henry as his sole executor. In Henry Cavendish, "List of Papers Classed," under "Mine," there is an entry "agreement about house in M.S.," no doubt "Marlborough Street," where his father's house was. We have not found that agreement. Charles Cavendish's will, signed 1 August 1756, probated 28 May 1783, Devon. Coll., Chatsworth, L/69/12.

[25] London County Council (1914, 162).

[26] John Cavendish to Henry Cavendish, 25 Aug. 1785. Henry Cavendish to John Cavendish, n.d., draft, Devon. Coll.

[27] Bedford Estate Archive, NMR 16/21/3. We were misled by the rate books in the first edition of this book. Jungnickel and McCormmach (1999, 315).

[28] Middlesex Deed Register, MDR/1784/2/353.

ford Square when he quit his house on Great Marlborough Street. Five or six months after Cavendish bought the house, Blagden, who was by then Cavendish's associate, moved to a house on Gower Street, just off Bedford Square, a few houses from Cavendish's.[29] At age fifty-two, while still a Londoner and still a solitary, Cavendish was less narrowly a Londoner, being at the point of removing his main home permanently to a country suburb, and less solitary, having taken on an associate. In addition, by giving up his father's house and acquiring a new house on Bedford Square, he stepped out of his father's shade, though we have no reason to think that this was a motive.

Bedford Square

Figure 11.5: No. 11 Bedford Square. Front view. This was Cavendish's townhouse from 1784 to the end of his life.

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[29] Charles Blagden to John Blagden Hale, n.d., draft, Blagden Letterbook, Yale. In this letter Blagden told his brother that he was moving to Gower St. at the end of next week. He said that he watched Blanchard's balloon on the day he wrote the letter, which dates it, 16 Oct. or 30 Nov. 1784.

Figure 11.6: No. 11 Bedford Square. Back of house. Photographs by the authors.

Bedford Square was relatively new when Cavendish moved there. Laid out in 1775–80, it was one of a number of squares built in the West End of London starting in the late seventeenth century. An early form of town planning, the squares imposed a degree of order on an otherwise sprawling metropolis. They came about as joint ventures between owners of large estates and builders, who were granted low-rent, long-term leases. According to a historian of eighteenth-century London, Bedford Square, which was built on the estate of the duke of Bedford, a relative of Henry Cavendish's, was "probably the most important of the planned aristocratic building ventures of the century."[30] The houses followed a specified design, lending the square a standard appearance from all approaches. They were three-story with

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[30] George Rudé (1971, 14).

basements and attics, terraced, and built of brick, with wrought-iron balconies to the first-floor windows and entrance doors decorated by Coade stone with rounded fanlights above them. Each side of the square was a block of houses considered as a single unit, the center house set off by an ornamented stuccoed feature. Bounded by broad streets, the square was spacious, 520 by 320 feet between facing houses, with a large garden in the center for use of the residents.[31]

No. 11 Bedford Square, Cavendish's house, which today is used for offices by the nearby University of London, carries a bronze tablet donated by the duke of Bedford identifying it as having once belonged to the chemist. In style the house is the same as that of the blocks of houses, but it does not physically join them. It is an end-of-row house on the northeast corner of the square, on Gower Street, with its entrance on Montague Place (Figs. 11.5–11.6). The neighborhood has long since been densely built-up, but when Cavendish moved there, Gower Street quickly ran into the fields. Today Bedford Square is one of the best preserved garden squares in London.

After Cavendish's death, an appraiser wrote of the house, "I have scarce ever met with a more substantial or better built House, and the whole Edifice is finished with the best material." The floors of the two main stories were made of Norway oak, the staircase was made of Portland stone, and the dining and drawing rooms had carved marble chimney pieces.[32] All three stories and the attic for servants had bowed windows in the back looking out over a deep garden leading to the stables and coach house. The house had the quality, elegance, and expense expected of a wealthy Cavendish.

What is unusual is the use Cavendish made of it, a library for his books, which he lent to qualified borrowers. To serve this purpose, he made extensive alterations. When the house on Bedford Square was evaluated for sale after his death, it was estimated that because of its long use as "Libraries, and Museums," it would need renovations costing one third of the value of the house to make it "fit for the residence of a family."[33] We can picture the interior as Cavendish left it from an inventory of the fixtures, furniture, plate, and other contents of the twenty-one rooms. Inside the entrance, a semi-octagonal bay opened onto a hall at the end of which was a staircase leading to the upper floors. Off the hall to the left was a library room, which appears to have been used as a dining room, and to the right was a bow-window dining room, which appears to have been used as a library room, off of which were two smaller bow-window sitting or dressing rooms used for the same purpose. The floor above, the principal floor, consisted of two large drawing rooms, front and back, and a small side bow-window sitting room. The drawing room with the bow window was not used for books, but the rest of the floor was. The next floor up, the two pair floor, consisted of two bedrooms to the front, and a bow-window bedroom and dressing room to the back. All four rooms on this floor, which included Cavendish's bedroom, contained books. Only the attic, which had two bedrooms, a bow-window nursery, and a dressing room for servants, was not used for books. Bookcases were built of handsome uprights, with plinths and cornices, and sliding shelves. There were around 700 sliding shelves all told in the house, the front drawing room on the principal floor holding the largest number, 268. Cavendish's investment in the books

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[31] London County Council (1914, 150). Anon., "Bloomsbury Squares & Gardens. Bedford Square" (http://bloomsburysquares.wordpress.com/bedford-square).

[32] "J. Willcock's Valuation of House & Stables in Bedford Square," 30 Dec. 1813, Devon. Coll.

[33] Ibid.

that filled the shelves was enormous, valued at his death at £7000. To put this in perspective, his house on Bedford Square sold for half that, £3530.[34]

The house contained various pieces of furniture, evidently of the same quality as the house, some relating to what readers and writers require. The front drawing room on the principal floor had a pair of low steps, a pair of high steps, and a step ladder for reaching high shelves. It also had a glass-topped table, a column-and-claw table, four cushioned banister back chairs, two side desks, two black Wedgwood inkstands, and a table clock. The library room on the ground floor had in addition to shelving ten banister back chairs, a glass-top table with fly leaves, a table desk, and a black inkstand. The two smaller rooms adjoining it, formerly sitting or dressing rooms, contained in addition to shelving a copying machine with double roller and apparatus by Watt & Co., a cupboard for maps, a bracket minute clock by John Skelton, a barometer, and a thermometer. The hall and staircase had a thermometer and an astronomical timepiece by George Graham. The back drawing room on the principal floor, which had no bookshelves, had twelve Japanned elbow chairs, two oval mahogany tables, one of which was a dining table, and silk-covered fire screens. The dining room on the ground floor, the other large room without bookshelves, contained three dining tables and ten banister back chairs. The interior of the house was unified by the use of the color green throughout: mahogany blinds lined with green transparent canvas, curtains of green moreen, green fire screens and chair back screens, and green chair covers. The furniture was mostly mahogany, the main exception being the sliding shelves, which were made with less expensive deal, or fir. A contemporary of Cavendish's said that the "sole furniture" of his house on Bedford Square was a library.[35] This was an exaggeration—two large rooms of the house were used for other purposes, as we have seen—but it gave the correct feel of the house. A visitor touring the house when Cavendish lived in it would have concluded that it was a house of knowledge. It would also have told him that its owner was a wealthy aristocrat who was proud of his family. It contained six paintings, one a landscape, the others all portraits of Cavendishes, one of an earl of Devonshire before there was a dukedom.[36]

Library

From his father, Henry Cavendish inherited a good library, which he added to until the end of his life. For his work, a personal library was an asset, since scientific books and journals were not conveniently accessible. The British Museum owned and acquired scientific books, but its collection was inadequate for Cavendish's needs, and the library of the Royal Society was very defective in just those subjects that interested Cavendish, works in natural philosophy and mathematics, according to a library inspection in 1773.[37]

Unlike the Cavendishes, most persons interested in science in the eighteenth century could not afford to buy or to subscribe to many scientific books and journals, relying instead

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[34] "6 Sept. 1810. Mr Paynes Valuation of Books £7000"; "29 April &c. 1814 Account Respecting the Sale of a Leasehold House at the North East Corner of Bedford Square," Devon. Coll.

[35] John Barrow (1849, 148).

[36] "Inventory of Sundry Fixtures, Household Furniture, Plate, Linen, the Property of the Late Henry Cavendish Esquire at His Late Residence in Bedford Square. Taken the 2nd Day of April 1810," Devon. Coll., 114/74. *The Particulars of a Capital Leasehold House, and Offices Situate at the North East Corner of Bedford Square… Sold by Auction, by Mr. Willcock on Friday the Twenty-ninth of April, 1814*, Devon. Coll. There were in addition to the five family portraits in the house ten damaged ones in the lumber room over the stables.

[37] 24 June 1773, Minutes of Council, Royal Society 6:177–178.

on private libraries made available to them upon application to their owners. In England, large scientific libraries like Hans Sloane's and Joseph Banks's served the purpose of later public libraries, their owners treating their collections as a "public trust on behalf of learning."[38] In this spirit, Cavendish made available the library in his house on Bedford Square, performing a duty of public service as well as promoting science.

At an earlier time Cavendish may have kept his collection at another location. According to his biographer Wilson, he set apart for his library a "separate mansion in Dean Street, Soho."[39] The rate books for Dean Street contain no entries for Cavendish from 1783, the time for which we have record,[40] and we have found no other evidence of a Cavendish "mansion on Dean Street." We know for certain that sometime after he acquired his house on Bedford Square in 1784, he located his library there. John Barrow said that it was there, and we have ample other evidence including the inventory just mentioned.[41]

Despite Cavendish's reputation for clockwork routine, he was not particularly good at keeping order in his affairs and possessions. His books being described as in a "bad state of arrangement," it was suggested to Cavendish that he let a certain gentleman who was in need live in his house and organize them. It was this gentleman who began the catalog, a great, heavy volume now at Chatsworth. The entries are in more than one hand, none of them Cavendish's, indicating that the catalog was continued by another librarian after the first left. Cavendish did his part to maintain the order, signing the register for every book he borrowed to take to his other house at Clapham Common.[42]

The first we hear of Cavendish's librarian is in 1785, the year after Cavendish moved to Bedford Square. He was almost certainly a German by the name of Heydinger, who that fall went to the Custom House to receive a chest of books sent by King's Packet to Cavendish from abroad.[43] We hear of him again two years later in a similar capacity, this time seeing to it that a new chemical journal from Germany reached Cavendish.[44] This librarian was useful to Cavendish in another way; Blagden wrote to Cavendish that he hoped that he had got Heydinger to read a letter in German for him.[45] Heydinger must have had scientific interests, since at least twice Cavendish brought him to the Royal Society as his guest.[46] Thomas Young said that after Cavendish's German librarian died, Cavendish himself devoted one day a week to checking out books.[47] How long he kept up this practice we do not know, but

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[38] Raymond Irwin (1958, 179).

[39] Wilson (1851, 163), cites Cavendish's early biographers Cuvier and Biot on Cavendish's library. All that Biot says is that Cavendish located his library two leagues, or five English miles, from his residence so as not to be disturbed by readers consulting it. Five miles is roughly the distance from Clapham, the location of Cavendish's country house, to the center of London. Since neither Biot nor Cuvier mentions Dean Street, Wilson supplied this address from unknown sources. Georges Cuvier (1961, 237); J.B. Biot (1813, 273).

[40] Dean Street entries turn up intermittently through the assessment of the poor rates; entries for the years 1783, 1785, 1790, 1795 contain no reference to Cavendish. From 1781 the rate books were split between the wards of King Square, West, and Leicester Fields, West. Westminster Record Office.

[41] Barrow (1849, 148).

[42] Ibid. Cuvier (1961, 237).

[43] Charles Blagden to Joseph Banks, 15 and 30 Sep. 1785, Banks Correspondence, Royal Botanic Gardens, Kew 1:204 and 207.

[44] Charles Blagden to Lorenz Crell, 7 June 1787, draft, Blagden Letters, Royal Society 7:60.

[45] Charles Blagden to Henry Cavendish, 23 Sep. 1787, in Jungnickel and McCormmach (1999, 641–644). Cavendish read printed German but clearly not German script.

[46] 17 Apr. 1788 and 24 Dec. 1789, JB, Royal Society 33.

[47] Thomas Young (1816–1824, 435–447, on 445).

when he died he had a librarian who received a small salary, and it was probably he who dealt with the borrowers.[48]

To a prospective user of the library, Blagden explained the official policy: "Wishing to promote science by every measure in his power," Cavendish made his library accessible "at all seasons of the year." Blagden made clear that what was accessible was the library and not its owner: Cavendish did not want people even to sit in his library but to "borrow such books as they wish & take them home for a limited time."[49] Ordinarily it was the librarian and not Cavendish who met the public, but this arrangement did not entirely guard his privacy. The journalist Pahin de la Blancherie complained directly to Cavendish about the treatment he received from his librarian. Having requested a history of astronomy, he was told that Cavendish had just taken that book to Clapham Common. When he then asked for a biographical dictionary, the librarian told him that Cavendish had taken it too. The librarian told him to come back, and when he did, the librarian told him that Cavendish still had the books and moreover had great need for them. Having been thwarted at the British Museum and now at Cavendish's library, La Blancherie thought that the British nation owed him damages. He said he knew that Cavendish would not authorize this conduct by his librarian but would condemn it,[50] but we are inclined to think otherwise.[51]

One of Cavendish's librarians was the beneficiary of a remarkable instance of Cavendish's largess. This librarian lived in Cavendish's house until he left his employment and moved to the country. Some while later Cavendish was told that the man was in poor health. Cavendish was sorry to hear it, and when it was suggested that he might help him out with an annuity, he said, "Well, well, well, a check for ten thousand pounds, would that do?"[52]

A few years after Cavendish's death, the sixth duke of Devonshire assembled the magnificent Chatsworth library from his own collections and from Cavendish's library, which had been given to him by Cavendish's heir, Lord George Cavendish.[53] With the possible exception of about 450 books in their original paper covers[54] and some books at Holker Hall, Henry Cavendish's library today is bound in leather and dispersed among the other books at Chatsworth, shelved in the beautiful old Long Gallery. Constituting about one quarter of the ducal library, his books are identified both by his book stamp, a simple *Henry Cavendish*, and by his separate catalog number.

The catalog of Cavendish's library is incomplete, extending only to the early 1790s, and because he continued to buy books after that time, we can speak more accurately of the contents of his catalog than of his library. Books in Latin and books in English appear in roughly equal proportions in the catalog, each accounting for about one third of the total, with

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[48] "Collingwood, the Librarian, One Years Salary Due Xtmas 1811" in "29th May 1812. Taxes &c. for House in Bedford Square," Devon. Coll.

[49] Charles Blagden to Thomas Beddoes, 12 Mar. 1788, draft, Blagden Letters, Royal Society 7:129.

[50] Pahin de la Blancherie to Henry Cavendish, 23 Sep. 1794; in Jungnickel and McCormmach (1999, 697–698).

[51] La Blancherie having found that where he was living Newton once had lived tried to capitalize on it. Three years before he complained to Cavendish, he published a grandiose plan for honoring Newton. Cavendish probably did not like it. There was also a question of his methods of journalism. Blagden believed that he was a victim of the "worst kind of indiscretion" on La Blancherie's part. Charles Blagden to La Blancherie, 21 May and 23 Aug. 1785, drafts, Blagden Letterbook, Yale.

[52] Wilson (1851, 174). The librarian was probably not the German. Thomas Young said that after the German librarian died, Cavendish himself checked out the books, and if that is correct the German librarian did not leave to live in the country.

[53] Historical Notice by J.P. Lacaita, July 1879, *Catalog of the Library at Chatsworth*, 4 vols. (London, 1879) 1:xvii.

[54] Listed as "Cavendish Tract. Draft Catalog 1966."

books in French coming next, and then, in sharply reduce proportions, books in German and in other European languages. The catalog lists about 9000 titles, representing some 12,000 volumes,[55] showing that Cavendish had a large library, but not an immense one for the time. Sloane's library, the foundation of the library of the British Museum, was four times as large, and even Cavendish's seafaring friend Alexander Dalrymple had a larger library.[56] A number of Cavendish's colleagues had substantial libraries, though much smaller than his. Nevil Maskelyne's in 1811 contained 757 "lots," the term used in auction catalogs; John Playfair's in 1820, 1421 lots; Charles Hutton's in 1816, 1854 lots. Large libraries belonging to professional persons tended to be libraries of physicians with an interest in science; William Cullen's contained 3010 lots.[57]

Cavendish's library was open to the qualified public, but its contents were not selected with the public in mind. The largest category in the catalog was natural philosophy, with nearly 2000 titles.[58] In this same category were many books on medicine, anatomy, and animal economy, very few of which were published after Charles Cavendish died. Mathematics, the second largest category, included in addition to books on pure mathematics, books on natural philosophy in which mathematics was used, such as Newton's *Principia* and *Opticks* and Robert Smith's *System of Opticks*. Astronomy was a category of its own and well represented, including classic works of science by Copernicus, Brahe, Kepler, and others. In the natural history of life, Cavendish had only slight interest, but he was interested in other parts of natural history, buying many books on mineralogy and geology. He took an interest in books on voyages and travels, which related to his scientific work. About half of the books in the catalog were scientific. The category of poetry and plays was as large as that of mathematics, some 1100 volumes, including works by Shakespeare, Dryden, Congreve, Pope, Swift, Gray, and other authors one would expect to find in a literary library. After Charles's death, when Henry alone added to the library, there were no more books of poetry or plays, with the exception of an Indian drama.[59] Henry had a passing interest in history and antiquities, which were separate headings in the catalog, with several titles having to do with India. His catalog had no division for histories of individual lives, or biographies, though he bought *The Life of Samuel Johnson*. Its author James Boswell was a guest at dinners of the Royal Society Club at which Cavendish attended,[60] and Cavendish may have met or seen Johnson, who frequented the Crown & Anchor, where the Royal Society Club met. His catalog had no division for moral philosophy, though he bought Adam Smith's

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[55] R.A. Harvey (1980, 284).

[56] Part I of the catalog of Dalrymple's library contains 7190 entries. Part II, containing books on navigation and travel, his specialty, might be even longer. *A Catalog of the Extensive and Valuable Library of Books;* Part I. *Late the Property of Alex. Dalrymple, Esq. F.R.S. (Deceased). Hydrographer to the Board of Admiralty, and the Hon. East India Company, Which Will Be Sold by Auction, by Messrs. King & Lochée ... On Monday, May 29, 1809, and Twenty-three Following Days, at Twelve O' Clock* (London, 1809).

[57] Ellen B. Wells (1983, 338, 354, 362, 370).

[58] Harvey (1980) has tallied books in Cavendish's catalog by subject according to whether they were published before or after 1752, the year Henry finished his university education. The results are not very meaningful in the way they are intended. A more useful division for distinguishing Henry Cavendish's interest from his father's is 1783, when Lord Charles Cavendish died.

[59] *Cálidás, Sacontula, or the Fatal Ring, an Indian Drama* (London, 1790). Not entered in the catalog, because it was too late, under poetry and plays but found in the Chatsworth library, with Henry Cavendish's stamp, is the related work, *The Loves of Cámarúpa and Cámalutà, an Ancient Indian Tale*, trans. W. Franklin (London, 1793).

[60] Boswell's Life is listed under "History" in Cavendish's catalog. Boswell dined at the Royal Society Club twice in 1772, both times with Cavendish in attendance. Archibald Geikie (1917, 118).

Theory of Moral Sentiments. We note that his catalog began with astronomy, mathematics, and natural philosophy, subjects which came first in his life.

Often libraries are revealing through their owners' marginalia. It seems that Cavendish rarely put a mark in a book; in the third edition of Newton's *Principia*, he (or someone) penciled in a few numbers, and in a speculative treatise on attracting and repelling powers by Gowin Knight, he (or someone) made a couple of penciled notations.[61] Cavendish's library holds few surprises. It is confirming, not revealing; it tells us that he was interested in the physical sciences and mathematics and not in literature and languages.

Clapham Common

In his scientific calling, Cavendish followed his father, and as an aristocrat who owned houses he again followed his father. As we know, when Charles Cavendish married, he bought a country estate, and if his wife had lived, we might expect him to have continued the familiar living arrangement of a gentleman, having two homes, one in the city and one in the country. Instead, five years after she died, he sold it and bought a townhouse on Great Marlborough Street, so far as we know living the rest of his long life without keeping a second home in the country. His activities were in the city, and he may have felt that as a single man he had no need for a second home, and there may have financial considerations. His oldest son, Henry, also had two homes, his second one coming late in life. Father and son held to patterns of living fairly common among men of their station and means.

In 1785, Cavendish bought a country house on Clapham Common, which would be his main house to the end of his life. Clapham at the time was a straggling village of handsome homes lying in the Clapham parish, about four miles distant from Westminster Bridge in London. When Cavendish arrived, the village had a population of around 2500 and the parish was growing. The best view of the village was from Clapham Common, a triangular piece of ground consisting of 202 acres with houses around its perimeter, lying partly in Clapham parish and partly in a neighboring parish. Twenty-five years before Cavendish moved there, the Common was a morass and the roads were impassable. Chiefly through the efforts of the resident and justice of the peace Christopher Baldwin, the Common was drained and planted with a large number of native and exotic trees, giving it the look of a park. As evidence of the improvement, Daniel Lysons, in his *Environs of London* published in 1792, said that a few years earlier Baldwin had sold fourteen acres of land near his house for £5000, or £357 per acre.[62] The buyer, whose name Lysons did not mention, was Cavendish. Property continued to increase in value; in 1810, the year Cavendish died, Robert Thornton sold his land for £500 per acre.[63] Clapham Common contained many country seats for well-to-do merchants, gentry, and members of Parliament. Cavendish and a woman referred to as "Lady" were the only aristocrats.[64]

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[61] Gowin Knight (1748, 11–12).

[62] Daniel Lysons (1795, 159–161). In the legal documents, the land Cavendish bought is said to be fifteen acres, not fourteen (it is in between). Historically, Clapham Common was common land for two parishes, Clapham and Battersea. Anon., "Clapham Common" (http://en.wikipedia.org/wiki/Clapham_Common).

[63] T.C. Dale (1927, 1).

[64] Map of Clapham Common, with names of all of the residents. "Perambulation of Clapham Common, in 1800. From C. Smith's 'Actual Survey on the Road from London to Brighthelmston,'" in J.H. Michael Burgess (1929, 112). Reproduced by permission of the Bodleian Library.

We first hear of Cavendish's interest in Clapham Common from letters that passed between him and Baldwin beginning in the spring of 1784. Both were members of the Monday Club, which may be where Cavendish learned about the property.[65] The two men met to discuss it, and at the close of a letter following their meeting, Baldwin wrote, "I wish among your other learned & very curious investigations in our atmosphere, you would tell me when I may safely begin hay-making, since you are interested in the attempt."[66] All business, Cavendish paid no attention to pleasantries and flatteries like this, beside which he knew better than to predict the weather.

Baldwin understood that Cavendish wanted to buy three contiguous parcels of land consisting of about fifteen acres adjacent to his house for the purpose of building a house on it. When he was first approached by Cavendish, he said that he was not interested, and he suggested other owners who might sell him land. When difficulties arose with another property, Wright's farm, Cavendish's agent Thomas Hanscomb returned to Baldwin.[67] Baldwin asked Cavendish to tell him what he would pay for the land. When Cavendish said £5000, Baldwin said that it did not meet what he called the "market price." Two of the three parcels of land were choice; the remaining "*front* land" on the Common could not be valued by the acre any more than could land in London or Westminster. Pointing out the beauty, the health, and the convenience of the parcels, Baldwin said that Cavendish should come look at them himself "before it's too late." Baldwin calculated the value for the three parcels separately, the total coming to £5650, which he said was £1280 below the market value. To come up with "a few hundreds more" ought to be no consideration, he said, for "a gentleman of your high rank & well-known great opulence," but Cavendish refused to bargain, and in due course Baldwin accepted his offer.[68] Mortgages on the fifteen acres caused delays in closing the sale until the winter of 1784. The purchase was absolute, the parcels belonging to Cavendish and his heirs and assigns forever. Cavendish named his closest scientific colleagues in London, Blagden, Dalrymple, and Aubert, as trustees to protect the inheritance. Ultimately the money that Cavendish paid Baldwin came from other Cavendishes, and like everything he owned, the Clapham Common property would one day be returned to other Cavendishes.[69]

As it turned out Cavendish did not build a house for himself on the fifteen acres. Instead he entered into an agreement with builders Hanscomb, Richard Fothergill, and Thomas Poynder, who were bound to spend a specified minimum amount of money within a specified time to erect substantial houses with coach houses and stabling. When the buildings were completed, Cavendish would join with them in granting separate leases for the houses, with covenants prohibiting the building of brick kilns or using any buildings on the property

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[65] Verner W. Crane (1966, 215).

[66] Christopher Baldwin to Henry Cavendish, 15 June 1784, Devon. Coll., 86/comp. 1.

[67] Christopher Baldwin to Henry Cavendish, 3 May 1784, ibid.

[68] Henry Cavendish to Christopher Baldwin, n.d. [After 3 May and 2 June 1784], drafts; Christopher Baldwin to Henry Cavendish, 2 and 7 June, 3 July 1784, ibid.

[69] The history of Cavendish's Clapham Common estate is told in a bulky document at Chatsworth, a title search in 1827, beginning with the bargain of sale between Baldwin and Cavendish on 2 November 1784. When Henry Cavendish died, his Clapham Common estate was left to his brother Frederick. When Frederick died two years later his will, which was unchanged, left his real property to Henry. In 1827, Frederick's heir at law William Spencer Cavendish, 6th duke of Devonshire sold the estate. "Abstract of the Title of His Grace the Duke of Devonshire to an Estate at Clapham Common in the County of Surrey," Devon. Coll. 38/78.

as public houses or shops "for carrying on any noisome or offensive trade or business."[70] The land was to be used for up-scale residences, insuring a proper tone. Cavendish arrived at Clapham Common as an eventual land developer and landlord.

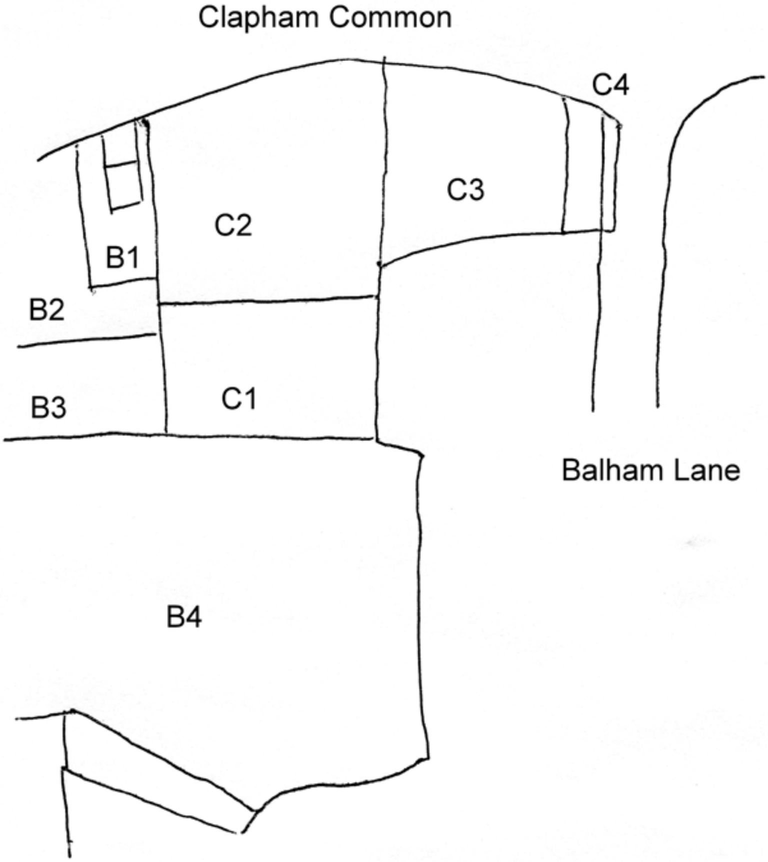

Figure 11.7: Map of Cavendish's Land on Clapham Common . C1, C2, and C3 are three parcels of land, totaling roughly fifteen acres, which Cavendish bought from Christopher Baldwin in 1784. C4 is a slip of land Cavendish bought from Baldwin later. B1 is Baldwin's house and garden. B2, B3, and B4 are fields owned by Baldwin. "Abstract of the Title of His Grace the Duke of Devonshire to an Estate at Clapham Common in the County of Surrey," 2 November 1784, Devonshire Collections, Chatsworth, 38/78.

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[70] "Statement of Leases by the Honourable Henry Cavendish of Messuages and Lands at Clapham in Surrey," 1795–1805, Devon. Coll., 34/10. "Henry Cavendish Esquire and Messrs Hanscomb, Fothergill and Poynder. Articles of Agreement for a Building Lease," 1791, ibid., L/31/45. "Abstract of the Title of His Grace the Duke of Devonshire to the Estate at Clapham Common in the County of Surrey, "ibid., L/38/78. The builders each paid £200 per year rent to Cavendish.

It is unclear exactly what Cavendish's plans were for relocating on Clapham Common. As early as May 1784, at the time he began negotiating with Baldwin over the fifteen acres, presumably to build a house on, he considered buying an existing house, "Mr. Mount's house," which was probably "Mrs Mount's house" on property adjacent to the house that Cavendish bought the following year.[71] What is certain is that his mind was set on moving to Clapham Common, which was not as smokey as London, an advantage when making astronomical observations, and it was healthier, as Baldwin claimed. His house would be a villa, with spacious grounds in a pastoral setting with fine trees, pastures, and ponds, again as Baldwin said. In addition to the peace and quiet of the place and to the privacy it offered, it was also convenient: Baldwin explained that there were good roads, which enabled inhabitants of Clapham Common to travel to London, cross over London Bridge, do business in the city, and return by way of Westminster Bridge, which was no further away from home.[72]

In June 1785, Cavendish bought a house on another side of the Common from his fifteen acres. Perhaps the house became available only after he bought the land from Baldwin. Perhaps its readiness appealed to him, for by buying an existing house he did not have to wait, and he avoided the aggravation of building, allowing him to return to his researches with a minimum of interruptions. It is also possible that Cavendish intended from the start to develop the fifteen acres rather than to build a house for himself there, though it is unclear why he would want to.[73]

His house was three-story, double-fronted, and "symmetrically planned and with a central doorway of typical Georgian design," with considerable grounds.[74] From a plan of the Common in 1800, it appears that with one exception, Cavendish's property occupied the largest frontage of the sixty-odd residences (Fig. 11.12). The lease tells the history of ownership of the house: "Assignment of lease. 18 June 1785. 1. William Robertson of George Yard. Tower Hill, merchant. 2. Henry Cavendish of Bedford Square Esq. Premises on Clapham, for residue of a term of 29 years granted on the experation of a lease of 22 March 1750 made between William Bridges and Henton Brown. Recitals of subsequent assignment. Consideration £3000."[75] Henton Brown is thought to be the first owner of the house, perhaps its builder. We know he lived there by 1748, for that year he requested leave of the vestry to fence a pond he had built on the Common opposite his house, where he kept a pleasure boat. This was Mount Pond, probably at first a gravel pit for road making, the water surrounding an existing mound, to which excavated earth was added, making it higher and improving the view. It was a fashion at this time at Clapham to build summerhouses on viewing mounds, and Brown built one in the pagoda style on top of the Mount to entertain his guests. Brown, an owner of a bank in London, died in 1775, and his bank failed a few

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[71] Henry Cavendish to Christopher Baldwin, n.d. [After 3 May 1784], draft, Devon. Coll., 86/comp 1. Mrs. Mount's house is referred to in Henry Cavendish, "Plan of Drains at Clapham & Measures Relating to Bason," Cavendish Mss Misc.

[72] Baldwin to Cavendish, 3 May 1784.

[73] In favor of this alternative might be his interest in buying Mount's house while he was negotiating with Baldwin about buying the fifteen acres. Also Thomas Hanscomb who dealt with Baldwin as Cavendish's agent would build houses for Cavendish on the fifteen acres.

[74] Clapham Antiquarian Society, "Cavendish House," *Occasional Sheet*, Aug. 1757. Eric E.F. Smith (1976, 78). Burgess (1929, 60). According to the land tax record for 1793, Cavendish owned ten acres. Clapham "Land Tax Assessment for Land Alone June 1793," Lambeth Archives.

[75] Surrey Deeds (Index), Lambeth Archives, 14.171.

years after.[76] A second person named in the lease, William Bridges, was the freeholder or head lessee, who sublet the property to Brown. A third person was a merchant in Surrey, William Robertson, who probably acquired a lease for the property after Brown. In June 1785, he sold his interest in the house to Cavendish for £3000. We see from the document that Cavendish did not buy the house freehold but instead bought a lease for twenty years or so.[77]

Cavendish's house has been called a mansion, but a better description of it from the time is "a tolerable good house, built with red brick."[78] Later owners of the house greatly changed the appearance of the house inside and out, making it difficult to get an idea of the original layout, the number and uses of the rooms, and other details.[79] It was made into a structure that could be called a mansion: among the additions were a magnificent reception room, another servants' wing, a terrace along the garden frontage, and an extension for hanging paintings. At some point, the original red brick central block, the house as Cavendish knew it, was stuccoed over. In 1880, the house was described by an auctioneer as containing "an elegant drawing room, noble dining room, handsome library, morning room and billiard room, a large conservatory, and seventeen bedrooms," and the park-like grounds were similarly sumptuous. Cavendish would have been hard-pressed to recognize the sensible building he made into a house of science in 1785. In 1905, the estate was sold and the house was torn down, replaced by rows of red brick villas.[80] Cavendish Road, originally Dragmire Lane, memorializes the place where Cavendish's house once stood.

We know some of the alterations Cavendish made to the house: from an instrument maker who saw the house, we learn that he converted the drawing room into a laboratory, the room next to it into a forge, and upstairs rooms into an astronomical observatory. A tree behind the house was used as a platform for making scientific observations,[81] and soon

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[76] Before Cavendish's arrival on the Common, a scientific experiment had been performed on Mount Pond by Cavendish's colleague in electricity Benjamin Franklin, who was at the time staying with Christopher Baldwin. Brown's will is in the National Archives, PROB 11/1011/362. Clapham Antiquarian Society, "Cavendish House." Michael Green, "Mount Pond, Clapham Common: Archaeology and History," The Clapham Society Local History Series 7 (http://www.claphamsociety.com/Articles/article7.html).

[77] In our biography *Cavendish* (1999), we said that Cavendish rented his house on Clapham Common. We correct ourselves here: Cavendish bought a lease for the house. I am grateful to Colin Thom for clarifying the purchase.

[78] James Edwards, *Companion from London to Brighthelmston* (London, c.1790), 11. Burgess (1929, 57).

[79] There is a document at Chatsworth that we originally thought applied to the house Cavendish bought at Clapham Common, which if so would give us an idea of the number of rooms in the house and their description. Jungnickel and McCormmach (1999, 326). The inventory is a room-by-room list of bookcases, curtains, stoves, and other fixtures, which were to be valued to the person who bought the estate. Mr. and Mrs. E. Collinson had lived in the house, and the fixtures belonged to Mr. Collinson and Mr. Tritton of Clapham. The name Tritton suggests a connection to Cavendish's house: Anna Maria Brown, daughter of Henton Brown, thought to be the first owner of Cavendish's house, married Thomas Tritton (1717–86); she lived on Clapham Common. The year of the inventory was 1732. In pencil, Cavendish located each room in the house: "west wing back," etc. This inventory is pinned to another inventory of fixtures Cavendish bought from the seller of his house on Clapham Common, William Robertson. The items in the two inventories are different. The earlier inventory was of fixtures in another large house at Clapham, not of the one Cavendish bought, as we first supposed. There is no explanation why Cavendish annotated the inventory and why he kept it with papers about purchases for his house. The puzzling document is "An Inventory of Fixtures Belonging to Messr Collinson and Tritton of Clapham in Surrey to be Valued to the Purchaser of the Estate May 13th, 1732." It is pinned to "An Inventory of Fixtures in the House Purchased by Mr.Cavendish of Mr Robertson." A related document is "Sundry Drawing Room Furniture of Wm. Robertson's Esqr Appraised to Cavendish Esqr. 11th June 1785." The general heading is "About Purchase of House & Furn. at Clapham." Devon. Coll., 86/comp. 1. "Anna Maria Brown," "The Peerage."

[80] Smith (1976, 78).

[81] Wilson (1851, 164).

after Cavendish arrived, he erected an eighty-foot-tall ship's mast, with a horizontal arm, for mounting an aerial telescope.[82] (Figs. 11.8–11.13). This most conspicuous feature of Cavendish's property would have told the neighbors, if they did not already know, that the new resident on the Common was different. He acquired a local reputation as a wizard.

In the middle years of the decade, the 1780s, Cavendish was kept busy moving from one house to another. He explained to Joseph Priestley that a reason he was so long in replying to a letter was that he had been prevented from making any experiments during the summer by "the trouble of removing my house."[83] The move to Clapham was particularly disruptive of his regular life. In June 1785, he postponed the beginning of a journey with Blagden to Wales by three weeks because of repairs to his new house on Clapham Common.[84] In September of that year, Blagden wrote to John Michell, who had invited him and Cavendish to visit him in Yorkshire, that Cavendish "cannot spare time for another journey this year, as it will give him full employment till winter to bring his new country-house of Clapham into order. He is but just removed thither: & all of his pursuits are interrupted till his books, instruments can be brought out of the confusion in which they lie at present."[85] Two months later, Blagden wrote to Laplace that "Mr.Cavendish will not soon have another paper ready, his apparatus having been deranged by moving to another house."[86] Given Cavendish's attachment to scientific activity, his desire to move had to be strong to accept this extended interruption.

In his letter to Laplace, Blagden said that Cavendish would have "conveniences for carrying on his experiments to still greater perfection" in his new house.[87] That may have been, but Cavendish's most important work was done in his first twenty-five years, when he lived behind his father's house in town. If we think of Cavendish's active career as spanning fifty years, 1760 to his death in 1810, his move to Clapham, falls exactly in the middle. Cavendish filled the last twenty-five years of his life at Clapham, as he had the first in the city, with scientific activity, but with the important exception of his experiment of weighing the world it did not make a notable difference to science.

Cavendish sometimes stayed at his house on Bedford Square, and he kept appointments there, but his needs were less than they were at his country house. He employed seven servants at Clapham Common, and an eighth if an instrument maker is counted.[88] He employed only three at Bedford Square, and a fourth if the librarian is counted. His two houses supported the two main activities of his life, reading and research. Complementing one another, his Bedford Square house was about scientific knowledge as recorded in publications, and his Clapham Common house was about scientific knowledge in progress. Cavendish kept his books at Bedford Square and his instruments at Clapham Common, and although the division was not absolute, at the end of his life the value placed on his instruments at Clapham Common was £545 and at Bedford Square nothing.[89]

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[82] Edwards, C*ompanion*, 11.

[83] Henry Cavendish to Joseph Priestley, 20 Dec. 1784, draft, in Jungnickel and McCormmach (1999, 598–599). In 1784, Cavendish would have been moving into his new house on Bedford Square.

[84] Charles Blagden to William Lewis, 20 June 1785, draft, Blagden Letter book, Yale.

[85] Charles Blagden to John Michell, 13 Sep. 1785, draft; in Russell McCormmach (2012, 399).

[86] Charles Blagden to Pierre Simon Laplace, 16 Nov. 1785, draft, Blagden Letters, Royal Society 7:733.

[87] Ibid.

[88] In the executor's accounts, the instrument maker William Harrison is listed with the servants, but in Cavendish's will he is mentioned separately from the servants. Copy of the will, Devon. Coll., L/31/65.

[89] "Extracts from Valuation of Furniture," Devon. Coll.

Other comparisons of Cavendish's two houses reinforce our picture of his life. The value of the furniture in each house was essentially the same, £645 at Clapham Common and £633 at Bedford Square, but his plate, China, and linen at Bedford Square were valued at £700, and at Clapham Common £168.

Clapham Common

Figure 11.8: Cavendish's House on Clapham Common. Demolished. This was Cavendish's country house from 1785 to the end of his life. We see the back of the house, much altered since Cavendish lived there. Frontispiece to *The Scientific Papers of the Honourable Henry Cavendish* (1921g). All rights reserved: Cambridge University press. Reprinted with the permission of Cambridge University Press.

Figure 11.9: View of Clapham Village from the Common. William Thornton (1784, 490).

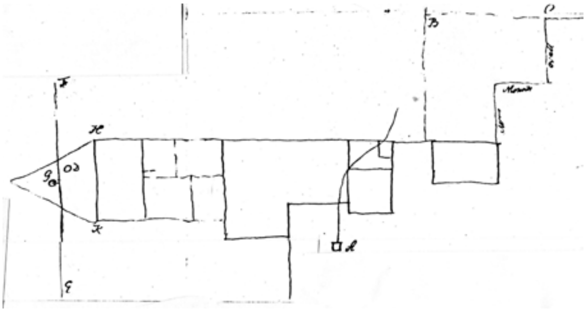

Figure 11.10: Plan of Drains at Cavendish's House. Cavendish's house faces Clapham Common at the bottom of the diagram. The separate building to the right is evidently a greenhouse, formerly containing an outhouse, which Cavendish refers to in his notes on experiments on air. To the left is a basin that becomes a pond, 7½ feet deep, into which the drains from H and K run, and which is filled from the pipe EF, which probably comes from the pond across the road in the Common. G is the valve for letting water into the pond. The other letters stand for: A, a drain sink; B, the gate to the kitchen garden; BC, a drain running from Mrs. Mount's house to the right of what Cavendish has labeled Mrs. Mount's wall; D, a well formerly supplying the pantry or dairy. Water from A eventually runs into a ditch in the field behind the house, and from there it is conducted to the "lane," presumably Dragmire Lane, which bounds Cavendish's property. Next to the pond is a sundial, which Cavendish used as a marker in taking measurements of the basin. Cavendish refers to his walled "courtyard," but he does not indicate its location. This diagram was probably drawn up in connection with renovations Cavendish made before moving into the house in 1785. Cavendish Mss, Misc. Reproduced by permission of the Chatsworth Settlement Trustees.

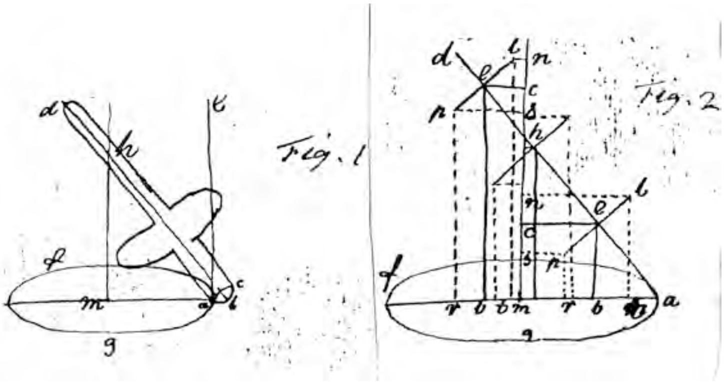

Figure 11.11: Mast for Aerial Telescope. The drawings accompany computations for an eighty-foot-high mast for mounting the Huygens lenses belonging to the Royal Society. Cavendish erected the mast on his grounds at Clapham Common. Cavendish Mss, Misc. Reproduced by permission of the Chatsworth Settlement Trustees.

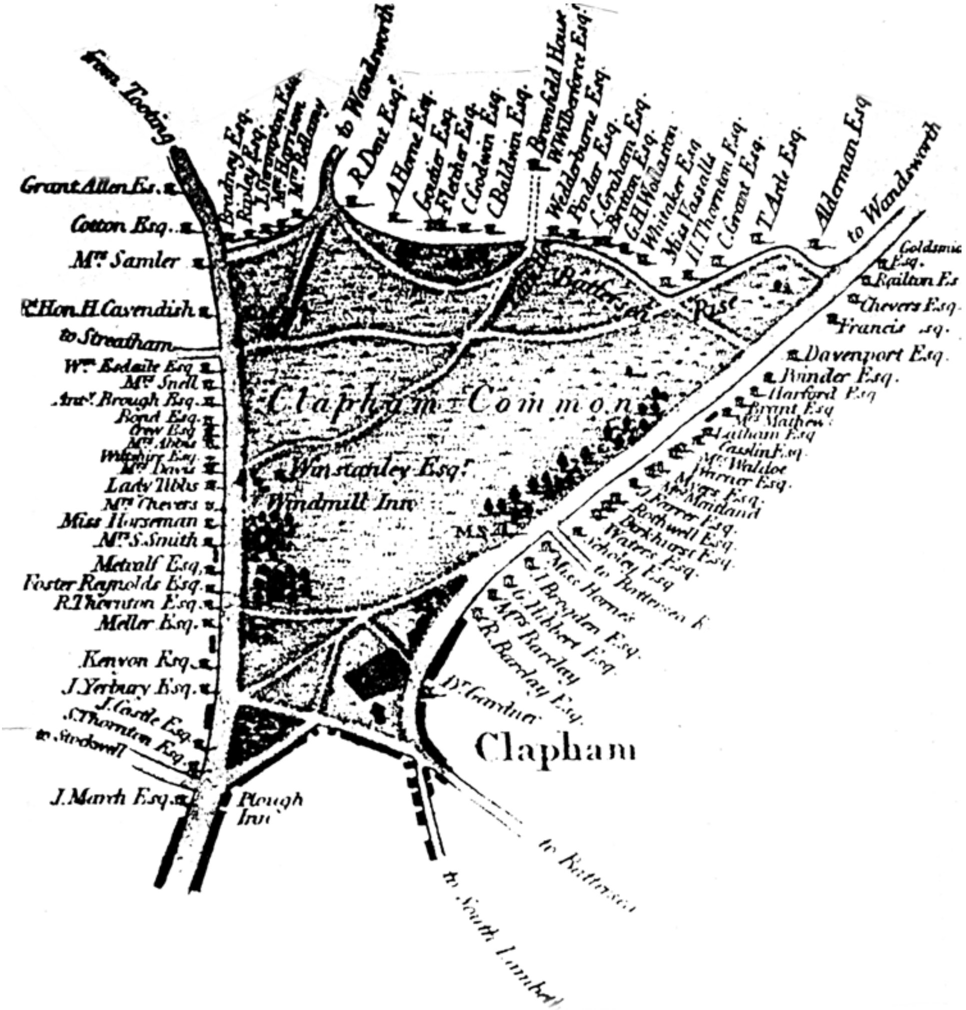

Figure 11.12: Map of Clapham Common. Cavendish's house is on the left side of the Common, fourth from the top. "Perambulation of Clapham Common 1800. From C. Smith's 'Actual Survey of the Road from London to Brighthelmston.'" Burgess (1929, opposite 112). Reproduced by permission of the Bodleian Library.

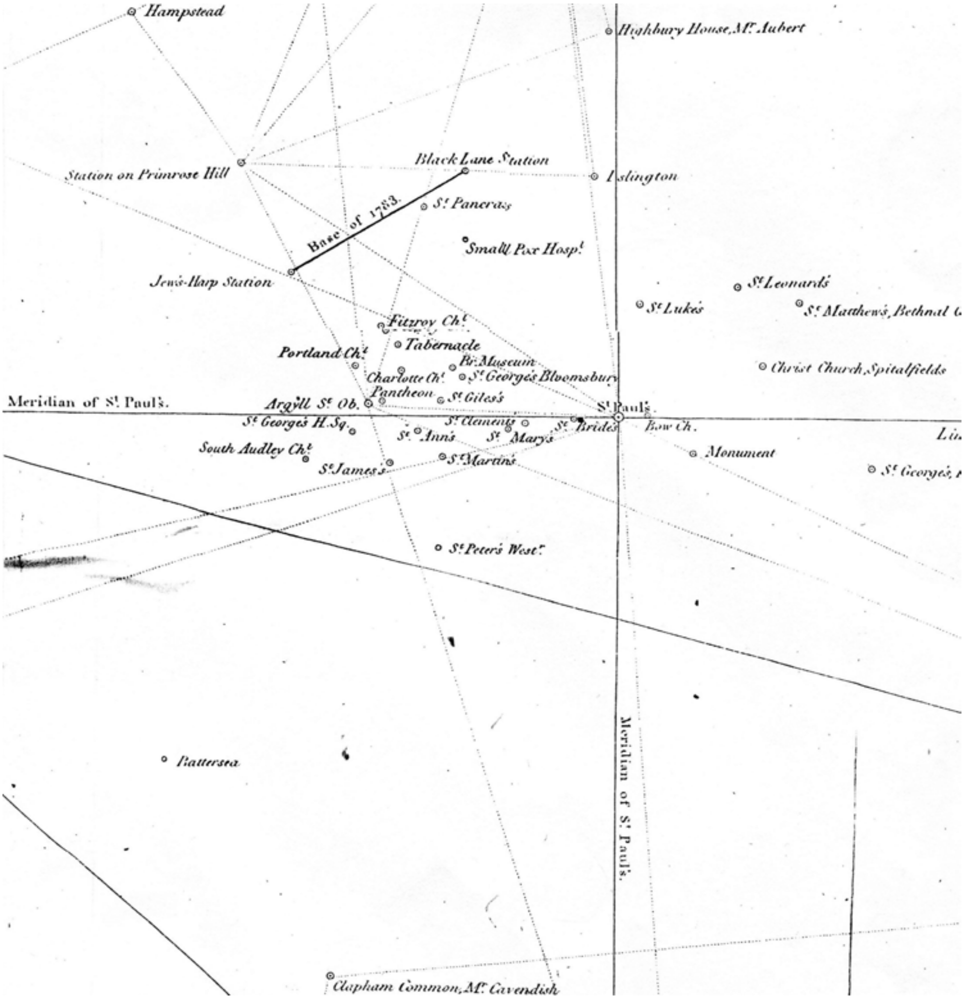

Figure 11.13: Triangulations around London. Triangles measured by the British surveyors showing their starting point, the baseline of 1783 on Hounslow Heath. The purpose of laying down secondary triangles was to improve plans of London and maps of the country. Cavendish's observatory at Clapham Common, shown at the bottom of the map, is one of the stations. Roy's observatory on Argyll Street is shown, as are Aubert's observatories at Highbury House and at Loampit Hill. Greenwich Observatory is just to the right of Loampit Hill, off the map. Detail from a map by Roy, appended to "An Account of the Trigonometrical Operation" (1790).

He kept his small wardrobe and his carriage and harness at Clapham Common, his pictures and wine (twenty-two bottles of port, tokay, and white wine, and ten dozen empty bottles) at Bedford Square.[90]

A map of the places Cavendish could call home reveals a paramount fact about him: he was a city man. When he lived outside of London as an adult, he was no further away than a suburb, probably within sight of the spires of the city. He owned properties in the countryside, but he had no thought of living there. London offered him the civilized amenities and learned company he needed for his chosen way of life. (Figs. 11.14–11.15).

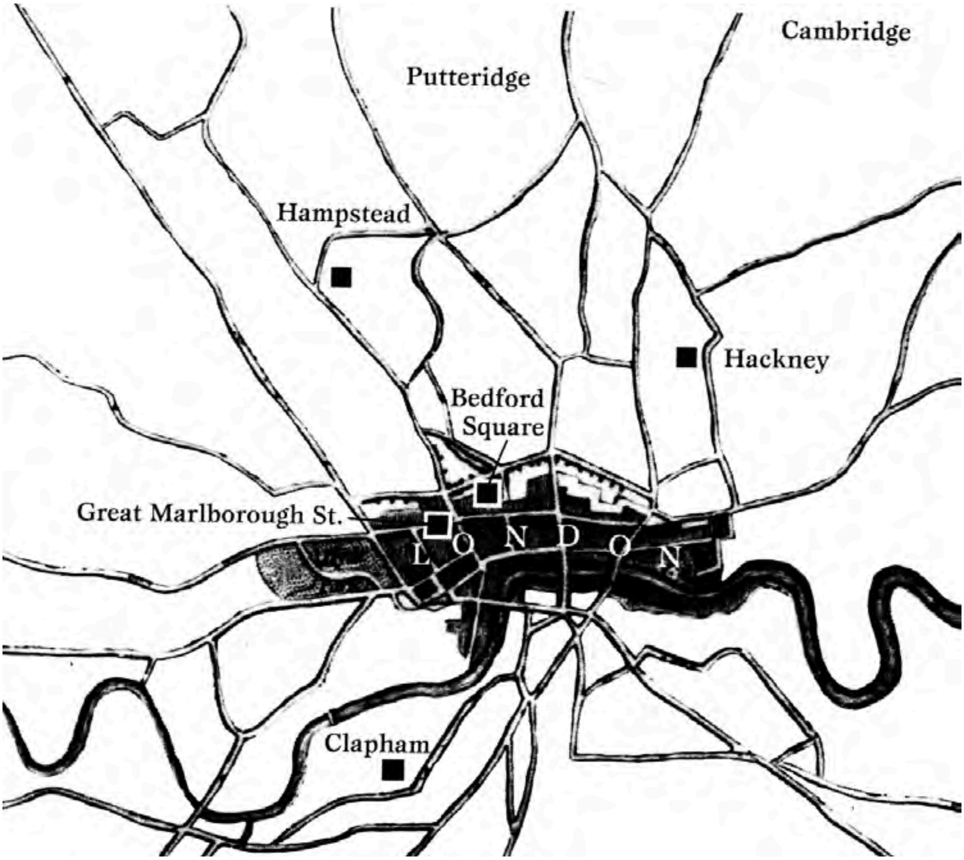

Figure 11.14: Places Where Henry Cavendish Lived. All of the places on this map are mentioned in the book. It shows that although Henry Cavendish did not always live in London, London was never far away.

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[90] Ibid.

Land Developer

Cavendish had further business to settle on Clapham Common. Baldwin owned a small piece of land, about one half acre, lying between the fifteen acres he had sold to Cavendish and Balam Lane, and extending partway onto the lane, which Cavendish wanted to buy to complete his property. Baldwin had incurred legal expenses in connection with it, for which he asked Cavendish to reimburse him, the bill coming to £60. Cavendish offered him £40. Baldwin claimed that he was actually out £124, but Cavendish's lawyer Thomas Dunn told him that Cavendish would file a bill in Chancery against him if he would not take £40. Since Baldwin would then have to file an action against Cavendish to try to recover the rest of his expenses, he could not believe that Cavendish wanted to "go through all this" for a "slip of land." Dunn told Cavendish, "I hope I shall never have any business to transact with such another man as long as I live."[91]

The dispute over the £40 was not yet settled when another problem arose. Dunn had heard that the people of Clapham planned to pull down all of the fencing on the Common and that Baldwin knew about it, in which event Cavendish "must not give him a farthing for the piece of ground," since it encroached on the Common. Learning of this objection, Baldwin wrote to Cavendish, "In my whole life I never was so heartily tired of any thing as I am of the un-meaning correspondence into which I have been drawn by you and your attorney… I am buried in the letters founded in error and ignorance." Baldwin was not going to accept £40, and it was not true that the people of Clapham were going to pull down the fences. It was true, Cavendish told Baldwin; moreover, he was informed that the people of the neighboring parish of Battersea planned to tear down the fences on their common unless the owners paid them a "composition." Cavendish said that he was "so confident" of his information that he was no longer prepared to pay Baldwin the £40, but only £40 less the composition. Baldwin warned Cavendish not to stir up the people of Clapham by spreading the idea of tearing down the fences. Cavendish replied that if Baldwin did not accept his offer, £40 less composition, and make over the rights of the property in two or three days, he would take it as refusal and act accordingly.[92]

Cavendish asked for a "direct answer," but Baldwin's answer was anything but direct. He asked about Cavendish's intention to build a fence between their properties. Even before Cavendish bought the fifteen acres from him, Baldwin had sent him "Hints for Consideration," advising him about building fences.[93] Later Baldwin told Cavendish that his fences were ruined, allowing cattle to enter Baldwin's garden from Cavendish's fields. Baldwin ordered Cavendish immediately to procure the oak pailing for the fence between their properties. The fence, Cavendish replied, "would have been put up long before now if I had not waited till the dispute about the ground taken in from the common was settled." He told Baldwin that he would observe his agreement about the fence "but will not be prescribed to about it nor bear your delays or cavils."[94] He told Baldwin to come to Dunn's on Wednesday

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[91] Christopher Baldwin to Henry Cavendish, 7 July, 19 Sep. 1785; Henry Cavendish to Christopher Baldwin, [July 1785], draft; Thomas Dunn to Henry Cavendish, 6 Sep. 1785, Devon. Coll., 86/comp. 1.

[92] Thomas Dunn to Henry Cavendish, 6 Feb. 1786; Christopher Baldwin to Henry Cavendish, 22 and 27 Feb. 1786; Henry Cavendish to Christopher Baldwin, n.d. [After 22 Feb. 1786], draft, and n.d. [after 27 Feb. 1786], draft, ibid.

[93] Christopher Baldwin to Henry Cavendish, Midsummer's Day, 1784, ibid.

[94] Christopher Baldwin to Henry Cavendish, 8 Feb. [1786]; Henry Cavendish to Christopher Baldwin, n.d. [on or after 8 Feb. 1786], draft, ibid.

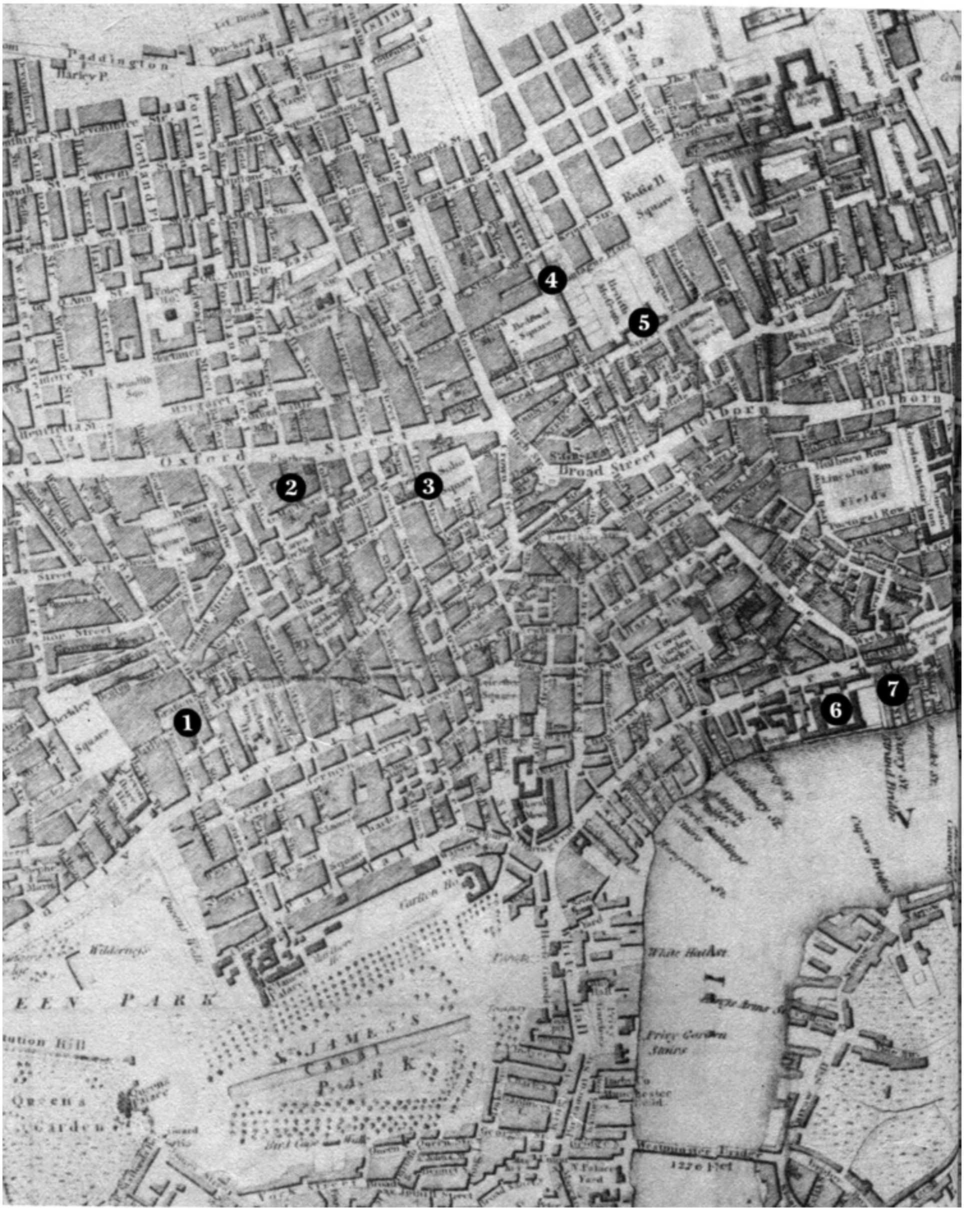

Figure 11.15: Map of Cavendish's London (West End). His familiar destinations in London are identified by numbers superposed on Plan of London, with Its Modern Improvements, published by Richard Phillips in 1808 or 1809.

1. Royal Institution.
2. Great Marlborough Street house.
3. Sir Joseph Bank's house.
4. Bedford Square house.
5. British Museum.
6. Royal Society.
7. Crown & Anchor.

Figure 11.16: Map of Cavendish’s London (East End).
8. Edward Nairne’s instrument shop.
9. George & Vulture.

or Thursday, where he would be waiting to execute the deed. If he did not come Cavendish would give him nothing for the land. Baldwin wrote back asking Cavendish what he meant by saying that he would observe his agreement about the fence. The correspondence between Cavendish and Baldwin came to an end with a flurry of letters, four of them passing between them on one day, the first Saturday in May 1786. Cavendish wrote: "I can not at all conceive what is the cause of this behavior whether you have any private reason for wishing to delay the agreement or whether you distrust my honour about the pailing & wish to make some further conditions about it. If the latter is the true cause you may assure yourself that I will never submit to make any such conditions or explanation with a person who distrusts my honour."[95] A few days later the papers were signed conveying the property to Cavendish.[96] Cavendish's business with Baldwin had taken nearly two years.

Both in the original sale of fifteen acres and in the consequent disagreements over the slip of land, Baldwin misjudged Cavendish. Baldwin thought that money was the issue, and for him no doubt it was, given his large debts. To Cavendish, the matter of Baldwin's legal expenses, £60 or £40 or £40 less composition, was one not of money but of principle. Baldwin's worst error of judgment was to question the honor of Cavendish.

Man of Property

Charles Cavendish owned farms and tithes in Nottinghamshire and Derbyshire, which were his for life as part of his marriage settlement. Living in London, he administered his estate by correspondence with his steward, whose responsibility it was to recommend to him repairs, improvements, and the proper rent to charge; to inform him about the reliability of existing and prospective tenants and what to do when they caused problems, which included eviction; to treat with other landlords and surveyors to settle disputes over enclosures; to influence voting in local elections; and to collect rents. Caught the middle between his distant employer and his tenants, a steward's life was not easy, being required at once to act as pleader, negotiator, spy, and enforcer. Charles's steward was a man named Cotes, who had come with a weighty recommendation from the "Archbishop." This prelate might have been the archbishop of Canterbury, who like Charles was a conscientious trustee of the British Museum, but we suspect he was the archbishop of York, who received money from Cavendish for paying pensions due from the rectory in the parish of Arnold. Cotes was healthy at the beginning, but he soon began to decline irreversibly. Cavendish perceived the "decay of his understanding for some years" without, however, taking steps. "Out of tenderness," and perhaps also with due respect to the archbishop, Cavendish "could not dismiss him abruptly." He wanted Cotes to resign instead, which Cotes eventually did, in 1764. In his place, Cavendish hired Thomas Revill, a choice he almost immediately regretted but which he nonetheless lived with for almost twenty years.[97] Revill abused his predecessor and evidently Cavendish's tenants as well, and Cavendish came to regard him as a "peevish

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[95] Christopher Baldwin to Henry Cavendish, 3 Mar. 1786, Saturday [1 Apr. 1786], Saturday [1 Apr. 1786]; Henry Cavendish to Christopher Baldwin, 1 Apr. [1786], n.d. [1 April 1786], drafts, ibid.

[96] Baldwin deeded to Cavendish the one half acre of land "abutting or bounding" Balam Lane. On the same day he released all claims on the fifteen acres he sold to Cavendish, for a consideration of £80. Christopher Baldwin to Henry Cavendish, 5 Apr. 1786, "Lease" for the one half acre. Christopher Baldwin to Henry Cavendish, 6 Apr. 1786, "Release of a Piece of Land on Clapham Common." Christopher Baldwin to Henry Cavendish, "General Release," for a consideration of £80. Devon. Coll., 38/78.

[97] Charles Cavendish to Thomas Revill, 5 Sep. and 13 Dec. 1764, draft, Devon. Coll., L/31/20.

old man," who created more problems than he solved. Two words appear with impressive frequency in Cavendish's half of their argumentative correspondence, "justice" and "reasonable," positive words he never applied to his steward but to actions his steward did not take and should have.[98]

Charles introduced his eldest son to business as he had to science, turning over the management of his estate to him in the summer of 1782. Charles did not yet formally make it over, and he continued to participate in its management,[99] but he allowed Henry to receive the income from rents, tithes, and land taxes, which came to around £1600 a year. In his manner of handing over responsibility, Charles repeated his own experience, his father having turned over the rents to him in the first year and in the second year the property itself. From Henry's point of view, it was time to begin leaving home; land, we suspect, was equated by him with independent living.

Henry Cavendish's activity as an absentee landlord gives us insight into his person. Like his father, he had first to settle on a steward. Unsatisfactorily as he had worked out, Revill had an extenuating circumstance, which he had explained to Charles Cavendish. Because of a problem with his throat, he could scarcely speak and was reduced to communicating by writing, though he was helped in his work by a nephew.[100] Revill's attitude, a mix of servility and arrogance, was exasperating, but his difficulty in speaking no doubt helps explain the roundabout way he went about his work. His new master Henry Cavendish, who himself had difficulty in speaking, evidently felt no bond of sympathy, neither making nor accepting excuses for Revill's lapses.

The duke of Devonshire was well served by his agent J.W. Heaton, who recommended William Gould for steward, citing his "integrity and judgment on country business."[101] Cavendish settled on Gould as his new steward before he fired his father's steward. Revill had already written that he wanted to collect the next rents, and when Cavendish told him not to because he intended to replace him, he protested. In his reply Cavendish said that he would not have answered him at all but for Revill's concern that his reputation would suffer. There was no cause for such concern, Cavendish said, since it was "so natural" for someone taking over an estate to entrust it to a steward whose judgment he could rely on. If, however, any doubts about his reputation were to arise on this account, Cavendish would direct his new steward to set matters right. Cavendish had meant to end the letter there but changed his mind, adding that although he had no doubt of Revill's fidelity and good intentions, he had good reasons for deploring his actions: "the infirmity of your temper which has made you either quarrel or behave with petulance to so many of those you have had business with & the little information my father could ever get from you concerning the matters under your charge render you very unfit a person to take care of an estate without which cause I should never have thought of employing another steward." To his new steward, Cavendish mentioned Revill's "angry letter," copying out part of his reply to Revill, only in place of "infirmity" of his temper substituting his father's expression, "the peevishness of his temper." For a full year, Revill wrote repeatedly to Cavendish to complain of his firing. Cavendish

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[98] Charles Cavendish to Thomas Revill, 19 Sep. and 3 Dec. 1776, 12 Apr. 1777, 18 Mar. 1778, drafts; Thomas Revill to Charles Cavendish, 31 Jan. 1765, Devon. Coll., L/31/20 and 34/5.

[99] Henry Cavendish to William Gould, 30 Dec. 1782, draft, Devon. Coll., L/34/7.

[100] Thomas Revill to Charles Cavendish, 16 Dec. 1764, Devon. Coll., L/31/20.

[101] William Gould to J.W. Heaton, 10 June 1782. Heaton forwarded this letter to Cavendish, adding his recommendation of Gould. Henry Cavendish to William Gould, 8 and 9 Aug. 1782, drafts, Devon. Coll., L/34/7.

neither answered his letters nor entered them in the index of his correspondence. The standard by which Cavendish judged Revill unfit he held up to his replacement. Gould was to give Cavendish's tenants no cause to complain, and he was readily to give Cavendish any and all information he desired. The first item of business was for Gould to make a complete examination "into the condition of the whole estate."[102]

In Nottinghamshire and Derbyshire, the Cavendish family had long counted among the big landlords who bought out the landed gentry and took over their manors.[103] Charles and Henry's properties were in the neighborhood of the duke of Devonshire's, from which they had been separated off.[104] The duke's main country house was in the region, at Chatsworth in Derbyshire. Family estate records were kept Hardwick Hall, nearby in Nottinghamshire, where Henry Cavendish directed his steward to examine documents.[105] The Cavendish family kept in touch on matters of property. When one of Henry's properties became available a prospective tenant approached him through his first cousin John Cavendish.[106] When pending legislation affected his estate, Cavendish was assisted in Parliament by his principal heir George Augustus Henry Cavendish. Physically, legally, and politically, Henry Cavendish's properties were in the family.

Under the old pattern of farming, tilled land was parceled into strips with mixed ownership, and pastures were subject to common right. To meet changing economic needs, this pattern was replaced by one in which strips were consolidated and common use of land was reduced; the device was called enclosure.[107] The practical intent of enclosure, as Charles Cavendish put it with his usual clarity, was to "lay each person's allotment together as much as can be."[108] If landowners could not agree on enclsure an act of Parliament was required to overcome local resistance. Enclosure was a costly improvement: landowners were out the expense of passing the act; fees for lawyers, surveyors, and commissioners, whose job was to carry out a survey, place the owners' allotments in enclosed fields, see to it that improvements specified in the act were built, and look into damage claims; and a very considerable capital investment in fences, drains, roads, and various structures.[109] Because substantial economic gains could be expected from enclosure, big landlords and farmers were for enclosure. Charles and Henry Cavendish were not big landowners, and they could not avoid conflict.

Their property in the parish of Arnold in Nottinghamshire is an example of the complications attending enclosure. Charles Cavendish was entitled to tithes from the use of the land at Arnold, from which he received rent twice yearly, the total of which, a little over £100, made Arnold intermediate in value among his properties. In the event of enclosure, Cavendish would be expected to forfeit his tithes in exchange for an allotment of land. Just how much and what kind of land were the question.

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[102] Henry Cavendish to Thomas Revill, 16 and 28 Aug. and 5 Sep. 1782, drafts; Henry Cavendish to William Gould, 6 Sep. 1782, draft, Devon. Coll., L/34/7.

[103] J.D. Chambers (1966, 7).

[104] For example, Cavendish received rent from the tithes of Marston in Derbyshire, the greater part of which parish was owned by the duke of Devonshire. William Gould to Henry Cavendish, 28 Sep. 1782, Devon. Coll., L/34/7.

[105] Henry Cavendish to William Gould, 2 Dec. 1787, draft, Devon. Coll., L/34/7.

[106] William Gould to Henry Cavendish, 20 Aug. 1785; Arundall Gallway to Henry Cavendish, 21 Aug. 1785; Pemberton Milnes to John Cavendish, 24 Aug. 1785; John Cavendish to Henry Cavendish, 25 Aug. [1785]; Henry Cavendish to John Cavendish, n.d. [reply to letter of 25 Aug. 1785], draft, Devon. Coll., L/34/7.

[107] Chambers (1966, 141).

[108] Charles Cavendish to Thomas Revill, 8 [9] Dec. 1776, draft, Devon. Coll., L/34/5.

[109] William Gould to Henry Cavendish, 25 Mar. 1784, Devon. Coll., L/34/7. Chambers (1966, 78, 199–200).

In 1776, the proprietors at Arnold considered petitioning Parliament to enclose their land. The quantity of land and the amounts given over to different uses were imprecisely known, since there had been no survey. Proceeding from incomplete information Revill made proposals to the proprietors about what share of the common fields and forest Charles Cavendish should receive in return for giving up his tithes. Revill's proposals were ill-received by the proprietors, whose spokesman repeatedly appealed to Cavendish and brought their grievances to him in person. Cavendish wanted the proprietors to deal with Revill, but they objected to him even more than they did to his proposals. Because of the animosity between the people at Arnold and Revill, agreement looked all but impossible. Revill asked for more than was "just," Cavendish said, and he urged reason and negotiation.[110] The matter of the Arnold enclosure languished, but several years later, it came up again in the form of a petition for a bill. Having just taken charge of his father's farms, Henry Cavendish inherited a local history of bad feeling.[111]

Henry's new steward told him that recent enclosures had been "attended with great detriment and injury to the estate," by which he meant not the unavoidable "great sums that have been expended on those Inclosures and the Buildings upon them" but the avoidable, absolute loss in the value of the estate owing to the previous steward's inattention. Cavendish entered into a long negotiation with the proprietors at Arnold over the amount of land he was entitled to receive in lieu of tithes, his father's quandary. In principle, the land he was entitled to receive was equivalent in rental value to the tithes he would have received from the improved land after enclosure, but the comparison of values was not straightforward. Depending on how it was figured, either the farmers or Cavendish benefited more. The proprietors took the initiative, offering Cavendish a specified allotment of land to compensate him for the loss of his tithes. Gould calculated the rent Cavendish would receive from the allotment, deducting the interest he would pay for putting up fences and buildings and for vicarial tithes, which he would go on paying, coming out to £169 per year, far below the £250 Gould estimated that Cavendish's tithes would bring. Because of the expenses Cavendish would incur, Gould advised him not to accept an allotment of yearly value less than £360. It was fair, Gould said, but the proprietors would not like it.[112] Cavendish did not like it either. If a specific monetary value were proposed, he told Gould, he would come out a loser, because the commissioners routinely overvalued land; instead he wanted them to allot him a certain "proportion" of land, a surer measure of value than money.[113] Gould then wanted to select the location of the allotment on the forest. Cavendish thought that he was being overly zealous, making unnecessary trouble for the commissioners, who might be "less disposed to do me justice," but otherwise he accepted the proportion Gould had calculated for him. The proprietors rejected the proposal; their spokesman said the land would rent for £500, and he knew a man who would pay it. The spokesman complained about Cavendish's steward, who refused to answer letters, attend parish meetings, or receive a delegation, exhibiting "all the insolence of delegated authority."[114] Gould, if the proprietors were to be believed, was behaving like Revill. Cavendish did not mention the proprietors'

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[110] Charles Cavendish to Thomas Revill, 3 and 12 Dec. 1776, drafts, Devon. Coll., L/34/5.

[111] Gould forwarded the petition from Arnold in a letter to Cavendish, 28 Sep. 1782.

[112] William Gould to Henry Cavendish, 24 Nov. 1784, Devon. Coll., L/34/7.

[113] Henry Cavendish to William Gould, Dec. and 24 Dec. 1784, drafts, Devon. Coll., L/34/7.

[114] Henry Cavendish to W. Sherbrooke, 6 Jan. 1785, draft; W. Sherbrooke to Henry Cavendish, 3 and 18 Feb. 1785. Cavendish also received an anonymous letter from a landholder in Arnold complaining of Gould, Mar. 1785, Devon. Coll., L/34/7.

complaints to Gould, which in any event could hardly have been news to him. He wanted Gould to get more exact information on acreage, rents, and tithes, for only then could they "prove" that their proposals were not "unreasonable." Fairness in this matter depended on reason, facts, and calculations, even though the quantities involved could be no firmer than estimates. Cavendish told Gould that justice all around would be served only if his "estate should be improved in the same proportion as that of the land owners,"[115] and his duty to his estate was to ensure that it received this proportion.

The "affair of Arnold," as Cavendish called it, dragged on for years.[116] Early in 1789 Gould told Cavendish that enclosure was likely, but a little later he corrected himself; it was unlikely because the vicar wanted more for his tithes on turnips and lambs than the proprietors offered him, and the vicar was a hard bargainer. Gould then learned that the landholders intended to go to parliament without the vicar, leaving the new allotments still subject to vicarial tithes, which meant that Cavendish would have to be given additional land equal to the tithes he must pay the vicar. The amount in question came to around £15 a year. Cavendish pressed Gould for facts on the vicar's turnip tithes, so that he could decide what "part of the turnips are tithable." Cavendish wrote sternly to Gould for not having "explained the matter to me clearly." Gould had given him his recommendations about the turnip tithes without at the same time giving him his "reasons." Henceforth Gould was always to give Cavendish his "reasons."[117] Cavendish was not a miser; the money £15 was trifling. The matter of the vicar's turnip tithes had to do with the way his mind worked: in making decisions about his estate, Cavendish needed "reasons."

In its own good time, the Arnold affair came to a close. Following upon the petition, on 2 March 1789 the enclosure bill was ordered, setting in motion an elaborate parliamentary procedure, which was concluded with the royal assent on 13 July. With the exception of two proprietors, all of the parties gave their consent to the bill.[118] For Cavendish, as no doubt for the other landowners, the news from Arnold would be bad before it was good again: in the summer of the following year, Gould told Cavendish that he had collected the rents from all but two of Cavendish's tenants but he was not remitting them because the entire amount was expended in the Arnold enclosure.[119]

Cavendish's early correspondence with his steward shows him to be new to the business. Once the farms were his responsibility, he set out to acquire a total familiarity with the facts of his estate, from which he reasoned on the basis of general principles, including the principle of justice, to conclusions about actions to take. In his approach to the management of his business, we recognize traits we have come to know in the natural philosopher.

Cavendish had a busy life in London with absorbing interests of his own choosing. From the questions he asked of his steward, we get the impression that he never visited his farms. He was burdened with landed property that was far away and that gave him trouble for a relatively small income he did not need. His steward sent him enclosure bills to study, and because he owned many properties, these bills repeatedly demanded his attention. With

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[115] Henry Cavendish to William Gould, 23 Feb. 1785 and n.d. [after 28 Feb.] 1785, drafts; Henry Cavendish to W. Sherbrooke, 16 Feb. 1785, draft, Devon. Coll., L/34/7.

[116] Cavendish to Gould, 2 December 1787.

[117] Henry Cavendish to William Gould, n.d [reply to letter of 21 Feb. 1789], draft, Devon. Coll., L/34/12. William Gould to Henry Cavendish, 19 Mar. 1789.

[118] William Gould to Henry Cavendish, 30 Mar. 1789, Devon. Coll., L/34/12. 2 Mar., 13 May, and 12 June 1789, *Journal of the House of Commons* 44:138, 361, 454, and 456.

[119] William Gould to Henry Cavendish, 5 June 1790, Devon. Coll., L/34/12.

regard to an enclosure that had been pending for two years, Cavendish wrote to Gould: "You ought to have informed me of it at the time instead of delaying it till lately & then representing it to me as brought in by surprise & without your knowledge [.] I am very sorry to find that you could act in this manner & hope I shall never see another instance of any thing of the kind."[120] Cavendish suffered irritations like this because they came with his life and its responsibilities. If managing his estate brought no joy, we trust it brought satisfaction of a kind, the performance of a duty. No matter how far his activities in science took him away from his family, in his occupation with landed property he was with them.

Places and Precision

The preceding sections have shown the importance of places in Cavendish's life, and in other sections we have seen the importance to him of accuracy and precision. Here we bring the two together. The occasion was an Anglo-French project to determine accurately the relative locations of the Royal Observatory at Greenwich and the Royal Observatory in Paris. The "astronomical" difference of longitude between the two observatories had been determined by the time difference between the two locations, with an uncertainty said to be as great as eleven seconds, which corresponded to 1700 fathoms, a large distance. More reliable than astronomical observations was a "terrestrial" operation based on "triangulation," by which the longitudes of the two observatories, as determined by astronomical measurements, could be corrected. In 1783 the director of the Paris Royal Observatory C.-F. Cassini de Thury proposed to George III that a series of triangles be laid from London to Dover, there to connect with triangles already executed in France. The proposal was passed to Joseph Banks, who replied that the Royal Society had "people enough [...] capable and willing." The Royal Society recommended to the king a larger project, which in addition to the longitudes of the royal observatories would include a survey of all of Britain corresponding to Cassini's already completed map of France. This survey would be made in the 1790s, but in the meantime the lesser project of connecting the two observatories was undertaken.[121]

Banks recommended a fellow of the Royal Society to head the English half of the project, William Roy. Close in age, Roy and Cavendish came together frequently at the Royal Society Club, where we assume they talked about their common penchant for accurate measurement. Roy's successive appointments tell us the kind of technical servant he was: Surveyor-General of the Coast, Engineer of Military Surveys for Great Britain, and Director and Lieutenant Colonel of Royal Engineers. He brought considerable experience to the Anglo-French triangulation project, having helped to make a military map of Scotland after the Jacobite rebellion in 1745, and having proposed a national survey on the grounds of national defense after the Seven Years War in 1763. For a time the government seriously considered his proposal, which would have built on the map of Scotland, but dropped the plan because of the expense. After peace with America was declared in 1783, Roy made small

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[120] Henry Cavendish to William Gould, 12 May 1789, draft, Devon. Coll., L/34/12. Gould defended himself against Cavendish's "severe reprimand" and gave his reasons. William Gould to Henry Cavendish, 20 May 1789, Devon. Coll., L/34/12.

[121] 29 June 1787, Minutes of Council, Royal Society 7:276. William Roy (1787, 213–214; 1785, 389). Charles Coulston Gillispie (1980, 122–123). In 1784, the elder Cassini died, succeeded as director-general of the Paris Observatory by his son Jean-Dominique Cassini, who was appointed by the Paris Academy of Sciences to superintend the French half of the project. He renovated the Observatory, procured new instruments, and oversaw the joint Anglo-French operations.

triangulations in the London area, determining positions of steeples and other prominences relative to each other and to the Greenwich Observatory "to facilitate the comparison of the observations, made by the lovers of astronomy" and to revive the plan of a national survey. He was engaged in writing up this work when Cassini de Thury proposed a triangulation of southeast England, an opportunity for which he had been preparing himself for twenty years. The "chief and ultimate purpose" of measuring a base on Hounslow Heath, as Roy understood it, was "the laying the foundation of a general survey of the British Islands."[122]

Believing that instruments of sufficient precision for the project did not yet exist, Roy said that it would be necessary to "reinvent them all." Principal among them was a theodolite built by Jesse Ramsden, a perfectionist, who kept the operation on hold while he worked on it, endlessly it seemed to Roy. The 200-pound instrument was fitted with a three-foot circle, which made it highly accurate, allowing a mark seventy miles distant to be read with an error of only two seconds of arc. Roy said that with it, angles would be measured "to a degree of precision hitherto unexampled."[123]

On 16 April 1784 Cavendish, Banks, and Blagden met with Roy on Hounslow Heath near the Greenwich Observatory to begin preparing a site for the baseline of the triangulation. Because the measurement of the baseline was critical, "infinite pains and care" were taken to see that it was accurate. Accurate bases had been measured in other countries with deal (fir) rods, and Roy intended to use them on Hounslow Heath, though Ramsden provided a choice of instruments, glass rods and steel chains in addition to deal rods. For the wood, Banks applied to the Admiralty, which cut up two masts. Ramsden finished making the roughly twenty-foot-long deal rods on 15 July, and on 16 July Cavendish met with Roy, Banks, Blagden, Smeaton, and Lloyd to start taking measurements with them. Although "extraordinary care" had been taken in the contraction of the rods to ensure that they were "the best which had ever been made," it was found that their length varied with humidity, seriously interfering with the precision of the measurement. The rods were accordingly replaced by a 100-foot steel chain and again by glass rods or "tubes," which despite their great length were so straight that one could see a small object on the axis of the bore at the other end. With the help of a pyrometer with microscopes attached, equations were derived for the expansion of the rods with temperature. Roy was awarded the Royal Society's Copley Medal in 1785 for this accurate work.[124]

From Hounslow Heath, triangles twelve to eighteen miles on a side were set out on a southward course to the coast, the terrain dictating a snake-like progression. The baseline was used for about half of one side of the first triangle. From then on, only angles were measured until the last triangle, which was measured by a second baseline of "verification," laid out on Romney Marsh on the southern coast. To judge the "accuracy" of their operation in determining angles and sides, they found the "error" between the length of one base as computed from the other base and the length of the same base as measured on the ground to be within a few inches. The triangulation was, Roy said, "an instance of exactness as probably never occurred in any former operation of this sort." From the English coast, observations were made to "intersect, with great accuracy," two points on the French coast,

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[122] Yolande O'Donoghue (1977, 41). Roy (1787, 188).

[123] Roy (1787, 188). Charles Blagden to William Farr, 22 Aug. 1787, draft, Blagden Letters, Royal Society 7:346. Charles Blagden to Dr. William Watson, 22 Aug. 1787, draft, ibid. 7:347. Gillispie (1980, 123).

[124] William Roy (1790, 116, 121, 133; 1785, 391, 394, 425, 430, 441). O'Donoghue (1977, 46).

establishing a "triangular connection between the two countries." Roy hoped that Cavendish would join him there, as he planned to do, but he put off the trip because of bad weather.[125]

The second baseline had been measured with steel chains, which were easier to work with than glass rods, and which had been proven accurate on the first baseline. After Roy's operation, there was some discussion of the error in measuring the two bases by different means, glass rods at Hounslow Heath and steel chains at Romney Marsh. On the principle that every base ought to be "measured twice at least," in 1791 the baseline at Hounslow Heath was ordered re-measured using steel chains instead of glass rods. The new measurement differed from the original by less than three inches. The object by then was a general survey of Britain which was Roy's goal, though he did not live to see it.[126]

Roy's plan did not make use of the conspicuous landmark St. Paul's Cathedral as a station of a triangle because he would have needed to make Hampstead and Harrow stations too, all three of which were inconvenient for what he called the "great instrument," the theodolite. There were other problems with those stations too such as the "smoke of the Capital." In fact, none of the stations Roy used were inside London, though from the stations outside, he could determine accurately the locations of St. Paul's, Hampstead, Harrow, and many other places with steeples within the city. Independently of the Anglo-French project, from the baseline on Hounslow Heath in 1788 and 1789 Roy laid down "secondary triangles" with the object of improving plans of London and maps of England. Cavendish's observatory on Clapham Common was one of the stations, as were Aubert's and Roy's observatories and Maskelyne's Greenwich Observatory (Fig. 11.13). Roy computed the latitude, longitude, and bearing of Cavendish's "Clapham Common, Transit-room":

- Latitude 31° 27' 12.7".
- Longitude from Greenwich 0° 8' 39.2". In time, 0^h 0' 34.613".
- Bearing from the center of the dome of St. Paul's, from south meridian westward 26° 29' 56.1".
- Distance in feet 24563.5.

Commenting on these numbers and those for several other places, Roy said that because he had the best instrument and a better way of measuring bases, the "relative geodetical situations of the stations [...] may be said to be free from sensible error."[127] Knowing Cavendish's desire for accuracy and precision, it is fitting that his principal home was a geodetic datum, angles expressed to a fraction of a second.

There was a problem. While preparing sheets of Roy's final paper for the *Philosophical Transactions*, Blagden discovered numerical "blunders," which he pointed out to Roy, who proceeded to find more on his own. Roy's health was poor, and while he was absorbed in the heavy task of discovering and correcting his errors, on 1 July 1790 he died at his house in

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[125] Roy requested a British commissioner to join the French commissioners in making measurements across the Straits of Dover, proposing Blagden, who was appointed by the Council of the Royal Society. O'Donoghue (1977, 1, 41). Joseph Banks to Charles Blagden, 13 Oct. 1783, Blagden Letters, Royal Society, B.19. Charles Blagden to Joseph Banks, 12 July 1784 and "Tuesday" [1784], Banks Correspondence, Royal Botanic Gardens, Kew, 167, 171. Charles Blagden to Henry Cavendish, 16 Sep. 1787; in Jungnickel and McCormmach (1999, 634–635). Henry Cavendish to Charles Blagden, [after 16 Sep.1787], draft; ibid., 638–640. Charles Blagden to Benjamin Thompson, 27 May 1787, draft, Blagden Letters, Royal Society 7:55. Charles Blagden to Dr. William Watson, 22 Aug. 1787, ibid. 7:347.

[126] Edward Williams, William Mudge, and Isaac Dalby (1795, 417–418). O'Donoghue (1977, 1, 42).

[127] Roy (1790, 260–261).

London. Roy took pride in his work as a military engineer, the aim of which was accuracy and precision,[128] earning a solid reputation in a field that had no tolerance for careless error. He regarded the triangulation under his direction as "infallible, because, by means of the base of verification, it will prove itself." The accuracy of it was a point of national and professional honor. Roy's is a case of the gods striking down one they love.

There was concern that more errors lay hidden in the paper, which would be (triumphantly) discovered by the French commissioners of the project, especially by P.F.A. Méchain, who was bound to read the paper carefully. Had Roy's errors been limited to the 1787 paper, they would not have been damaging, since it was only a sketch of the operation to come, but errors in the 1790 paper were another matter, since it was the final report, and the operation was an official undertaking of the Royal Society. Blagden turned for advice to one of the Society's experts on errors. "Conversing a few days ago on this subject with Mr. Cavendish," Blagden told Banks, "he suggested, that the best way of preventing any disgrace which might fall upon the Society on this account would be, to get the paper well examined here, and print such errors as might be discovered in the *errata* to the present volume of the Transactions, thereby anticipating, as far as possible, the remarks of foreigners."[129] Roy would have recommended the same course. At a time when the French triangulation had been criticized in Russia as "extremely erroneous," Roy had expressed confidence that the Paris Academy of Sciences would, "no doubt, vindicate the credit of their own operation."[130] To vindicate its own, the Royal Society acted as Cavendish proposed. Roy's assistant Isaac Dalby, in Roy's words "an able and indefatigable calculator," was recommended by Blagden to examine the paper for errors. After meeting with Dalby, Blagden reported to Banks that "he said there were to his knowledge very many blunders retained by the General, though clearly pointed out to him." Dalby doubted that it would look right if he were the one to point out the errors, but Blagden told him "to put himself in the place of a foreigner, whose object it might be to criticize as severely as possible." Blagden said that they would "take care to present the result to the public in the tenderest manner for the General's reputation, consistent with our duty to the Society," and Dalby then agreed.[131] In an appendix to Roy's posthumous paper, Dalby went through the paper page by page, noting where corrections were in order.[132] In a second appendix, Blagden gave a brief personal account of Roy in which he offered a partial excuse for his errors. After finishing the triangulation in September 1788, Roy devoted what time his health and his military duties allowed him to preparing his paper. Advised to go to Lisbon in the winter of 1789, he hurriedly finished the paper, and when he returned in April his paper went to press. At the time he died he had corrected the sheets but he had not compared the manuscript with the original observations.[133]

The errors came about this way. Roy regarded the triangulation operation as relevant to the long-standing problem of the figure of the Earth. He made calculations for a number of assumed hypotheses about the Earth, finding good agreement between theory and observation with Bouguer's hypothesis of a spheroidal Earth. Roy made three kinds of mistakes

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[128] "Roy, William," *DNB*, 1st ed. 17:371–373, on 373. Sven Widmalm (1990, 199).

[129] Charles Blagden to Joseph Banks, draft, 31 Aug. 1790, BL Add Mss 33272.

[130] Roy, "Account of the Mode Proposed," 211.

[131] Blagden to Banks, 26 Sep. 1790.

[132] Charles Blagden, "Appendix," to Roy (1790). Isaac Dalby, "Remarks on Major-General Roy's Account of the Trigonometrical Operation, from Page 111 to Page 270 of This Volume," ibid., 593–614.

[133] Charles Blagden, "Appendix," to Roy (1790). Blagden's and Dalby's appendices were printed at the end of volume 80 of *Philosophical Transactions*.

in calculating lengths of arc, and these were explained in a footnote to the paper. At this stage all that could be done about the errors was to annex a corrections slip to the paper. The mistakes did not invalidate the general reasoning of the paper, since the only purpose of the computed lengths of arc was to show that Bouguer's hypothesis agreed better than other hypotheses with the actual measurements.[134] Roy made errors, but he was also wise about errors. Experiments "rarely leave no room for doubt," he wrote on another occasion; different experimenters using different instruments and different methods arrive at different results, and it is "not until things have been viewed in every possible light, that the errors, even of our own experiments, are discovered."[135] He could have been describing Cavendish's practice.

Errors haunted Roy's publication. In a paper the following year giving measures deduced from Roy's triangulations, Dalby noted yet another error in the 1790 paper. Cavendish's house on Clapham Common had been the corner of one of Roy's secondary triangles, and its bearing from the dome of St. Paul's was printed incorrectly. Dalby wrote to Cavendish about it and corrected it in his appendix.[136] The error might not seem like much: instead of 26°, 29', and 56.1", it should have been 26°, 29', and 52", but given the instruments, methods, and objectives of the triangulations, it was significant.[137]

In protecting the reputation of the Society, the reputation of Roy was protected as well; for what was valuable in his work was his observations, which were excellent.[138] In 1784 he laid the foundation for the national survey, and in his papers of 1785, 1787, and 1790, he explained the methods for accurate triangulation. Cavendish headed the list of committee members appointed to examine Roy's apparatus from the triangulation operation, which the king had donated to the Royal Society.[139]

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[134] Roy (1790, 201).

[135] William Roy (1777, 653–654).

[136] Isaac Dalby to Henry Cavendish, 13 Nov. [1790]; in Jungnickel and McCormmach (1999, 680).

[137] Dalby, "Remarks on Major-General Roy's Account," 614.

[138] Roy's errors were unimportant relative to his observations, according to John Playfair in his review of William Mudge's collection of memoirs on the triangulation begun by Roy. Playfair (1822, 4:198–201).

[139] 11 Nov. 1790, Minutes of Council, Royal Society 7:232–234.

Chapter 12
Associates

Charles Blagden

Around the time of his father's death, Henry Cavendish acquired a helper, Charles Blagden (Fig. 12.1). Wilson called Blagden Cavendish's "assistant,"[1] which he was, but his part in Cavendish's affairs was more extensive than what what we normally think of as an assistant's. He was a professional man, a physician, and a scientific colleague, and he was also a confidant, who knew personal things about Cavendish such as his income and the terms of his will. For these reasons and to distinguish him from the young men Cavendish occasionally hired to assist him, we speak of him as Cavendish's "associate." His relationship to Cavendish being unique, he holds our attention for what he can tell us about our subject.

A man of modest means and talent, unlike Cavendish in both respects, Blagden made for himself a suitable place in science. Born in 1748 in a village in Gloucestershire, he studied medicine at the University of Edinburgh, where he received a good introduction to science from lecturers who included William Cullen and Joseph Black, two of the most eminent scientific men of their day. Among Blagden's papers is a copy of Black's lectures, partly written in Blagden's hand, together with a testimonial by Black saying that Blagden attended his lectures. Cullen thought highly of Blagden, describing him as "a man of very great worth," who was "very much in my family."[2]

In 1768 Blagden graduated with an M.D. The following year he was informed of an "immediate opening for a physician at Gloucester" created by the departure of one of the two physicians at the infirmary. In this way, he began his life as a physician close to where he grew up.[3] His correspondence from the time tells of his interest in acquiring scientific instruments. A London friend ordered for him an electrical machine from Ramsden, which he may have used on his patients, and a microscope with a lens made by the optical instrument maker Peter Dolland.[4] He was interested in learning foreign languages. His friend looked in bookstores for a German grammar for him, and after inquiring at twenty bookstores he found a two-volume French and German grammar, printed in Paris. A bookstore that had the *Journal de Medecin* had no German books on medicine, but it would order them.[5] Blagden was restless in Gloucester, wanting to be where the world-class instrument makers and bookshops were. His London friend tried to dissuade him from leaving Gloucester, where he had the opportunity of "reducing theory to practice." Dutifully having made inquiries for

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[1] Wilson (1851, 129).

[2] William Cullen to William Hunter, 11 Feb. 1769, in John Thompson (1859, 555–556).

[3] Henry Cumming to Charles Blagden, 7 Nov. 1769, Blagden Letters, Royal Society, C.72. J. Smart to Charles Blagden, 22 Sep. 1769, ibid., S.11.

[4] Jesse Ramsden to Charles Blagden, 23 Nov. 1769, ibid., R.40.

[5] Henry Cumming to Charles Blagden, 26 Mar. 1767. Thomas Curtis to Charles Blagden, 26 Dec. 1769, 7 Jan., 8 Feb. 1770, ibid., C.77–79.

him, his friend learned that leading London physicians such as William Heberden earned between £2000 and £4000 annually, perhaps more, but that a physician was at a great disadvantage if he was not at least thirty, and if he did not graduate from an English university. He advised Blagden that to be taken seriously, he should wait four or five years or longer and probably get an Oxford degree before setting up practice in London.[6] Not long after, in early 1772 another friend wrote to Blagden concerned that he had left Gloucester where many people had "conceived so good an opinion" of him that they preferred him to the older physician in the infirmary; but he realized that Blagden's happiness lay with the "great town where all of your interests centered."[7]

In London Blagden soon expanded his connections with science, which led to his election to the Royal Society in 1772. It is in connection with that body that he made his main contributions to science, which included a number of scientific researches appearing in the Society's *Philosophical Transactions*. His preferred subject was heat, a specialty of his professors in Edinburgh, and one of Cavendish's major interests. His first two published papers dealt with the physiological side of heat, bridging his medical and scientific interests. Cullen taught that animals have the power of "generating cold," and while Blagden was in Edinburgh, he made a "rude experiment" with a thermometer and a frog to observe the power. In response to a request from a colleague "to observe the effects of air heated to a much higher degree than it was formerly thought any living creature could bear,"Blagden together with several other fellows of the Royal Society entered a closed room containing a red-hot cast iron stove, where they remained for various lengths of time. The room was heated to over 260°, hot enough to roast beef and harden eggs, while all the time their body temperature remained normal. Evaporation of sweat helped to cool them, but it could not account for the exact preservation of their body temperature. The experiments proved, Blagden said, "in the clearest manner, that the body has a power of destroying heat" arising from the "principle of life itself," disproving common theories of the generation of heat in animals held by the "mechanical and chemical physicians." The experiments were repeated to determine the effect of clothing, using even higher temperatures. Blagden called the heated room an "instrument," useful to a physician to produce sweating in treating ailments.[8] The experiments demonstrated courage of conviction, but being directed to physiology and to medicine and concerned with the principle of life, they did not directly relate to Cavendish's interests.

Wilson and the author of a biographical article on Blagden said that it is unknown how Blagden became acquainted with Cavendish, how he came to work with him in 1782, and why they parted in 1789.[9] While we cannot answer these questions completely, we have a fair idea. Blagden met Cavendish at the Royal Society Club, where he dined as a guest several times in the fall and winter of 1773, and again several times in 1774 and 1775.[10] The first documented scientific connection between Blagden and Cavendish came about in the year of Blagden's last experiments in a heated room, 1775. The war with the American Colonies had begun, and Blagden was scheduled to go to North America as an army surgeon. Cavendish advised him to compare the temperature of the air with the temperature of the sea on his journey there and when he arrived to take the temperature of wells and springs

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[6] Thomas Curtis to Charles Blagden, 15 Jan. 1770, ibid., C.78.

[7] J. Smart to Charles Blagden, 24 Feb. 1772, ibid., S.16.

[8] Charles Blagden (1775a, 112, 119, 122; 1775b, 493).

[9] Wilson (1851, 128–29). Frederick H. Getman (1937, 70–71).

[10] He was elected to the Club in 1780. Geikie (1917, 122, 125, 127, 148).

as a way of estimating the mean temperature of the climate. Under the conditions of war, Blagden was unable to make many observations of wells and springs, but he succeeded in measuring the temperature of the sea, leading to his discovery of the Gulf Stream and to another publication in the *Philosophical Transactions*. Having determined that the temperature of the Gulf Stream was several degrees warmer than that of the surrounding sea, he proposed that seamen add the thermometer to their navigational instruments.[11] We see that by 1775 Blagden was already Cavendish's scientific collaborator, that he had an interest in instruments, and that he was able to make careful observations and interpret them.

In North America, Blagden could hear the heavy guns of Hudson River forts firing on British ships. He went ashore in New York, not far from the action, a cannon shot passing through his house and raising the floor of his room. He witnessed great confusion in the city, flames rising in much splendor over a horrible scene. He sent back to England plant and animal specimens, in return for which Banks sent him scientific news and a book just published, perhaps for its relevance to Britain in its peril, volume one of Edward Gibbon's *Decline and Fall of the Roman Empire.*[12] Blagden longed to return to England and to the scientific life he had begun there. Upon rumors of peace, he expected to return in the summer of 1778, but the war took an unfavorable turn, and by the fall he was convinced that Britain should quit the war. In the hope of "soon leaving this accursed scene of disgrace,"[13] he appealed to General Charles Cornwallis, who gave him permission to return in late 1779. By June 1780, he was in Plymouth working in a military hospital.[14] C.J. Phipps, who a few years before had made a voyage of discovery towards the North Pole, offered to help Blagden's medical career through his connections with the admiralty,[15] but that was not the life Blagden saw for himself. In November 1781, he was in London in time to attend elections at the Royal Society.

While Blagden was in North America, Cavendish was on his mind; he wrote to Banks, who was then president of the Royal Society, asking why Cavendish was left off the Council in 1778.[16] Soon after his return, on March 9, 1782, Blagden had breakfast at Cavendish's house, the first mention in his diary of a visit.[17] He was still a regimental surgeon,[18] and in June he was called back to Plymouth.[19] In July he wrote to Banks that he would like to "live as much as I can among books," wondering if the Royal Society would make a position for him such as "Inspector of the Library, or something of that sort," with apartments next to the Royal Society's in Somerset House. He would be willing to pay or to superintend the library in exchange, but Banks told him it was not possible to use the apartments for any purpose other than what they were meant for.[20] In November he wrote to Banks that he endured the "miserable exile here only with the hope of soon returning to your society, in which all the

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[11] Charles Blagden (1781, 334, 341–44). A more complete statement of Cavendish's part in this discovery is in the draft of a paper in Blagden Papers, Yale, box 2, folder 26.

[12] Charles Blagden Diary, 1776–1779, Yale

[13] Charles Blagden to Joseph Banks, 12 June 1778, 20 Oct. 1778, Natural History Museum, Botany Library, Dawson Turner Collection 1:191–192, 228–229. Hereafter BM(NH), DTC.

[14] Charles Blagden to Joseph Banks, [21 June 1780], ibid. 1:290–291.

[15] C.J. Phipps, Lord Mulgrave to Charles Blagden, 1 Mar. 1780, Blagden Letters, Royal Society, B.35.

[16] Charles Blagden to Joseph Banks, 2 Mar. 1778, BM(NH), DTC 3:184–185, on 184.

[17] 9 Mar. 1782, Charles Blagden Diary, Royal Society, 1.

[18] 15 May, 14 Aug. 1782, Charles Blagden Diary, Royal Society, 1.

[19] Charles Blagden to Joseph Banks, 30 June, 3 Nov. 1782, BM(NH), DTC, 2:147–150, 205–206.

[20] Charles Blagden to Joseph Banks, 19 July 1782, draft; Joseph Banks to Charles Blagden, 19 Aug. 1782, Blagden Letters, Royal Society, B.8a and 9.

comfort of my life is centered."[21] Two weeks later he was back in London; he would not return to Plymouth.

As he had promised Cavendish before he left for war, Blagden bottled air in Plymouth in all kinds of weather. In his diary for 28 November 1782, he noted that he assisted Cavendish in testing the bottled air with a eudiometer at his new house in Hampstead,[22] the first entry in his diary pointing to a formal association. The next entry is in December, when he went again to Hampstead to dine with Cavendish and to look over the Hudson's Bay experiments on the freezing of mercury,[23] and that same month Cavendish advised him to draw up an historical account of the freezing of mercury for the *Philosophical Transactions*. In his diary in December, Blagden recorded Cavendish's idea that dephlogisticated air is only water deprived of phlogiston, his principal conclusion from a long series of experiments on air.[24] The next time Blagden mentioned his assistance to Cavendish in his diary was in January 1783, when before dining with a club, the two of them measured the thermometer tubes used at Hudson's Bay to freeze mercury.[25] He recorded that on 26 February Cavendish froze mercury without the aid of Hudson's Bay weather. Caught up in this work, Blagden froze a finger white several times while experimenting on freezing mixtures.[26] Such was the beginning of Cavendish and Blagden's association, and all indications are that it went well. On Thursdays when Cavendish did not attend the dinners of the Royal Society Club, neither did Blagden, the two clearly having a common activity keeping them away.[27]

In May 1783, Blagden went on half pay as an army physician.[28] In June and July, he visited Paris for the first time, noticing things that catch a visitor's eye such as people walking on dirty streets wearing their hair highly dressed. More important, he dined with Lavoisier, who showed him experiments.[29] Blagden's papers contain a list of people to meet in Paris, who included Laplace, Lagrange, Coulomb, and Berthollet. Without knowing it, he was preparing himself for the role he would play as a conveyor of scientific information between England and France.

After Blagden had declined Phipps's offer to help him in his medical career, a similar opportunity came up in In October 1783, this one definite and remunerative. Heberden informed Blagden that he could replace the "chief Physician" in Canterbury, where there were "many Gentlemen's families in the neighborhood," the previous physician being "supposed to have got about 1000 guineas annually." Blagden again declined, and explained why: "My views are so little turned towards wealth and so earnestly fixed upon objects which can scarcely be obtained out of the capital, that I feel I could not be happy, for the present at least, in any engagement which should remove me to a distance from London." He thanked Heberden, filling the rest of his letter with scientific news, in keeping with his refusal.[30] Blagden told Banks that he was not "at a loss for a moment" how to respond to

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[21] Charles Blagden to Joseph Banks, 3 Nov. 1782, BM(NH), DTC 3:205–206, on 205.

[22] Blagden to Banks, 3 Nov. 1782. Entry for 28 Nov. 1782, Charles Blagden Diary, Royal Society, 1.

[23] 17 Dec. 1782, Charles Blagden Diary, Royal Society, 1.

[24] 23 Dec. 1782, ibid. An ahistorical translation of this statement is: oxygen is only water deprived of hydrogen.

[25] 21 Jan. 1783, Charles Blagden Diary, Royal Society, 1.

[26] 25 Feb. 1783 and following entries, ibid.

[27] For the 1780s: Minute Book of the Royal Society Club, Royal Society, 7.

[28] Letter from the war office: FitzPatrick to Charles Blagden, 7 May 1783, Blagden Letters, Royal Society, F.10.

[29] 7 June 1783, Charles Blagden's diary of his travels in France in 1783, Blagden Papers, Yale, box 1, folder 3.

[30] William Heberden to Charles Blagden, 7 Oct. 1783, Blagden Letters, Royal Society, H.23. Charles Blagden to William Heberden, 8 Oct. 1783, ibid., H.23.a. These two letters are also published in Ernest Heberden (1985, 185).

Heberden's offer, even though he realized that by declining he would "never hereafter either have an opportunity or inclination to resume" the practice of medicine. He anticipated being "employed in far different occupations."[31]

Two days after informing Banks of his decision, Banks told him that he wished he were one of the secretaries of the Royal Society instead of the person who had the job, and that he had thoughts of doing something about this.[32] In late spring 1784, with Banks's backing, Blagden was elected secretary, which came with a small salary. Blagden, it seems, had realized his desire. He was at the center of scientific activity in the nation's capital, the right-hand man of the president of the Royal Society, the secretary in charge of the Society's *Philosophical Transactions*, a correspondent with scientists around the world, and the associate an eminent man of science, Cavendish.

At the time that Blagden and Cavendish came together, both were single and resettling, Cavendish in midlife at fifty-one, and Blagden in a change of career at thirty-four. Cavendish would have been drawn to Blagden for what Boswell called his "copiousness and precision,"[33] which were, after all, traits of Cavendish's too. The author of a profile of Blagden said that he was "very methodical in his *work* and permitted no interruption to his daily routine," in which respect he again was like Cavendish, and that he was good at "arranging and expounding data, a qualification which made him of great value to Cavendish."[34] Because of his office in the Royal Society and his regular attendance at Banks's social gatherings and at meetings of scientific clubs, he was well informed about what went on in science. "It is scarcely possible that any ph[ilosophical] discoveries can be made in England without coming to my knowledge by some channel or another," he wrote to a foreign colleague.[35] With his facility in foreign languages, his frequent visits abroad where he met with scientific men, and his extensive foreign correspondence, he was almost equally well informed about research abroad. Robert Brown said that he was "au courant du jour" in following the progress of the sciences. The French archeologist and engineer Edme François Jomard said he was also "au courant" on a wider range of activities, such as new voyages, new industrial discoveries, and new productions of all kinds.[36]

During the time he acted as Cavendish's associate, and because of it, he became involved in a controversy over the discovery of the composition of water, whether it belonged to Cavendish, Watt, or Lavoisier. His integrity was called into question, but his accusers were wrong, and unbiased colleagues such as Thomas Thomson and Robert Brown understood him to be an honorable man.[37] He was conscientious, loyal, and accessible when Cavendish wanted his help. He "was not a man of genius," Wilson said, "his writings displayed no originality, nor has he any place among the discoverers of science,"[38] but his limitations may have been an asset. Lacking a strong scientific direction of his own, Blagden effortlessly entered into Cavendish's scientific life.

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[31] Charles Blagden to Joseph Banks, 16 Oct. 1783, BM (NH), DTC 3:127–131, on 127.

[32] Joseph Banks to Charles Blagden, 18 Oct. 1783, Blagden Letters, Royal Society, B.21.

[33] James Boswell (1821, 4:309).

[34] Getman (1937, 74).

[35] Charles Blagden to Benjamin Thompson, 7 Feb. 1786, draft, Blagden Letterbook, Yale.

[36] Wilson (1851, 132).

[37] Ibid., 135.

[38] Ibid., 132.

Samuel Johnson called Blagden a "delightful fellow," but that is not the impression he often made. One of Wilson's sources called him "formal," another called him "stiff,"[39] the chemist Humphry Davy found him "cold & selfish."[40] A close friend of the historian Edward Gibbon, whom Blagden visited abroad, described him as "the scientific, but most conceited and pedantic ex-secretary of the Royal Society, whom I first saw, and learned to dislike, at a great supper at my friend Mr Freudenreick's at Berne in 1788."[41] Generalizing from a number of impressions, Wilson described Blagden as "a somewhat formal and ungenial person, more an object of respect than of love," a description which, to be sure, applied to many men of science, and definitely to Cavendish.[42] Blagden's and Cavendish's impenetrable exteriors, which were received by others as asocial, may have played a constructive role in their relationship, keeping them focused on the common work at hand.

Blagden's association with Cavendish was recognized. In a letter to Blagden in 1785, Banks asked him to give his compliments to Cavendish, toasting them, "May success attend all your mutual operations."[43] Their association was mutually advantageous: Cavendish received assistance in his experiments and in other scientific activities, and in return Blagden received several benefits. First, Blagden got attention for assisting a renowned researcher. After Cavendish's death, Robert Stewart, Lord Castlereagh listed Blagden's qualifications in science: he had published a number of papers, he had been secretary of the Royal Society, and he had been "an intimate friend of the late Mr.Cavendish."[44] Second, Blagden profited scientifically. Of the ten substantial papers he published in the *Philosophical Transactions*, four originated with Cavendish's line of research, and two others were done with Cavendish's help.[45] Blagden was awarded the Copley Medal of the Royal Society for his last original research, on the freezing points of solutions, a subject Cavendish was then investigating.[46] Blagden's published research came to an end with his break with Cavendish.

A third benefit was a supposed annuity of £500 settled on him by Cavendish. Several early writers state this as fact, and one of them says that the annuity came with the condition that Blagden discontinue his practice of medicine.[47] Both of these statements are subject to question. Over the six years for which we have Blagden's financial records, 1785–90, which overlap the years he was Cavendish's associate, we see that his income came from three sources: his half pay from the army, his salary from the Royal Society, and dividends from his securities, which included and may have consisted entirely of Scotch bonds. From his financial records, we find no direct evidence of an annuity, but in 1789 he deposited £1400 from the sale of a house he bought while working for Cavendish.[48] An annuity or its

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[39] Wilson's sources were Robert Brown and Mr. Caddell. Ibid.

[40] Davy said the same thing about Cavendish. J.C. Fullmer (1967, 133).

[41] Sylvester Douglas, Lord Glenbervie, F.R.S., in his *Diaries* (London, 1928), quoted in G.R. De Beer (1950, 76).

[42] Wilson (1851, 133).

[43] Joseph Banks to Charles Blagden, 28 July and 4 Aug. 1785, Blagden Letters, Royal Society, B.35 and 36.

[44] Lord Castlereagh to Charles Stuart, 13 July 1819, copy, Blagden Letters, Royal Society, C.6.

[45] Cavendish's involvement in Blagden's scientific work is documented in Blagden's publications and in his manuscripts, which contain pages written in Cavendish's hand. In counting ten papers in the journal by Blagden, two papers are omitted: an extract from a letter by Blagden on the tides at Naples in 1793, and an appendix to Ware's paper on vision in 1813.

[46] Blagden developed a simple quantitative relation, the lowering of the freezing temperature of a solution is proportion to the quantity of solute, known to this day as "Blagden's Law."

[47] A £500 annuity was mentioned by Blagden's brother John Blagden Hale, Thomas Thomson, and Henry Brougham. The latter said that Cavendish insisted that Blagden abandon his practice of medicine. Wilson (1851, 133, 142, 160).

[48] Gloucestershire Record Office, D1086, F156.

equivalent as a house was delivered in a way that did not show on his balance sheets as income. Payment in some form for Blagden's services would have been proper, and Cavendish would not have accepted them otherwise. Concerning the other statement, there is evidence that Cavendish encouraged Blagden to practice medicine, which is incompatible with the alleged condition that he give up the practice of medicine unless Cavendish's encouragement implied an end to their relationship.[49]

Blagden's association with a rich aristocrat was grist for rumor mills. The chemist Richard Kirwan wrote to a French colleague that Blagden looked at questions in science only through Cavendish's eyes because Cavendish "is a near relation of the duke of Devonshire and has six thousand pounds yearly income."[50] Blagden's critics said he was avaricious. According to Henry Brougham, Blagden wished to marry the widow of Lavoisier for her wealth. Jomard, however, described him as liberal with money.[51] "Frugal" better describes Blagden. At the time of his death, his estate was valued at around £50,000,[52] an amount which had ceased to be a large fortune in the eighteenth century, though with it Blagden could have lived quite comfortably.

In the fall of 1784 Blagden exchanged his lodgings off Great Ormond Street for a rental house across the street from Cavendish's, No. 7 Gower Street, Bedford Square. The following spring he gave notice to his landlord,[53] and he moved into another house on the same street on the same side as Cavendish's, No. 19 Gower St., Bedford Square. The move was definitely upscale; the ratable value of his new house was double that of his first, and half the value of Cavendish's. Blagden owned this house and after four years he sold it.[54] His papers contain two undated, unaddressed draft letters referring to a house, which we have reason to think were written to Cavendish.

> Just after you were gone Mr Hanscombe called here with the inclosed note, & opened it; he had [-----] before at your house, but having been informed you were gone by to Hampstead came to shew it to me. I am extremely obliged to you for the liberal offer you have made; but as, were I so rich that the sum would be no object to me I should still think it too much for the house, & shd probably refuse to give it. I cannot but consider it as totally inequitable that you shd give it for me. I therefore do most seriously request that you would refuse to comply with the terms proposed, & wait till an opportunity offers of making a fairer purchase; and in the mean time I will use every means in my power to become reconciled to my present situation.[55]

Blagden refers to Hampstead, where Cavendish had a country house from 1782 to 1785. The other reference is to Thomas Hanscomb, a builder who around this time inquired about

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[49] Charles Blagden to Joseph Banks, 8 Apr. 1790, draft, BL Add Mss 33272.

[50] Richard Kirwan to Guyton de Morveau, 9 Jan. 1786, in Guyton de Morveau (1994, 161–164, on 163).

[51] Getman (1937, 73).

[52] Ibid., 74.

[53] Charles Blagden to Mr. Mountfort, 22 Mar. 1785, draft, Blagden Letterbook, Yale.

[54] In 1786 the rate books list Blagden at both of his addresses on Gower Street; for that year the ratable value of his first house was £32 and of his second house £65; Cavendish's house was valued at £120. Rate Books for Gower Street: Bloomsbury Division (part 1), 11; St. Giles in the Fields Division (part 2), 23, 25. For 1789: St. Giles in the Fields Division (part 2), 31–32. Camden Archives.

[55] Blagden Collection, Royal Society, Misc. Matter – Unclassified.

property on Clapham Common as Cavendish's agent. Among Blagden's papers is a carpenters' bill from Hanscomb & Fothergill for work done on *Blagden's*, house at Clapham in the summer and fall of 1785.[56] It is conceivable that Blagden considered settling at Clapham, where Cavendish intended to do his experimental work. There are no extant rate books for Clapham, but the less inclusive Clapham land tax records have survived, and they contain no listing for Blagden. Nowhere in Blagden's correspondence or in other papers do we find mention of a house at Clapham. Cavendish had extensive work done on his new house at Clapham Common in the months covered by the carpenters' bill, which was probably incorrectly labeled, intended for Cavendish with Blagden acting for Cavendish. The second draft letter concerns a quarrel between Blagden and Banks, placing it in 1789 or 1790: "The generosity of your conduct in your original offer, in your subsequent present of this house, in your late confirmation of that present, and especially in your further offer when I expected to marry last year, I shall always take a pride in acknowledging."[57] The mention of the offer to Blagden upon his marriage almost certainly identifies Cavendish as the recipient of this letter. The house that Blagden sold in 1789 was his house on Gower Street, and we believe that it was the "present" referred to in this letter, and that Cavendish was the giver. We know that Cavendish wanted to help Blagden resume his medical practice, a goal which a house would have served more directly than an annuity.

It is thought that Cavendish and Blagden ended their association in 1789. Thomson said that they did not get along and Blagden "left him."[58] Wilson was able to learn only that their association "did not suit." The timing of their break and almost certainly part of the reason for it had to do with a conflict between Blagden and Banks. Blagden felt that he was exploited by Banks, who used him for his own ends without recompense, while discouraging him from following his profession, medicine. He seriously considered resigning his secretary post in the Royal Society, believing that his accepting it had been the great misfortune of his life. Cavendish was not a cause of his break with Blagden so much as an affected third party, though a break would probably have come in any case. Cavendish's call on Blagden's services had never been onerous, and long before 1789 he had come to the end of most of his major experimental investigations, the one exception coming much later.

Blagden considered living abroad for the winter 1789–90 in part because it might "prevent an open rupture with Sir Jos. Banks." He asked Cavendish if his absence would hold up any work of his that Blagden was unaware of. "Now I trust to the strict principles of <openness> sincerity by which I know you are always guided "that you will fairly tell me" for an open & explicit answer to the question whether you have on your own part any objection to my going."[59] These are not words of someone who is breaking off a relationship. Cavendish's only concern about Blagden's going was that it might interfere with the pursuit that Blagden had "much more at heart than any object in life," which Cavendish understood to be the practice of medicine.[60] Blagden went abroad with Henry Temple, Lord Palmerston that fall but the tour had to be abandoned, and Blagden remained in London through the winter. To all outward signs, Cavendish and Blagden's break was amicable, and it was

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[56] Hanscomb & Fothergill, "Carpenters Work Done for Dr Blagden at His House at Clapham," Gloucestershire Record Office, D 1086, F153.

[57] Blagden Collection, Royal Society, Misc. Notes, 224.

[58] Thomas Thomson (1830–1831, 1:338).

[59] Blagden to Cavendish, Aug. 1789.

[60] Charles Blagden to Henry Cavendish, Aug. 1789, draft; in Jungnickel and McCormmach (1999, 666–667).

not sharp. A year later, in late 1790, Blagden was still acting as Cavendish's associate, writing letters to several colleagues inviting them to Cavendish's house at Clapham Common to witness an experiment on the specific heats of different airs.[61] Some days later, he told Banks that he had been detained from setting out on a journey "at first by an experiment at Clapham."[62] Blagden continued to accompany Cavendish to meetings of the Royal Society, and he continued to send Cavendish scientific news from abroad. Frequent as their meetings were, they did not have their former closeness. When Cavendish died, Blagden wrote in his diary of an earlier time when he had been "intimate with him."[63]

Clubs

The setting of Henry Cavendish's social life was clubs. From the Restoration in the seventeenth century through the eighteenth century and beyond, men of science congregated in the coffee houses and taverns of London, often meeting as clubs.[64] The Royal Society Club, the best known and best documented of Cavendish's clubs, met at the Mitre Coffee House on Fleet Street, and later at the Crown & Anchor on the Strand. In letters to Cavendish from the 1770s, Alexander Dalrymple sent greetings to their mutual friends at the Mitre and at the King's Head. The King's Head Tavern in Chancery Lane was where Robert Hooke and other fellows of the Royal Society gathered in the late seventeenth century, but King's Head was a common name for taverns.[65] In letters to Cavendish from the 1780s, John Michell greeted their common friends at the Cat & Bagpipes, a popular tavern and chophouse located on Downing Street.[66] Cavendish went with his father to a club that met in a private house on the Strand, mentioned earlier. His father met with a club at Rawthmell's Coffee House on Henrietta Street, in Covent Garden, and we are almost certain that Henry did too. To settle the time of inspection of government powder magazines, William Watson asked a fellow committeeman Benjamin Franklin to "call in Henrietta Street," as "Mr. Cavendish [chairman of the committee] seldom fails of coming there."[67] There were other scientific clubs Cavendish did not belong to but his colleagues did. One club, which included Aubert, Nairne, and Kirwan, met at the Chapter Coffee House and later at the Baptist Head Coffee House.[68] Another club, which included Blagden, Banks, and Maskelyne, met at Jack's Coffee House and later at Young Slaughter's Coffee House on St. Martin's Lane.[69] Other clubs met at Banks's[70] and at Kirwan's[71] houses.

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[61] As the experiment would take the better part of the day, they were to arrive by 10 AM, and if they arrived by 9 AM they could join Cavendish at breakfast. Charles Blagden to Edward Nairne, 5 Oct. 1790, draft, Blagden Letters, Royal Society 7:457. Charles Blagden to Henry Cavendish, 5 Oct. 1790, draft; in Jungnickel and McCormmach (1999, 679).

[62] Charles Blagden to Joseph Banks, 17 Oct. 1790, BL Add Mss 33272, 91–92.

[63] 1 Mar. 1810, Charles Blagden Diary, Royal Society 5:428(back).

[64] A.E. Musson and E. Robinson (1969, 58). Bryant Lillywhite (1963, 22–24).

[65] From the 1730s, there is a record of a meeting that included a number of scientific men at a King's Head. R. Parkinson (1854–1857, vol. 1, pt. 2, 556). Seven King's Head taverns are listed under the signs of taverns in *Vade Mecum*, included in Walter Besant (1902, 639–640).

[66] Archibald Geikie (1918, 58).

[67] William Watson to Benjamin Franklin, 31 July 1772, in Wilcox (1969/1974, 213).

[68] G.l'E. Turner (1967, 220).

[69] Henry B. Wheatley (1891, 2:484). Lillywhite (1963, 404).

[70] John Strange to Joseph Banks, 8 Aug. 1788, Banks Correspondence, Royal Botanic Gardens, Kew, l.315.

[71] Musson and Robinson (1969, 123).

Named after the day of the week it met, the Monday Club met at the George & Vulture, a coffee house located in George Yard, off Lombard Street.[72] This club had been meeting since at least the 1760s,[73] and Cavendish came to it regularly for fifteen years or more. When John Pringle returned from Edinburgh to London in 1781, he rejoined the Monday Club, where he met with "such friends as Mr. Cavendish, Dr. Heberden, and Dr. Watson."[74] Blagden began coming to it soon after he returned to London, as we know from his diary.[75] Aubert, Dalrymple, Franklin, Phipps, Nairne, and Smeaton were members.[76] The discussions at this club were often continuations of those at the Royal Society and the Royal Society Club.[77] Blagden's diary shows that he and Cavendish frequently went together to dine at the Monday Club; upon returning home from there one night, Blagden noted: "went with him [Cavendish] to Club: I spoke of spirit & independence, & true friends."[78]

Colleagues

Figure 12.1: Sir Charles Blagden. Etching from the portrait by Thomas Phillips. Secretary of the Royal Society and Cavendish's associate in the 1780s.

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[72] Lillywhite (1963, 160, 201, 699, 792).

[73] Verner W. Crane (1966, 213).

[74] Quotation from the *Annual Register*, 1783, 45; in James Sime (1900, 50).

[75] 1 Jan. 1782, Charles Blagden Diary, Royal Society, 1.

[76] On Franklin and Aubert: Crane (1966, 213). On Dalrymple: 15 June 1795, Charles Blagden Diary, Royal Society 3:62 and elsewhere. On Phipps, Nairne, and Smeaton: Alexander Aubert to Joseph Banks, 1 July 1789, BL Add Mss 33978, no. 251.

[77] Alexander Aubert to William Herschel, 7 Sep. 1782, Herschel Mss, Royal Astronomical Society, W 1/13, A 10. Aubert to Banks, 1 July 1789.

[78] 25 Aug. 1794, Charles Blagden Diary, Royal Society 3:13.

Figure 12.2: Alexander Dalrymple. Engraving by Rudley from a drawing by John Brown. Frontispiece to *European Magazine* 42 (November 1802).

Figure 12.3: Engraving by J. Chapman, painting by S. Drummond. Wikipedia.

Figure 12.4: Engraving by J. Mitan from the portrait by G. Slous. Wikimedia Commons.

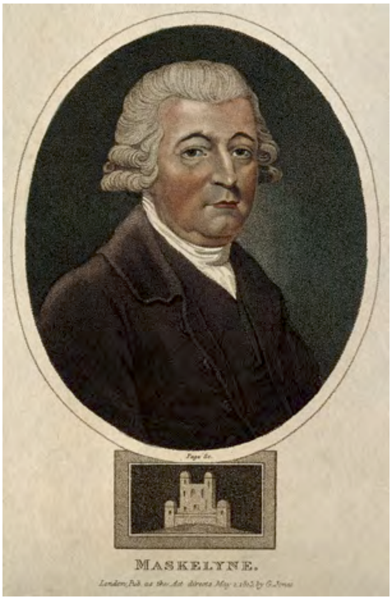

Figure 12.5: Nevil Maskelyne. Coloured stipple engraving by R. Page, 1815. Wellcome Library, London.

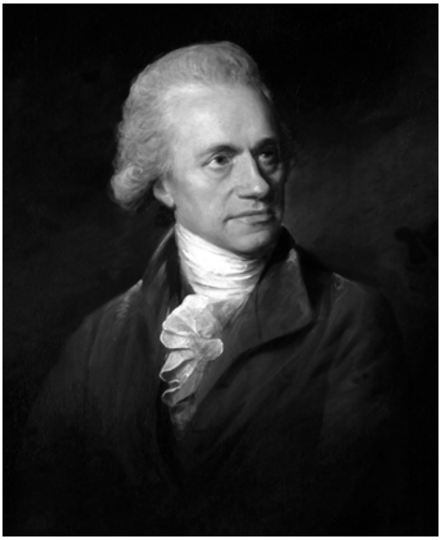

Figure 12.6: Sir William Herschel. Painting by Lemuel Francis Abbott, 1785. Wikimedia Commons.

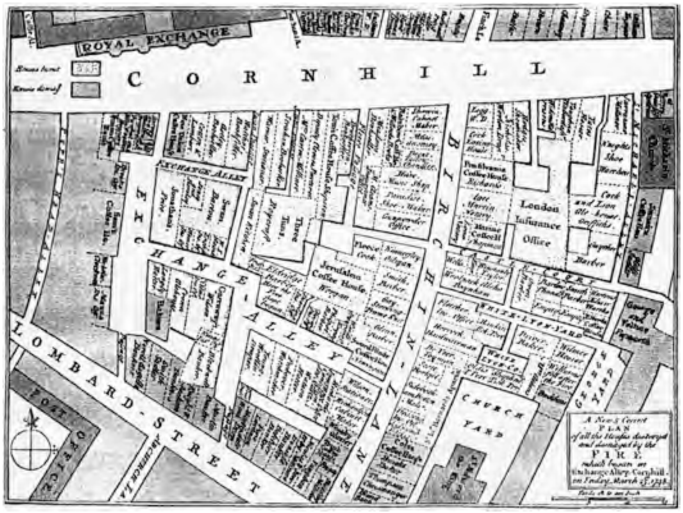

Figure 12.7: Map of Cornhill. The map shows the buildings damaged or destroyed by a fire originating in Exchange Alley in 1748. A good number of coffee shops relocated but not the George & Vulture off George Yard, shown at the upper right corner of the map. Cavendish met his colleagues there on Monday evenings. Aytoun Ellis (1956, 94).

Chapter 13
Politics

The president of the Royal Society Joseph Banks kept his distance from political faction: “I have never entered the doors of the House of Commons,” he told Benjamin Franklin at the time of the American Revolution, “& I will tell you that I have escaped a Million of unpleasant hours & preserved no small proportion of Friends of both parties by that fortunate conduct.”[1] The year after Banks wrote to Franklin of his apolitical conduct, he became the focus of a political struggle in the Royal Society. Like Banks, Henry Cavendish did not participate in national politics, but like Banks and because of Banks he was drawn into a contest for power among the men of science.

Royal Society

In his history of the Royal Society, Charles Richard Weld wrote that it was “painful” for him to turn to the events of 1783 and 1784. He would rather have passed over them in “silence,” but duty fobade it. He gave what he regarded as an impartial account of the so-called “dissensions,” which “turned the hall of science into an arena of angry debate, to the great and manifest detriment of the Society.”[2] The dissensions originated, Weld said, in a widespread resentment of Joseph Banks (Fig. 13.1). President of the Royal Society since 1778, Banks announced at the time of his election his “determination to watch over the applications for admission, and the election by ballot.” There being no secret about it, fellows wishing to elect a new member would likely bring him to one of Banks’s breakfasts, and if Banks approved of him, he would then be invited as a guest to a dinner of the Royal Society Club, at which Banks also presided, where he would meet influential members. If Banks disapproved of the candidate, he would urge fellows to blackball him at balloting time.[3] Banks was not always successful.[4]

For the good of the Society, Banks believed, members should bring in two kinds of persons, men of science and men of rank.[5] Like the membership at large, the ruling Council of the Society contained men of both kinds, and here again, in the elections Banks made clear his likes and dislikes, exposing himself to the charge of packing the Council with pliant friends. Banks’s forceful interference in elections revealed a pattern, so certain members

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[1] Joseph Banks to Benjamin Franklin, 9 Aug. 1782, quoted in A. Hunter Dupree (1984, 15).

[2] Charles Richard Weld (1848, 2:151). This discussion is taken largely from Russell McCormmach (1990). We acknowledge permission by the Associated University Presses to use material from this chapter.

[3] Weld (1848, 2:152–154). “Sir Joseph Banks,” in Henry Brougham (1845, 364).

[4] Charles Blagden to Joseph Banks, 30 Oct. 1785, Banks Correspondence, Royal Botanic Gardens, Kew, l:213.

[5] In defense of Banks, Andrew Kippis said that in addition to men of science and men of rank and fortune, the Royal Society should have a third category, “men of general literature,” who could form “a right opinion concerning the general value of the philosophical observations and experiments which are produced at the Society’s meetings.” J.L. Heilbron (1993b, 88).

thought, of a bias against men of the mathematical sciences, in favor of men of rank and and men of the life sciences. Their dissatisfaction with Banks came to a head in, as Weld turned it, the "violent dissensions, foreign to matters of science," of 1783 and 1784.[6]

Figure 13.1: Sir Joseph Banks, Bt, by Thomas Phillips, 1810. Wikimedia Commons.

In Weld's and other historical accounts of the dissensions, Henry Cavendish receives only one brief mention, if any at all. Speeches are quoted at greater or lesser length, but Cavendish is recalled only for his seconding of a motion of approval of Banks as president of the Society.[7] This, to be sure, is the only time Cavendish entered the public record of the dissensions, but there was more to his involvement than this, as there had to be given his standing in the Society.

To understand his part in the dissensions, we need to recall some of the characteristics of the political Cavendishes. A historian writes of the family:

> Much was heard of the "great Revolution families" – of whom some of the proudest, as Sir Lewis Namier has pointed out, were in fact descended from Charles II's bastards. These families—above all, perhaps, the Cavendishes—could not forget that their ancestors had, as it were, conferred the crown upon the king's ancestors, and they did not mean to let him forget it either, for they alluded to it in season and out of season. They looked upon themselves as his creators rather than his creation: one would almost say that they had forgotten

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[6] Weld (1848, 2:153, 170). Henry Lyons (1944, 198–199).
[7] Weld (1848, 2:162). Lyons (1944, 213).

> that the dukedom of Devonshire itself had been established, less than a century earlier, by the merely human agency of a king.[8]

Edmund Burke observed in 1771, "No wise king of Great Britain would think it for his credit to let it go abroad that he considered himself, or was considered by others, as personally at variance with [...] the families of the Cavendishes ...,"[9] but George III, Burke also observed, was no wise king. By then it was understood that the nation was governed by a cabinet headed by a prime minister, who depended on a majority in the House of Commons, all of the ministers in the Cabinet being seated in Parliament, an arrangement that was thought to have resolved the conflict between the king and Parliament of the previous dynasty. Having other ideas, George III wanted to make the cabinet and prime minister instruments of his will,[10] and upon acceeding to the throne in 1760, he immediately set about to break the power of the old Whig families. In fact, although it was not entirely obvious at the time, the Whig ascendancy had already come to an end. Marking this historic turn was the resignation in 1762 of the fourth duke of Devonshire, after whom never again could a Devonshire assume that holding high office was his birthright.

Henry Cavendish entered the public world of science at just this time, in 1760, with his election to the Royal Society and the Royal Society Club. He showed no interest in a career in politics, then or ever. He would have found campaigning hard and speaking in the House of Commons probably impossible; at best, he would have found assignments in technical committees. In its place, he wisely chose a life of science. However, from his part in the dissensions of the Royal Society, we get an idea of the kind of politician he would have made.

Devonshire House, the Piccadilly mansion of the dukes of Devonshire, was the London headquarters of the so-called New Whigs of the 1780s.[11] They were libertarian and passionately opposed to George III's policy on the American colonies. Their leader Charles James Fox was in fundamental disagreement with his king over who should govern. He believed that power was properly exercised only through the king's ministers, while the king was merely to sit on the throne, not rule from it, whereas George III believed that his ministers were *his* ministers, bound by loyalty to uphold his policy. In the ensuing constitutional struggle, the government of the kingdom was brought to a standstill. The person of George III was *the* political issue, as John Dunning asserted in his resolution of 1780, which was favored by a parliamentary majority: "That the influence of the Crown has increased, is increasing, and ought to be diminished."[12] An argument has been made that the years 1783–84 saw the greatest "political convulsion" in Britain since the Revolution of 1688–89.[13]

The same years saw the dissensions of the Royal Society, in which the president, Joseph Banks, was accused, like George III, of desiring personal rule, bringing the regular business of the Society to a standstill. The immediate cause of the dissensions was a disagreement between Banks together with his Council on the one hand and the foreign secretary Charles Hutton on the other. The foreign secretary was not necessarily on the Council. When Hutton was elected to his office in 1779, he happened also to be on the Council, but not after 1780

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[8] Richard Pares (1953, 58–59).

[9] Ibid., 59.

[10] G.M. Trevelyan (1953, 3:64).

[11] Whigs are important in Hugh Stokes (1917).

[12] Pares (1953, 119–125, 134–135).

[13] John Ashton Cannon (1969, x–xi).

when the dissensions occurred. At a meeting of the Council on 24 January 1782, Hutton's responsibility and performance were taken up, the former judged burdensome, the latter deficient. Hutton, it was decided, had not dealt with the foreign correspondence with sufficient punctuality" and was "by no means adequate to the duties of his office." Because he was also overworked and underpaid, a probable reason for his tardiness, the Council resolved that in the future, he should not be expected to translate foreign articles and extracts from books, and in return he was not to fall behind in the foreign correspondence. Hutton agreed to continue as foreign secretary with this new understanding. Nothing more was heard of the matter publicly until nearly two years later when at the meeting of the Council on 20 November 1783, it was resolved that the foreign secretary had to live permanently in London, a vote which was obviously directed against Hutton, who as professor of mathematics at the Royal Military Academy at Woolwich could not live in London. Hutton promptly resigned, and at an ordinary meeting of the Society on 11 December 1783, it was moved that Hutton be formally thanked for his services as secretary for foreign correspondence. After a vigorous debate, the motion was passed by a narrow margin, and Banks, who had opposed the motion, duly thanked Hutton. At the following meeting, on 18 December, the secretary read aloud a written defense by Hutton of his performance, after which a motion was made that Hutton had justified himself. The motion was carried, but again only after a vigorous debate. The mathematician Samuel Horsley attacked Banks, accusing him of infringing upon the chartered rights of the Society, and he said he knew of enough wrongs to keep the Society "in debate the whole winter [...] perhaps beyond the winter."[14] The prospect of a winter spent in debate would have been disturbing to Cavendish, who regarded the serious scientific purpose of the Society as having priority. He became actively, if invisibly to all but a handful of members, engaged in shaping the outcome of the dissensions. His activity is reported in daily letters from Blagden in London to Banks at his country house.

Highly personal in tone, the debates about the leadership of the Society turned on a judgment. The principal question the members addressed was this: had the Society been seriously damaged scientifically and had its honor been tarnished by its president? To keep Banks informed about opinions on the question, Blagden delicately inquired into Cavendish's position. Naturally, Banks needed to know where the Society's scientifically most eminent member stood. Four days after the stormy meeting of the Royal Society, after dining at the Monday Club Cavendish accompanied Blagden to his home, where they discussed the troubles of the Society. That morning Cavendish had gone to see Heberden, and the two of them had arrived at a common position. Blagden reported to Banks that Cavendish and Heberden would support him, but "just." While Cavendish did not "absolutely refuse a vote of approbation" of Banks, he would "absolutely oppose" any resolution that by its wording would seem to pass censure on Horsley and his friends, for they had given no evidence of acting out of any motive other than the good of the Society, and the good of the Society required that its members exercise just such scrutiny of their president and Council. Cavendish, however, did not mean that debates should be allowed during regular meetings, disrupting the scientific business of the Society. To put a stop to the debates without denying members their rights, Cavendish proposed a resolution that he believed would be passed by a very large majority. From dictation Blagden wrote down the resolution and read it back to confirm the wording: "That the proper method of rectifying any abuses which may arise in

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[14] Weld (1848, 2:154–160). 24 Jan. 1784, Minutes of Council, Royal Society 7:97–98 (University Publications of America microfilm edition).

the Society is, by choosing into the Council such persons as it is supposed will exert themselves in removing the abuses and not by interrupting the ordinary meetings of the Society with debates." Blagden did not think that this resolution would have the result Cavendish intended. Horsley would agree that it was the task of a new Council to remedy abuses, but he would argue that for the Society to be made aware of the abuses, the debates must continue. Such an argument from Horsley, Cavendish thought, would carry weight, but there was an effective answer to it. The Society would inform itself of any abuses by holding special meetings for the purpose, and then if Horsley persisted with his interruptions, the Society would be within its rights to censure or even expel him. After his conference with Cavendish, Blagden gave Banks his opinion: the resolution Cavendish proposed was probably the best of any so far, and if to it was added another resolution that any motion had to be announced at the meeting before it was to be debated, the whole affair might be brought to a speedy and favorable conclusion.[15]

Cavendish's resolution omitted all mention of support for the incumbent president, Banks, which was something less than Blagden and Banks had hoped from him. Cavendish did not even want to talk to Banks about past Councils because he would find it awkward, one obvious reason being that Cavendish had been omitted from them. Cavendish believed that Banks was "a little blamable" on this subject, though he "forgave" him. With Blagden's prompting, Cavendish recalled past presidents he had served under. Banks's predecessor, the physician John Pringle, Cavendish said, had acted like Banks and had given rise to the same complaint about ineffective Councils. Pringle's predecessor, the antiquary James West, was "King Log" (from Aesop's fable of the frogs who desired a king to watch over their morals and were thrown an insipid log instead). But West's predecessor, the astronomer and mathematician Lord Morton, had handled the affairs of the Society in an unexceptionable way. Cavendish allowed that Banks's method of choosing the candidates for Council was fair, but he blamed him for not doing as Morton did, which was to "put in people who would have an opinion of their own, without agreeing implicitly with the President in every thing." Cavendish believed that if his resolution carried, it would mean that on election day there would be a contest. He wanted Blagden to reassure Banks that he would support the "House list" on election day unless it was "very exceptionable." He also wanted Blagden to tell Banks that he did not want to be consulted on the list beforehand, as Banks hoped he would. Blagden told Cavendish—Blagden quoted himself to Banks—that "any list that he [Cavendish] can possibly think good, will be sufficient for me."[16]

Through Blagden, Banks asked Cavendish to come to his house the next day, which was Christmas. Cavendish replied, through Blagden, that he could not come. Blagden explained to Banks that it was "possible" that Cavendish had set aside the day for doing experiments, but most likely he wanted to avoid an "embarrassing conversation" with Banks. Banks was to be reassured that Cavendish was not "hostile" toward him and wanted to remain on good terms. It was necessary only that Banks should allow Cavendish to differ with him in opinion

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[15] Charles Blagden to Joseph Banks, 22 Dec. 1783; original letter in the Fitzwilliam Museum Library; copy in BM(NH), DTC 3:171–172.

[16] Ibid. Charles Blagden to Joseph Banks, Wednesday morning [24 Dec. 1783]; original letter in the Fitzwilliam Museum Library; copy in BM(NH), DTC 3:176.

at any time "without an open quarrel"; this was to repeat what Cavendish wanted of Banks in his dealings with the Council.[17]

In conversation with Cavendish, Blagden brought up the principal disrupter of the meetings of the Society, Banks's enemy, Horsley. To Banks, Blagden quoted Cavendish to convey his exact meaning. These being the only faithfully recorded spoken words by the taciturn Henry Cavendish, they hold an interest of their own.

> *Cavendish*: I did not expect any success from the Drs negotiations [Dr. Heberden and, no doubt, Dr. Horsley]. But whatever violence *they* may express, that is no reason against proceeding with all moderation, as by such conduct the sense of the Society will be insured against them.
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> *Blagden*: I wish you would see Dr. H[orsley] & learn from himself the implacable temper expressed; as I think you would then change the opinion to which you seemed inclined when we conversed last, that those gentlemen might have nothing in view but the good of the Society.
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> *Cavendish*: I did not say they had nothing else in view, but only that no proof yet appeared of other motives.

At the end of their conversation, Cavendish came around to Blagden's position: he, like Heberden, would approve a vote of confidence in Banks, but only if the wording gave no offense. By this, Blagden declared himself highly satisfied with the results of his mediation.[18]

Blagden informed Banks, "Great opposition is making against you," some members being "decidedly against you even on the subject of the Presidency." So far as he could learn, Blagden said, they intended to put Lord Mahon in Banks's place. The alleged injustice done to Hutton as foreign secretary was only the pretext; the real cause of the dissensions was a "grudge of very old standing," backed by many grievances, Heberden told Blagden.[19] Heberden did not elaborate on the grievances, but they certainly included hard feelings arising from a rift between the natural historians and the mathematical practitioners, who were competing for authority within the Society. The natural historian Banks was thought to favor natural history, as were his allies the aristocrats and gentry, who were interested in horticulture and agriculture.[20] The grievances also included Banks's alleged exclusion of deserving men from the Society because they were not of sufficient social rank, their favorite example being the able mathematician Henry Clarke, whom they said was kept out because he was a mere schoolmaster. The membership of the last Council they held in derision. The battle line, as they drew it, was between Banks's fancy gentleman, or "Maccaroni's," and the "men of Science."[21]

Blagden attached a postscript to a letter he sent to Banks dated Monday, 29 December, which read: "Resolved, That this Society approve of Sir Jos: Banks as their President, and

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[17] Charles Blagden to Joseph Banks, 24 Dec. 1783; original letter in the Fitzwilliam Museum Library; copy in BM(NH), DTC 3:177–179.

[18] Ibid.

[19] Blagden to Banks, 23 and 27 Dec. 1783. *Supplement* to: Friend to Dr. Hutton, *An Appeal to the Fellows of the Royal Society, Concerning the Measures Taken by Sir Joseph Banks, Their President, to Compel Dr. Hutton to Resign the Office of Secretary to the Society for Their Correspondence* (London, 1784), 11, 15.

[20] David Philip Miller (1981, 288–289).

[21] Blagden to Banks, 27 Dec. 1783. Charles Blagden to Joseph Banks, 28 Dec. 1783, Fitzwilliam Museum Library, Perceval H202.

mean to support him in that office." "Such, my dear friend," Blagden wrote to Banks, "is the resolution Mr. C[avendish] has just approved at my house." In Blagden's view, the vote on this resolution would sort out Banks's friends from his foes. Cavendish, he added, still thought that the resolution he first proposed would prove necessary, since the Society would not agree that under the present statutes they are forbidden to debate except on the day of elections.[22]

In anticipation of the coming meeting of the Society, Horsley told his friends that Banks was going to try to expel him, in that way ensuring, Blagden told Banks, an ample turnout of his friends.[23] To make certain that his own friends turned out, Banks sent a card to all members of the Society requesting their attendance, and at the meeting on 8 January 1784, some 170 members came, fewer than half of whom attended regularly. From the president's chair, facing the massed assembly, Banks watched as "each side took their station and looked as important as if matters of the utmost consequence to the State were the subject of their deliberation."[24] As planned, the accountant general of the Society, Thomas Anguish, rose to make the motion. The previous two meetings of the Society, he reminded his audience, had been disrupted by debates, and at the second of these, Horsley had threatened to keep the Society debating the rest of the winter, the obvious intent of which was to unseat Bank. The motion Anguish put to the members was the resolution approving of Banks, which Cavendish had earlier approved. Cavendish now seconded the resolution before the Society. Cavendish said nothing in support of it, and there is no evidence that he said anything else during this long night of angry speeches.[25]

The first speech was made by Edward Poore, a barrister at law in Lincoln's Inn, who called the motion a dishonorable attempt to evade scrutiny of Banks's conduct by praising it. The attempt would not succeed, he said; it would not stop debate (and did not, as Cavendish and Heberden had predicted). Francis Maseres, curistor baron of the exchequer and mathematician, said that for the Society to exercise its power of election of the president and Council, it had first to discuss the question of Banks's "abuse of power." Horsley said that the "abuses are enormous," going on about them at such length that Banks's supporters clamored for the question, almost drowning him out with their cries and with a clatter of sticks. As a last resort, Horsley said, "the scientific part of the Society" would secede, which would leave Banks leading his "feeble *amateurs*," his mace standing for the "ghost of that Society in which philosophy once reigned and Newton presided as her minister." Maskelyne, the astronomer royal, said that if it proved necessary to secede, the "*best* Society would be the *Royal* Society in fact, though not in name." The mathematician James Glenie was interrupted before he could finish what he had to say, which was that the present Council was incapable of understanding mathematics, mechanics, astronomy, optics, and chemistry, and that the Society as led by the natural historian Banks was degenerating into a "cabinet of trifling curiosities," a "virtuoso's closet decorated with plants and shells." When late in the evening the motion was finally put to a vote, it carried 119 to 42, the Society favoring Banks

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[22] Postscript dated 29 Dec. 1783, Blagden to Banks, 28 Dec. 1783.

[23] Charles Blagden to Joseph Banks, 30 Dec. 1783, Fitzwilliam Museum Library, Perceval H203.

[24] Notes of the meeting taken by Banks, quoted in Hector Charles Cameron (1952, 134).

[25] [Paul Henry Maty], *An Authentic Narrative of the Dissentions and Debates in the Royal Society, Containing the Speeches at Large of Dr. Horsley, Dr. Maskelyne, Mr. Maseres, Mr. Poore, Mr. Glenie, Mr. Watson, and Mr. Maty* (London, 1784), 24–25. *Supplement*, 9.

to continue as their president by a margin of three to one.[26] This, then, was the outcome of all the meetings, letters, maneuverings, and canvassing. The safest course had been taken by Banks's supporters in their approval of a resolution that contained no detail; it said nothing about limiting debates, nothing about abuses, and nothing about reforms, nothing, that is, that might divide the majority.

The opponents of Banks as well as his supporters claimed that they longed for a return of "tranquility, order, harmony, and accord" and the "instructive business of these weekly meetings, *the reading of the learned of papers presented to the Society*."[27] For three consecutive meetings, debates had prevented the reading of all new scientific papers. Only John Michell's paper on the distance and other measures of the fixed stars, which Cavendish had communicated to the Royal Society, continued to be read at two of these meetings, on 11 and 18 December, while at the third meeting, on 8 January, no papers at all were read.[28]

Along with Michell's paper, the main new paper read at the next meeting, on 15 January, was another strong paper, and though it was not mathematical like Michell's, but experimental, it was written by a mathematical member, Henry Cavendish. Earlier that day Paul Maty, secretary of the Royal Society and outspoken critic of Banks, wrote to Banks asking him to send papers, since there were not enough for the meeting. He said that he would not read papers he was not prepared for, nor would he come to Banks' house on Soho Square to pick up papers unless a statute was made to command him. Banks wrote back that same day saying that he had read the papers at hand, ordering Maty to read Cavendish's paper, which he sent to him forthwith. The paper, "Experiments on Air," contained Cavendish's investigation of the production of water from the explosion of gases, considered by many his most influential paper. Following upon three meetings at which members had listened to speeches contrasting the present, feeble state of the Royal Society with what it had been in Newton's day, and coming one week after Cavendish had seconded the motion approving of Banks's presidency, the reading of Cavendish's paper at the first opportunity was an answer to the charges.[29]

On 22 January, the Council of the Society passed a resolution on debates, which stated that any motion or question to be balloted had to be put in writing and signed by at least six fellows and delivered to a secretary. It would then be posted in the common room at the next meeting and be balloted on at the meeting after that. At the next council meeting, Maty moved that the opening words of the resolution be deleted: "That the Meetings of the Society may not be wasted by unprofitable debates contrary to the intent & meaning" of the statutes of the Society. He was voted down.[30]

The new statute requiring all motions to be announced in advance did not produce the desired calm. Duly announced was a motion to reinstate Hutton in his office. It and motions

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[26] *Narrative*, 26–77. *Supplement*, 9. Despite charges to the contrary, in the Royal Society at this time the physical sciences were active and appreciated. At the St. Andrews Day meeting for elections on 1 Dec. 1783, Banks gave a discourse on two Copley Medals, one awarded to John Goodricke for his paper on the variation of the star Algol, the other to Thomas Hutchins for his experiments on freezing mercury, which Cavendish directed. 1 Dec. 1783, JB, Royal Society 31.

[27] *Narrative*, 30, 70.

[28] Charles Blagden to Claude Louis Berthollet, 13 Jan. 1784, draft, Blagden Letterbook, Yale. 31:265, 268–271. On 27 Nov. 1783, the reading began of John Michell's paper (1784).

[29] Paul Maty to Joseph Banks, 15 Jan. 1784; Joseph Banks to Paul Maty, 15 Jan. 1784, BL Add Mss 33977, 257 and 257(2).

[30] 22 and 29 Jan. 1784, Minutes of Council, Royal Society 7:154, 157 (University Publications of America microfilm edition).

to restrain Banks's interference with elections led predictably to renewed debates in late January and February.[31] At a meeting in March, Maty gave a speech and then went on to read papers, as was his duty. Horsley was at that meeting but few of his supporters came, and Banks took encouragement.[32] Maty, who had "distinguished himself by his violence against Sir Jos: Banks," in Blagden's words, resigned as secretary of the Society.[33] Banks sent another card to all members of the Society on 29 March to inform them of the vacancy and to say that "at his desire," Blagden had declared himself a candidate for the office. Banks's opponents took fresh offense, referring to Banks's card as his permission to elect, or as they put it, the "President's Congé d'Elire."[34]

Following the row over the election of Maty's replacement, new contingency plans were laid, with Cavendish again taking part and for the same reason. On Monday, 5 April, Blagden told Banks that Cavendish and his friend Alexander Dalrymple had accompanied him home that evening to determine the "proper measures for preventing a few turbulent individuals from continuing to interrupt the peace of the R.S." Cavendish was willing to join a committee or to call a meeting to form a plan of action and draft appropriate resolutions. The general idea was that the committee would present the resolutions to a much larger meeting of members, the composition of which was to be decided by the committee. If the resolutions were acceptable to those members, they would be expected to vote for them at such times as the dissensions again interrupted the scientific work of the Society. From a list of members, Cavendish selected seven as being "proper" for drafting the resolutions. Heberden was one of them, and when Blagden said that Heberden probably would not join them, Cavendish offered to go to Heberden the next morning to try to persuade him. Cavendish had nothing against taking the lead except for his general "unfitness for active exertion."[35] That evening Cavendish wrote to Blagden: "It is determined that Mr Aubert & I shall go to Dr Heberden & see what we can do. If it is to no purpose a larger meeting will be called & very likely some resolution similar to what you mentioned proposed to them."[36] To "render the R.S. more peaceable," Blagden wrote to Banks, Cavendish called not only on Heberden but also on Francis Wollaston and Alexander Aubert, and he was going to write to William Watson, all of whom were on Cavendish's list of seven. He called for the meeting to take place in his house and settled on a time for it.[37]

That is the last we hear of Cavendish's efforts to restore peace in the Royal Society. One month later the Society voted for the secretary to replace Maty. Hutton, the deposed foreign secretary and still the primary rallying cause for Banks's opponents, ran against Banks's man, Blagden. The vote was again not close, 139 to 39, roughly 3 to 1 in favor of Blagden. Given that Banks had endorsed Blagden, and that Blagden had served throughout the stormy times as his proxy, Banks in effect had made the election of the secretary a vote of confidence in his presidency.[38]

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[31] Weld (1848, 2:162–164). *Narrative*, 79–134.

[32] Joseph Banks to Charles Blagden, 6 Mar. 1784, Blagden Letters, Royal Society, B.26.

[33] Charles Blagden to le comte de C., 14 May 1784, draft, Blagden Letterbook, Yale. 1 Apr. 1784, Minutes of Council, Royal Society 7:160 (University Publications of America microfilm edition).

[34] Weld (1848, 2:165). *Supplement*, 12.

[35] Charles Blagden to Joseph Banks, 5 Apr. 1784, BM(NH), DTC 3:20–21.

[36] Henry Cavendish to Charles Blagden, Monday evening [5 Apr. 1784]; in Jungnickel and McCormmach (1999, 586).

[37] Charles Blagden to Joseph Banks, 6 Apr. 1784, BM(NH), DTC 3:25–26.

[38] Weld (1848, 2:165–166).

The turmoil of the Society was reflected in the *Philosophical Transactions*, the printing of which was held up, and volume 73 for 1783 was a mass of "confusion." Cavendish's paper on Hutchins's experiments on the freezing of mercury was printed out of order because Hutchins's own paper was mislaid by the secretary Maty. When after much delay Hutchins's paper was found, it was paginated with asterisks and then, unaccountably, inserted in the middle of Cavendish's paper. Different copies of the journal had different mistakes. Two years later Blagden was still picking up the pieces.[39]

Yet after the event, the dissensions seemed hardly more than a tempest in a teapot to Blagden. He was surprised that foreigners took such interest in that "foolish & trifling affair, as it really was with us."[40] He wrote to a foreign correspondent that the disaffected members of the Society had not only failed to unseat Banks but in the end had planted him in his seat more firmly than ever.[41] Most important, science had not stopped: to a friend, Blagden wrote that "notwithstanding the interruption given to our business in the Royal Society by some turbulent members [...] several valuable papers have been read, and some discoveries of the first magnitude announced," adding that "of these, the most remarkable was made by Mr.Cavendish."[42] Banks received a letter from abroad at this time, beginning with the observation that the Royal Society's dissensions had "made a good deal of noise on that Continent" and that Banks's report that the troubles were "nearly quelled" was welcomed, observing that Cavendish's discovery of the production of water from air was "one of the greatest steps that have been made" towards understanding the elements.[43]

The dissensions did not flare up again, but smoldering resentments continued to the end of Banks's long presidency. In late 1785 Blagden informed Banks about an alternative to the *Philosophical Transactions*, an "opposition Transactions," in which Maskelyne was involved, though Maskelyne denied that it had anything to do with the "late opposition." As far as Blagden had been able to learn, it was a work Hutton had undertaken to publish twice a year, and it would not be confined to mathematics. Blagden took to calling it the "*seceding Transactions*."[44] From 1784 there is evidence of a mathematical club that convened at the Globe Tavern on Fleet Street every other week on Fridays, not on Thursdays when the Royal Society and Royal Society Club met. To judge by its membership, which included Hutton, Maseres, and Maskelyne, it was an opposition dining society.[45] Some dozen years after his dismissal as foreign secretary, Hutton gave a bitter description of the Royal Society in his *Mathematical and Philosophical Dictionary*: "This once illustrious body," the meeting hour of which had been adjusted to the convenience of "gentlemen of fashion," now consisted mainly of honorary members, who did not usually communicate papers, and those members who did were discouraged "by what is deemed the arbitrary government of the society," and in consequence the *Philosophical Transactions* had "badly deteriorated."[46]

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[39] Charles Blagden to le comte de C., 2 Apr. 1784, draft; Charles Blagden to John Michell, 13 Sep. 1785, draft; in Russell McCormmach (2012, 395–400).

[40] Charles Blagden to Joseph Banks, 9 Aug. 1788, BL Add Mss 33272, 50–51.

[41] Blagden to le comte de C., 2 Apr. 1784.

[42] Charles Blagden to Charles Grey, 3 June 1784, draft, Blagden Letterbook, Yale.

[43] Henry Cavendish (1784a, 119–169); in *Sci. Pap.* 2:161–181; read 15 Jan. 1784. The Abbé Mann to Joseph Banks, 4 June 1784, published in Ellis (1843, 426–427).

[44] Charles Blagden to Joseph Banks, 23 and 30 Oct. 1785, Banks Correspondence, Royal Botanic Gardens, Kew 1:213–214.

[45] Derek Howse (1989, 161).

[46] Miller (1981, 289). Charles Hutton (1795–1796, 2:399–400).

Under Banks's presidency the Council of the Royal Society was dominated by aristocrats and gentry,[47] and we might expect Cavendish, as an aristocrat if not for other reasons, to have been on the Council during the dissensions. Before Banks became president in 1778, Cavendish had frequently been a member, but in the years following, 1778–84, he was a member only once. Had he been on the Council then, the charge that Banks ignored mathematical fellows would have been substantially weakened. Banks would not repeat that mistake; never again would he leave Cavendish's name off the house list. In 1785, the year after the dissensions, Cavendish was elected to the Council, as he was every year after that to 1809, just before his death.[48]

As an ordinary member without office, Cavendish had attended the meetings of the Society at which the debates took place. He seconded, undoubtedly by prearrangement, the motion approving Banks's presidency. He did nothing more during the debates, but that was all that was needed from him. First, he owed nothing to, and needed nothing from, Banks, and for him to act from reasons of personal gain would have been seen as acting out of character. Second, he was universally respected for his achievements in physical science, and he was also known to be a good mathematician. If Cavendish had sided with Horsley and his friends, mathematicians who styled themselves as the genuine scientific element of the Society, Banks's credibility would have been damaged. Blagden understood this, a reason why Cavendish was a key to his stratagems to save Banks's presidency, as his letters to Banks show. Cavendish's endorsement of Banks by seconding the crucial motion was a *scientific* answer to Horsley's characterization of Banks's men as feeble amateurs.

According to his critics, Banks showed favoritism to natural history, and considering that Cavendish worked in natural philosophy, he might be expected to have joined the opposition if he took any side at all, but if we look at Cavendish's actions in the Society, we see that he had always supported natural history. His many recommendations of voyagers of discovery for membership in the Royal Society were a show of support for natural history as much as for natural philosophy. He brought as his guest to the Royal Society Club Daniel Solander, a natural historian, who organized the natural history collection at the British Museum, and who worked as Banks's librarian.[49] For his part, Solander was a refutation of Banks's critics: he "takes an interest in all the sciences," Playfair said, "and is not of the number of those naturalists who, while they count the scales of the salmon, or inspect the wings of a butterfly, despise the labors of the moralist or the astronomer."[50] On many occasions, Cavendish brought as his guest to the Royal Society Solander's successor as Banks's librarian, the natural historian Jonas Dryander.[51] Cavendish himself worked in natural history from the side of the physical sciences as a collector of stones and minerals.

Blagden, in a letter of 2 April 1784 in which he referred to the politics of the Royal Society, wrote of the wider political scene: "our internal operations in politics, & the consequent general election, have set the whole kingdom in a ferment; it is a very interesting scene which the wisest & steadiest among us contemplate not without emotion."[52] Scientific politics and general politics were often compared in the course of the dissensions, one side

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[47] Miller (1981, 49).

[48] Cavendish was elected every year, and we assume that he was on Banks's lists.

[49] Archibald Geikie (1917, 117). Roy A. Rauschenberg, "Solander, Daniel Carl," *DSB* 12:515–517.

[50] Rauschenberg (1975, 515–517). Playfair (1822, 1: Appendix, no. 1, "Journal," lxxxii).

[51] 13 Dec. 1781, JB, Royal Society 30; 16 Jan. 1783, ibid. 31. "Dryander, Jonas," *DNB* (1st ed. 6:64).

[52] Blagden to le comte de C., 2 Apr. 1784. Writing to Banks three days later about the dissensions, Blagden added a postscript concerning the elections in London.

complaining of the "ruins of liberty," the other side of Englishmen "apt to be mad with ideas of liberty, ill understood."[53] The one side spoke of the "leveling spirit and impatience of all government which infects the present age," the "great evil and disease of the time." The other side spoke of the Royal Society as a "Republic," according to which all laws decided by the Council are debated by the entire membership whenever a mover and a seconder wish it.[54] The one side urged a democratic solution to the abuses of the Society, while the other warned of illegal "democratic infringements on the principles of the Constitution," which was "very much like what was passing in another place."[55] The analogy between the Royal Society and Parliament was made explicit. When speakers against Banks were shouted down and the question was demanded, Maskelyne protested that he had been at other meetings that modeled their debates after Parliament, and the question was not put until everyone had had a chance to speak.[56] The favorite analogy was between Banks as president of the Royal Society and the king or some high official. Horsley described Banks's call upon the members to elect Blagden as their secretary as a "nomination by the president, *as their sovereign*, of the person he would have them chuse which is exactly similar to the proceeding of the king in the nomination of a new bishop."[57] Horsley's colleague Maty said that he viewed the presidency of the Royal Society as a "presidency of bare order, like that of the Speaker of the House of Commons, and in Council the President ought not to lead more than any other person."[58] Banks's opponents spoke of his despotism, of his dictatorial ways, of his wish for dominion, and of his blindness to the reality that the age of absolute monarchs was past. But the supporters of Banks did not wish for an absolute monarch any more than his detractors did, and no one was more definite on the subject than Henry Cavendish.

In explaining Cavendish's behavior to Banks, Blagden drew the appropriate parallel between Cavendish's position in science and that of his relatives in politics. "The sum is," Blagden wrote to Banks, "that like his namesakes elsewhere, he [Cavendish] is so far loyal as to prefer you to any other King, but chooses to load the crown with such shackles, that it shall scarcely be worth a gentleman's wearing."[59] With regard to Cavendish's "grievance" against Banks, Blagden wrote again to Banks, "It is exactly the old story of an absolute Monarchy, whereas he [Cavendish] thinks the Sovereign cannot be too much limited." Putting a positive light on Cavendish's position, Blagden wrote to Banks after a meeting with Cavendish, "The utmost consequence will be, some diminution of power, but none of dignity."[60] That reassurance was important to Banks, who wore the red ribbon of the Order of the Bath to

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[53] J. Glenie's speech on 8 Jan, quoted in *Narrative*, 70. Blagden to Berthollet, 13 Jan. 1784.

[54] Blagden to Banks, 28 Dec. 1783. Letter written by Michael Lort to Bishop Percy, 24 Feb. 1784, at the height of the dissensions, quoted in Weld (1848, 2:169). Lort to Bishop Percy, 24 Feb. 1784.

[55] Anguish's speech on 12 Feb., quoted in *Narrative*, 112.

[56] Maskelyne's speech on 8 January, quoted in *Narrative*, 62. The Royal Society and Parliament were occasionally joined in the same person. C.J. Phipps, Lord Mulgrave, who was active both in the debates of the House of Commons and in the debates of the Royal Society, spoke with Blagden on the subject of the dissensions as much as "his present political agitation would allow." Mulgrave strongly urged Banks and his supporters against temporizing, since discontented men were "never made quiet by coaxing." Blagden, who used the analogy himself, thought that Mulgrave carried the analogy of "H[ouse] of C[ommons] ideas to our Society" further than was justified. Blagden to Banks, 23 Dec. 1783.

[57] Horsley's speech on 1 Apr., quoted in *Supplement*, 12.

[58] Maty's speech on 12 Feb., quoted in *Narrative*, 99.

[59] Blagden to Banks, 22 Dec. 1783.

[60] Blagden to Banks, 24 Dec. 1783.

meetings of the Society because he believed that the office he held deserved the utmost dignity.[61]

Cavendish exercised authority within the Society, but as we have seen in the episode of the dissensions, he did so unobtrusively. We take as an example a more routine disagreement. In 1793 William Charles Wells, an American-born physician then practicing in London and soon to become physician at St. Thomas's Hospital, was a candidate for membership in the Royal Society. There was a party against Wells, and Blagden asked members about him, finding that there was little in his favor and little against him. Blagden looked at Wells's book on vision published the year before, satisfying himself that the candidate was not a "man of mean understanding" nor one who had "confined his attention solely to medicine." That was the "state of things" when at the Royal Society Club Blagden "consulted" Cavendish and also another senior member, both of whom said that no opposition should be made, and "on their authority" all intention of soliciting votes against Wells was "given up."[62]

Nation

Henry Cavendish's political arena was the Royal Society as his family's was Parliament, but apart from the setting his political behavior was the same as theirs. We may compare him with an older first cousin William Cavendish, fourth duke of Devonshire, who in the political diary he kept revealed "complete self-assurance as to his place in the order of the world. He sits in [Privy] Council as naturally as at his dining-room table.… No maker or unmaker of ministries, he advised Kings about ministers, though his main concern was always to preserve harmony amongst His Majesty's servants." He had no intimate friends in political life. "This detachment was natural to him and inevitably confirmed his exalted station. Here however lay the key to Devonshire's usefulness, recognized by everyone. He was the supremely objective man, never led away by passion." Devoted to work and duty, everything he did he did well.[63] The characteristics of William—self-assured, conscientious, dispassionate, withdrawn, competent, and supremely objective—were those, by and large, of the Cavendish family including the member who distanced himself farthest from the active political life of the nation, Henry Cavendish. The family motto *Cavendo tutus*, a play on words meaning "safe by being cautious," was William's guide through life, as it was Henry's.

Henry Cavendish worked in committees, in agreement with his understanding that power should be exercised by councils of serious men of independent judgment. No "maker or unmaker" of presidents of the Society, he was ready to assist presidents as a call of duty, always in the interest science. This is seen in his participation in the events of 1783–84, which also shows that he had a clear-sighted understanding of political behavior; he was an objective observer of men as well as of nature.

Blagden, in his capacity as secretary of the Royal Society, wrote to a correspondent in 1789 that there was no science to report, that "everybody's attention seems turned to politics."[64] The next year he wrote that science throughout Europe was languishing and that the Royal Society had heard nothing important since William Herschel's paper on the

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[61] Cameron (1952, 158, 200).

[62] Charles Blagden to Joseph Banks, 8 Nov. 1793, BL Add Mss 33272, 127–128. William Dock, "Wells, William Charles," *DSB* 14:253–254.

[63] P.D. Brown and K.W. Schweizer (1982, 19–21).

[64] Charles Blagden to William Farr, 24 Jan. 1789, draft, Blagden Letters, Royal Society 7:206.

rotation of Saturn's ring, "the minds of men being turned to greater interests."[65] Two years later on a visit to France, Blagden was mobbed and nearly hanged. Banks wrote to him that in England "minds are much heated" by the "dreadful state into which reform has placed France," and he trusted that the English people would learn a lesson from it.[66]

Wilson's sources on Cavendish missed a side his nature he occasionally revealed. Kirwan wrote to Banks, "Mr.Cavendish talks politics," which surprised him because Cavendish had been "silent" during "Ld North's Rump Parliament, in wh his family were so much engaged," and which had "agitated the whole Nation."[67] Blagden wrote in his diary that at the George & Vulture, Cavendish was "freer than usual," saying that "minister & measures" had to be changed and that they "should have confidence in Fox."[68] Like his family, Henry stood by the brilliant and flawed Charles Fox, whose political address was, in effect, Devonshire House in London. Present during a conversation in which there was talk of war the sooner the better, Cavendish "said he could scarcely refrain from bursting out."[69] Blagden recorded a number of Cavendish's observations about war in his diary, and though in each instance the note is brief, they give us an idea of Cavendish's view of nations in conflict. Blagden laid out the arguments for setting on Prussia while holding out peace. "Never was a nation so mad," Cavendish responded.[70] The only possibility of a combined resistance to the French was by a "fair intelligence" between Prussia and Austria, Cavendish said, to which Blagden replied "impossible" because Austria's goal was to swallow up Prussia.[71] On the report of a new war with America, Cavendish said that the Americans were "now more moderate than their predecessors." Blagden disagreed on the grounds that Americans would hold onto their places at any cost, to which Cavendish "assented & looked in agitation." Blagden said that England had best turn into a nest of Pirates and war against all the world, and that England was likely to be at war soon with Russia. "To all this [Cavendish] sadly assented."[72] To Blagden's remark that all mankind had gone mad together, Cavendish "thought there was a great diminution of common sense in the world."[73] Taken together, these and other comments by Cavendish point to a man who looked to reason in human affairs and did not always find it.

If one looks at the dissensions of the Royal Society as a kind of experiment of the Enlightenment, a test of its core beliefs, the outcome is subject to interpretation. But it seems clear that through it all, Cavendish acted consistently upon certain of those beliefs. He trusted that disputes can and ought to be settled by discussion between men who are fair, moderate, informed, and willing to exercise their reason. In the eighteenth century, as in any other, a person who held that expectation of human nature was liable to disappointment from time to time.

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[65] Charles Blagden to William Farr, 31 July 1790, draft, ibid. 7:429.

[66] Charles Blagden to Joseph Banks, 5 Sep. 1792, BL Add Mss 33272, 107–108. Joseph Banks to Charles Blagden, 19 Feb. 1793, Blagden Letters, Royal Society, B.41.

[67] Richard Kirwan to Joseph Banks, 10 Jan. 1789, BM(NH), DTC 6:122–124.

[68] 16 Mar. 1795, Charles Blagden Diary, Royal Society 3:50(back).

[69] 20 Dec, 1795, ibid. 3:82(back).

[70] Ibid.

[71] 30 Nov. 1804, ibid. 4:286.

[72] 15 May 1806, ibid. 4:442.

[73] 3 April 1804, ibid. 4:217. This exchange on the unreason of people may not have had to do with politics, but it would apply.

Chapter 14
Air and Water

"Chemistry is the rage in London at present," John Playfair noted in his journal on a visit in 1782.[1] This observation sets the stage for Henry Cavendish's next course of experiments and series of publications. In Cavendish's time, a major achievement of chemistry was the distinction between various kinds of air, the first step in the chemistry of the gaseous state of matter. We have discussed Cavendish's paper on factitious air, published in 1766. When in 1771 the industrial chemist James Keir brought out an English translation of Macquer's five-year-old *Dictionary of Chemistry*, he made corrections and added material from Black, Macbride, and Cavendish, since by then a chemical dictionary had to include pneumatic chemistry to cover the "present state of chemical knowledge."[2] Two years later the president of the Royal Society John Pringle gave a "discourse" on the history of the subject.[3] In 1781 Tiberius Cavallo, an Italian physicist who lived in England, surveyed the field in a book, *Treatise on [...] Air*, in which he observed that over the last ten years, pneumatic chemistry had advanced more than had any other field at any time in so few years. When we see solids transformed into invisible airs and when we see airs lose their elasticity and turn into solids, we have a subject of "the profoundest contemplation, for a philosophical mind."[4] Pneumatic chemistry was an indispensable, expanding, and challenging branch of chemistry when Cavendish returned to it after his electrical researches.

Good Air

When Cavendish studied factitious air in 1766, phlogiston provided the framework. In the seventeen years between his first paper on air and his next, Priestley, Scheele, and others relying on the same framework had identified a number of new gaseous substances. The chemistry of phlogiston did not anticipate their discoveries but it proved capable of accommodating them. Yet in the end, it would be the chemistry of gases that most clearly revealed the limitations of phlogiston. In the 1780s, when Cavendish's last publications on chemistry appeared, phlogiston was on the defensive.

In this chapter we are concerned with the main components of common air, of which there are two, oxygen and nitrogen, which chemists at the time distinguished by the presence and absence of phlogiston, as their names indicated, "dephlogisticated air" and "phlogisticated air." We are also concerned with two other substances: inflammable air, one of the factitious airs Cavendish investigated in his first paper, which is our hydrogen, and "nitrous air," which is our nitric oxide, a new air then.

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[1] Playfair (1822, 1:xxxv).

[2] Pierre Joseph Macquer (1771, 1:iii–iv).

[3] John Pringle (1774), Supplement at the end of the volume.

[4] Tiberius Cavallo (1781, 797, 801).

Joseph Priestley was now Cavendish's most important colleague. The son of a cloth-finisher in Yorkshire, Priestley studied for the ministry; in 1767 he acquired a ministry in Leeds, where he began his serious investigation of airs. He first studied known airs, repeating experiments done by others, learning techniques that he soon put to original use. Cavendish, as we have seen, stored water-soluble airs over mercury, a technique which Priestley made a tool of discovery. He invented apparatus, devised clever experiments, and carried them out with skill; he examined airs for solubility, combustibility, respirability, density, and reactions with other airs; and he published regularly and often. In their ways of experimenting, Priestley and Cavendish complemented one another. Priestley experimented on many airs, which were often new, and he made measurements sparingly, whereas Cavendish experimented on a few known airs, and he made copious measurements.[5] Priestley expanded the field of pneumatic chemistry, Cavendish made it rigorous. Priestley was congenial and outgoing, Cavendish was guarded; Priestley initiated a correspondence on chemistry with Cavendish, but Cavendish did not keep up correspondences and he soon let this one drop. For Cavendish science came before other interests, and in the balance only science mattered, whereas Priestley had many interests outside of science: "let it be remembered, that the taste for science, pleasing, and even honourable as it is, is not one of the highest passions of our nature, and the pleasures it furnishes are even but one degree, above those of sense; and therefore that temperance is requisite in all scientific pursuits." Piety, friendship, and other avocations came before science. In support, Priestley quoted the psychologist David Hartley's advice to scientific investigators to take frequent breaks from their studies to attend to God and men and to resist the temptations of vainglory, self-conceit, arrogance, emulation, and envy.[6] Temperance in the pursuit of science, as advocated by Priestley, was contrary to Cavendish's practice, and perhaps it was an implicit rebuke. This difference may have colored their relationship on a personal level, though they valued and learned from one another's work (Figs. 14.7–14.8).

As Cavendish had noted in his 1766 paper on factitious air, in a major paper in the *Philosophical Transactions* in 1772 Priestley surveyed the field of pneumatic chemistry, adding to it a new substance, nitrous air (nitric oxide). To this, his first discovery, he had been partly guided by a conversation with Cavendish, which came about in the following way. Priestley was interested in an experiment by Stephen Hales in which common air was mixed with a colorless, insoluble air (nitrous oxide) generated from a certain pyrite and spirit of nitre, generating red fumes, which absorbed part of the common air. He mentioned the experiment to Cavendish, who said that other pyrites and metals would probably do just as well and that the red fumes might depend only on the acid, spirit of nitre. Priestley acted on this suggestion. Having no pyrites, he substituted metals which he placed in spirit of nitre, obtaining, he said, "what I wanted, and a good deal more."[7]

"I hardly know any experiment that is more adapted to amaze and surprise than this is," Priestley wrote in his paper in 1772, "which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is diminished by it."[8] This was the experiment with nitrous air and common air: when the two were mixed, the nitrous air combined with part of the

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[5] Aaron J. Ihde (1964, 40–50).

[6] Joseph Priestley (1767, xxii).

[7] Joseph Priestley (1772b, 210).

[8] Ibid., 212.

common air, producing brown fumes, which dissolved in water. He found that the reduction of common air was proportional to the fitness, or "goodness," of common air for breathing. This new way of "phlogisticating" air suggested to him a test for the goodness of air: he mixed known quantities of nitrous air and the air to be tested over water and then admitted the residual air into a graduated tube, for which he used the word "eudiometer." The nitrous test was soon taken up by chemists, who regarded it at once as a tool of science and a potential aid to public health.[9] The name eudiometer was retained after the method was extended to the analysis of gases in general.[10]

Upon combining different kinds of air, chemists observed a large change in volume, the basic understanding of which came about only at the very end of Cavendish's life. To look ahead, in 1809 Joseph Louis Gay-Lussac published the law of combining volumes, according to which gases combine in simple proportions, and their contraction upon combining bears a simple proportion to their original volume; two years later his law received a molecular interpretation by Amedeo Avagadro.

Priestley's work on airs in turn stimulated Cavendish to return to the subject, at first in connection with Priestley's nitrous air, the working agent of a new instrument. At the time of his first paper on air, fifteen years earlier, he estimated the combustible, or breathable, portion of common air by the loudness of the explosion when it was detonated with inflammable air, inventing, in effect, a crude sort of acoustic eudiometer.[11] The sense of hearing is discriminating, but when the stimulus is explosions, accuracy in making comparisons is limited. This is clear from the table that Cavallo made of Cavendish's observations of the comparative loudness of explosions upon mixing inflammable and common air in different proportions, on a scale of 1 to 10.[12]

Infl. Air	Common Air	Effects
1	9	Fired with difficulty, little noise
2	8	Fired easily, moderately loud
3	7	Loud
4	6	Louder
5	5	Same
6	4	Less loud
7	3	Gentle
8	2	Burned without notice

With a desire to improve on the method of loudness, Cavendish invented a mechanical apparatus to measure the strength of detonation of inflammable air with other airs, the pressure lifting a pivoted board to different heights.[13] Given his interest in the composition of the atmosphere, he welcomed the new instrument, the chemical eudiometer, for determining the breathable portion of air. In 1783, he published a paper on a "new eudiometer," which he

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[9] Cavallo (1781, 453–457).

[10] Ihde (1964, 47).

[11] Wilson said this technique might be called an "Acoustic Eudiometer" (1851, 41).

[12] Cavallo (1781, 665).

[13] Cavendish, Mss II, 5:130.

began: “Dr. Priestley’s discovery of a method of determining the degree of phlogistication of air by means of nitrous air, has occasioned many instruments to be contrived....”[14]

Among Cavendish’s scientific papers is a large bundle of small, carefully indexed sheets, over 400 in number, labeled “Experiments on Air.”[15] Here and there they bear dates, telling us that Cavendish began his new experiments on air in 1778 and effectively ended them in 1786, though the last group of sheets carries the watermark 1800. The account of the first experiment is accompanied by a drawing of a eudiometer, essentially two bottles inverted in water, one containing either dephlogisticated air or common air, the other nitrous air, the two connected by a siphon. (Figs. 14.1–14.3). With air collected from several gardens, William Watson’s, William Heberden’s, and his own, he subjected samples to repeated tests, varying the procedure and the apparatus.

An example shows how carefully Cavendish investigated the working of a eudiometer He wanted to know if any fixed air was produced by mixing common and nitrous airs. Before mixing them, he washed both airs with lime water to remove any fixed air that might already be present as an impurity. He then combined the two airs in a bottle inverted in a vessel of lime water, which he had filtered through paper to make certain it was clean, and set it by for a day. He observed no clouds or sediments “in the smallest degree,” which if present would have indicated fixed air. To make certain that the lime water was effective and not saturated by dissolved fixed air, he breathed through it, observing clouds formed from the fixed air in his breath. “Lest it might be supposed” that the clouds in the lime water were owing to a volatile alkali in his breath, he breathed the same way through distilled water” to which he had added a reagent, finding that no clouds were formed.[16] This test of a test of a test shows Cavendish’s circumspect awareness of experimental deception.

The main problem with eudiometers was that they gave irregular results. (No one at that time understood the varying reactions of nitric oxide and oxygen.) The eudiometer Cavendish preferred, as did several other chemists such as Cavallo and Jan Ingen-Housz, was invented by the Florentine Felice Fontana in 1775. Fontana succeeded where others had not in devising a eudiometer capable of giving consistent results when used in a prescribed manner. Cavendish called it “by much the most accurate” of any that had been published, the reason he preferred it. He altered Fontana’s instrument and the method of using it to make the results “still more certain and regular.” Fontana’s method was to measure the reduction in the volume of the gases upon the removal of the soluble part, which Cavendish found subject to “very considerable errors.” His own method was to determine the quantities of the airs not by volume but by weighing the vessels containing them underwater. He weighed the two airs separately and then he weighed the mixed airs, the difference giving the diminution upon mixing. He also found that his method of mixing the two airs was “rather more accurate” than Fontana’s, and to be certain of this point he tried combining Fontana’s method of mixing with his own method of weighing.[17] Most of his paper is about precautions to be taken to minimize the errors the method was liable to. They could arise from the order of mixing, the

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[14] Henry Cavendish (1783a, 127).

[15] Most of the sheets are small folded pairs, with an occasional sheet folded four ways. Cavendish (1784a, Cavendish Mss II, 5).

[16] Ibid., 27–28.

[17] Cavendish (1783a, 129, 137). The eudiometer Cavendish described in 1783 was not what later became known as the “Cavendish Eudiometer,” which the Cavendish Society adopted as its emblem in the early nineteenth century. The emblem is a pear-shaped, later version of the instrument, which Cavendish would not have recognized, an electrically detonated eudiometer invented by Alessandra Volta. Cavendish used an electrically detonated globe in

impurity of the water, the impurity of the nitrous air, the time taken to transfer the nitrous air to the respirable air, and the briskness with which the bottle containing the airs was shaken.[18] Cavendish investigated all of these variables, showing the kind of attention to detail that characterizes all of his chemical work.

Because the eudiometer was a measuring instrument, Cavendish defined a set of quantitative terms to use with it. He called a bottle of a size that held 282 grains of water one "measure"; in mixing airs, he used three bottles, holding three, six, and twelve measures. He defined the "test" of an air as the diminution that it and the nitrous air suffer upon mixing. He introduced a "scale" together with a "standard": the upper fixed point of the "scale" was the "standard" 1, which stood for the goodness of common air (a mixture of nitrogen and oxygen); the lower fixed point was the standard 0 of perfectly phlogisticated air (nitrogen). By this scale, the standard of pure dephlogisticated air was nearly 5. "If common air, as Mr. Scheele and La Voisier suppose, consists of a mixture of dephlogisticated and phlogisticated air," the standard of any sample of the air was proportional to the quantity of dephlogisticated air (oxygen) in it. In general, if the standard of a sample is S, it has S times as much dephlogisticated air as common air has. Cavendish's use of the word "standard" was clarified by Blagden in his instruction to Cavendish's translator. "Standard […] means properly that fixed measure to which others [of the samed kind] are compared, but in a more general sense is used by us to express the proportion which any thing [of whatever kind] bears to a fixed measure: thus if a mixture was made of 3 parts of gold & one of base metal, we might say the *standard* of the mixture was 3/4." As he had the fixed points of the thermometer scale, Cavendish wanted an agreement on the eudiometer scale. It was important for investigators of the purity of factitious airs to "reduce their observations to one common scale, as the different instruments employed for that purpose differ so much, that at present it is almost impossible to compare the observations of one person with those of another."[19]

Once he was assured that his eudiometer gave consistant readings, Cavendish applied it to a question of interest at the time, the constancy or variability of the atmosphere. In the year Cavendish began his researches on the eudiometer, 1781, Cavallo brought out his comprehensive *Treatise on* […] *Air*, which we draw on it for the state of the question when Cavendish addressed it. The Swiss physicist H.-B. de Saussure toured the Alps, finding the air purer at the middle altitudes than at the top or on the plain. Marsillio Landriani, an Italian physicist who coined the word "eudiometer," toured his country comparing the air at various places and altitudes; upon scaling a mountain he found the air to be purer the higher he went, though on Mt. Vesuvius he found the opposite. Wherever the air was reputed to be bad, Landriani found it to be bad, confirming his theory, leading Cavallo to conclude that he was biased and that his instrument was inexact. Jan Ingen-Housz, a Dutch physiologist and chemist, found the air at or near the sea purer than the air on land, and generally he found the air to vary in purity from place to place and from time to time. Cavallo thought that Ingen-Housz's portable barometer was inexact too. Fontana, who carried out

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his experiments involving the production of water, discussed later in this chapter, but he never referred to it as a eudiometer. Wilson (1851, 42–43). Kathleen R. Farrar (1963).

[18] A.J. Berry (1960, 58–59). Jan Golinski (1992, 125).

[19] The standard for an air containing less oxygen than common air is found by making an artificial mixture of common air and nitrogen, and adjusting the mixture until one measure of common air and a variable measure of nitrogen experience the same contraction, that is, have the same test. Cavendish gives a formula for calculating the standard in this case. If x is the quantity of nitrogen added to 1 part of common air, the standard of the air in question is $1/(1+x)$. Cavendish (1783a, 130–131, 141–142). Wilson (1851, 228).

his experiments before Ingen-Housz, said that accounts by several authors of the purity of air were “not to be depended upon,” because their methods were “far from being exact.” The great differences they found in the air from different countries and at different times were the “fallacious effects of uncertain methods.” His own measurements showed that air differed very little from place to place, but varied considerably from time to time and season to season. He also thought that people were wrong to believe that the purity of the air affected their health; what was unhealthy were vapors carried by the air, which could be noxious like “so many particles of arsenic swimming in the atmosphere.”

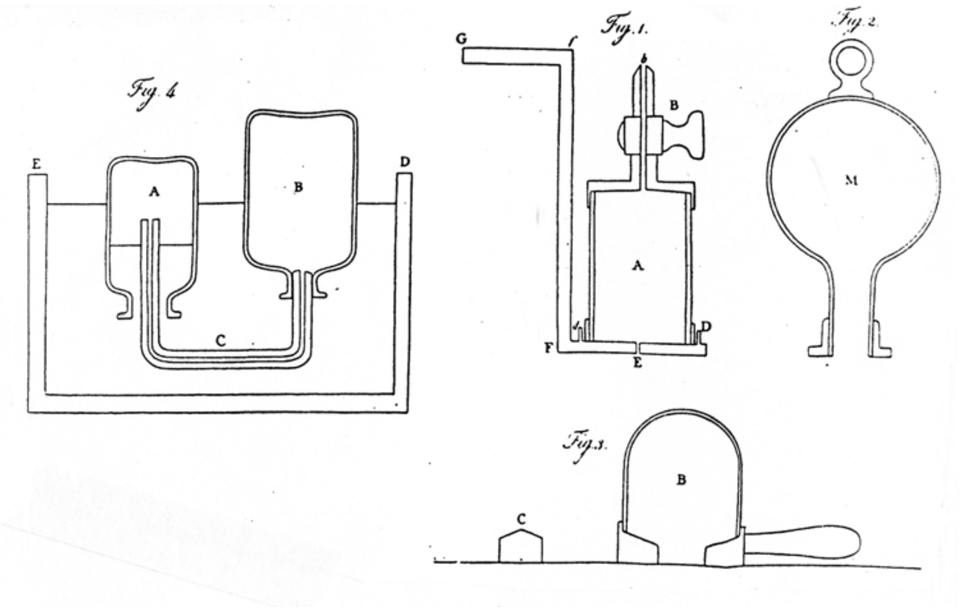

Figure 14.1: Eudiometer. Figure 1 shows the main apparatus, a glass cylinder A with brass cap and a cock at the top and an open brass cap at the bottom fitted into a socket of a bent brass holder as “a bayonet is on a musket.” The whole is submerged in a tub of water. Figure 2 is an inverted bottle for holding air, and Figure 3 is a standard measure of air. Cavendish’s method was to put a certain measure of nitrous air (nitric oxide) into the inverted bottle and a certain measure of common or dephlogisticated air into the glass cylinder. The cylinder was then set on the socket and the bottle over the cock, and the two kinds of air were mixed in the bottle. Figure 4 shows a different eudiometer. Bottle A contained common air, B nitrous air, which was slowly introduced through tube C into the common air without coming into contact with the water in the tub. Cavendish (1783a, 134).

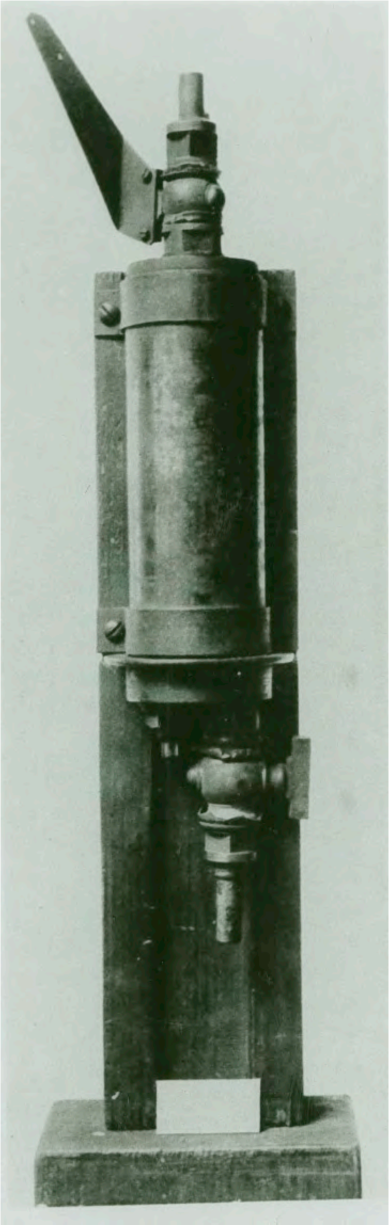

Figure 14.2: Eudiometer. The metal eudiometer belonging to Cavendish was presented by Humphry Davy to the Royal Institution, where the authors took this photograph. The instrument is about 6 inches long and 2 inches across. The stop-cock on the top served to fill and exhaust the cylinder, the one on the bottom to remove the water resulting from explosions of airs in the cylinder. Reproduced by permission of the Royal Institution of Great Britain.

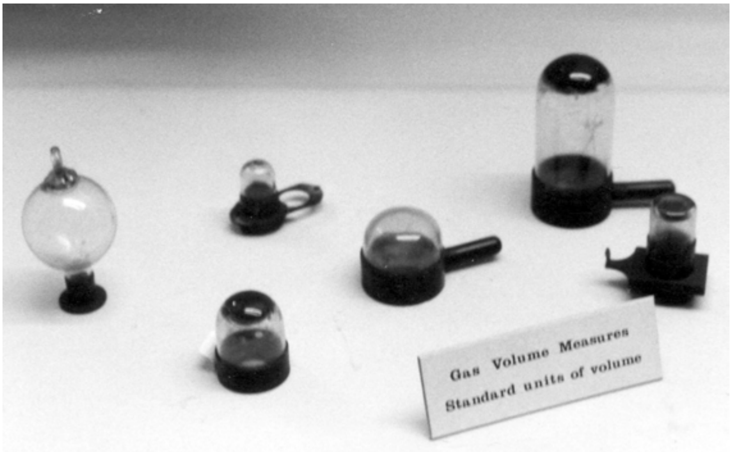

Figure 14.3: Standard Volume Measures for Air. These measuring glass vessels with brass caps on the bottom are described and drawn in Cavendish's paper on the eudiometer. Henry Cavendish (1783a, 106–135, Plate III). The standard volume measures are kept in the Royal Institution of Great Britain. Photograph by the authors. Reproduced by permission of the Royal Institution of Great Britian.

Cavallo accepted Fontana's conclusions about the purity of air in different places and at different times, and also about its irrelevance to problems of health, "but the essential part seems to be still in the dark; it is therefore requisite that philosophical people, in various parts of the world, would make as many and as various experiments, concerning the purity of the air at different times [...] in order to investigate the laws of those changes; which study is perhaps the most interesting part of the study of elastic fluids."[20] Cavendish made the experiments that Cavallo wanted, though his finding was not what Cavallo expected.

With his new eudiometer, Cavendish measured air taken in London and in Kensington under variable conditions, on clear, soggy, and wet days, and early in the day and late. Over a course of sixty days, during which he made no fewer than 500 trials, he concluded that within the error of measurement, there was no difference in the degree of phlogistication of the air from place to place and from time to time.[21] Twenty-four years later an author in the *Philosophical Transactions* wrote that Cavendish's "masterly analysis" of the air in London and Kensington had an "accuracy" that had been "more distinctly perceived the more the science of chemistry has advanced."[22] Subsequently chemists translated his results into terms and quantities corresponding to our understanding of the atmosphere: according to Cavendish, the concentration of oxygen in the atmosphere is 20.83%, which is remarkably near the currently accepted value of 20.95%. In making this comparison, it should be noted that Cavendish is credited with a somewhat greater precision than he would have claimed.[23]

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[20] Cavallo (1781, 458–467, 477). Felice Fontana (1779). Rembert Watermann (1968, 302–303).

[21] Cavendish (1783a, 140). Wilson (1851, 226–227).

[22] William H. Pepys (1807, 249).

[23] Separated off from his "Experiments on Air" is a 14-page paper containing eudiometer tests made in London, at his home at Great Marlborough Street, and in Kensington, "Miscellaneous Data on Eudiometer Experiments, 1780–81" (not Cavendish's label), Cavendish Mss II, 8. He continued his tests after moving to Hampstead in 1782,

To judge by the results he obtained with it, Cavendish's improved eudiometer was very good. Several years after his paper, Blagden advised Benjamin Thompson on instruments to acquire from England: "Of Eudiometers Mr. Cavendish's [...] is undoubtedly preferable to any other."[24] Although investigators would come to regard eudiometers as unreliable,[25] the interest in continuing to improve them remained strong for a good while to come, with claims made for their "precision and accuracy" and "present perfection."[26] In a discussion with Blagden on a paper about eudiometer tests made by Alexander von Humboldt, Cavendish referred to "my paper on Eudiometers," then fifteen years old. Humboldt tried the quantity of nitrous air remaining after the mixing, which had the "appearance" of an improvement, but it made the experiment "liable to the error of 2 operations instead of one." "However that may be, the great difference which he [Humboldt] finds in the purity of common air convinces me that there must be some fault in his method; for though I tried the air of 60 different days, I could not find any difference; & though a faulty method of trying will make the purity of the air appear different at different times when in reality it is not, I do not see how it can make it appear always the same when in reality it is different."[27] Cavendish was knowledgeable about sources of error, confident of his experiments, skeptical of methods resulting in conclusions that differed from his, and unexceptionable in his reasoning.

At the end of his paper on the eudiometer, Cavendish compared its action with the sense of smell. The eudiometer is not like the telescope, an instrument for extending the human senses, but on the contrary, the sense of smell can detect "infinitely smaller" quantities of impure air than can be measured by the eudiometer. Cavendish gave an example, no doubt drawing on his own experience: a person can detect ten ounces of nitrous air released into a twelve-by-twelve-by-twelve-foot room, a measure which would not alter the eudiometer test by more than 1/47,000th part, an immeasurably small quantity. What the nitrous test does show is the degree of phlogistication "and that only," a limitation which does not detract from the usefulness of the test; for our smell is no "test" of phologistication. There are ways of phlogisticating air that do not impart a smell to it, just as there are ways of imparting a smell that do not phlogisticate.[28] Cavendish's conclusion is a realistic affirmation of this instrument of measurement of limited sensitivity in science.

Around the time that Cavendish measured the composition of the atmosphere, it became a medium of human transport. The balloon was invented, and with it a new kind of adventurer came onto the scene, the "aeronaut." Balloons offer their passengers "scenes of majestic grandeur," inciting in them "enthusiastic rapture and pleasure," a balloon traveler wrote.[29] Much about this earliest human flight was derring-do and wonder, but there was a limited role for science in it too, both in the principles of flight and in the use of flight for carrying out meteorological measurements. A new field of applied pneumatic chemistry was born.

Cavendish was regarded as a founding father of balloon flight. From his description of inflammable air (hydrogen) in his first publication on air, it was self-evident to Joseph Black

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where he recorded "Register of Test Air," Cavendish Mss, Misc. There is another untitled manuscript comparing his, Fontana's, and Ingen-Housz's methods. Peter Brimblecombe (1977). Bent Søren Jørgensen (1967).

[24] Charles Blagden to Benjamin Thompson, 27 May 1787, draft, Blagden Letters, Royal Society 7:55.

[25] Golinski (1992, 93).

[26] Pepys (1807, 259). W. Allen and W.H. Pepys (1808, 249).

[27] Henry Cavendish to Charles Blagden, 18 Dec. [1798]; in Jungnickel and McCormmach (1999, 713).

[28] Cavendish (1783a, 144).

[29] Thomas Baldwin (1785, 2).

that balloons filled with this lighter-than-common air were a practical possibility. Black spoke about it with friends and in his lectures, but he did not do the experiment.[30] "Theoretical flying," Blagden said, "has been a topic of conversation among our philosophers as long as I can remember, at least ever since Mr.Cavendish discovered the great lightness of inflammable air."[31]

"Practical" flying was a French specialty. The relevant chronology of events is as follows. In 1782 the French brothers Joseph and Étienne de Montgolfier filled a small silk bag with heated air as an experiment. In June 1783 they gave a public demonstration: a large balloon was suspended over burning straw and wood, and upon release it rose several thousand feet and sailed about a mile and a half.[32] The brothers did not attribute the flight to the rarefaction of air with heat but to a light air given off by the burning material. When news of the experiment reached Paris, people came up with the idea of using inflammable air instead of hot air, and in August a large inflammable-air balloon was flown with considerable success. Animals soon were sent up with balloons and then people, the first successful manned flight taking place in November 1783. The same month saw the first balloon fly over England. The first man to fly there, on 15 September 1784, was Vincenzo Lunardi, secretary to the Neapolitan ambassador in London; his balloon was filled with inflammable air obtained by dissolving zinc in dilute vitriolic acid, the same way Cavendish obtained it in 1766. The second successful manned flight was made on 16 October 1784 by the professor of anatomy in the Royal Academy John Sheldon and the French inventor Jean-Pierre Blanchard, who had come to England to raise money. On 30 November 1784 Blanchard made a second flight with the American physician John Jeffries.[33]

Interested in the science of flight,[34] Cavendish was naturally interested in balloons from the start. When balloons appeared in the skies above England, Cavendish and his colleagues came out in force to observe them. From the top of Aubert's house at Austin Friar's, Cavendish and Blagden made observations of Lunardi's balloon every one or two minutes for above an hour. From a house on Putney Heath the next month, Cavendish and Dalrymple observed the balloon carrying Blanchard and Sheldon. Using a different method than the others, taking altitudes only, Cavendish calculated the height of the balloon as 3000 feet.[35]

Cavendish was not attracted to the adventure of balloon flight, and he did not go up in one, but he was interested in what he could learn from them. Through Blagden, he enlisted

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[30] In a letter from Joseph Black to James Lind, in William Ramsay (1918, 77–78).

[31] Charles Blagden to Le comte de Cat[–]lan., 2 Apr. 1784, draft, Blagden Letterbook, Yale.

[32] W.A. Smeaton (1974). Charles C. Gillispie (1983, 15–31).

[33] Charles Hutton (1795–1796, 1:35–39).

[34] For his sketch of Cavendish in 1845, Henry Brougham borrowed two manuscripts which are now lost: "Theory of Kites" and "On Flying." Their existence and loan to Brougham are noted in Cavendish's manuscripts at Chatsworth.

[35] Alexander Aubert to William Herschel, 13 Sep. 1784, Royal Astronomical Society, Herschel Mss M1/13. Charles Blagden to Joseph Banks, 16 Sep. 1784, Banks Correspondence, Royal Botanic Gardens, Kew, 1.173. Charles Blagden to Joseph Banks, 17 and 21 Oct. 1784, BM(NH), DTC 4:75–76, 77–78. Henry Cavendish, "Air Taken by Dr. Jeffries: Tried Dec. 3, 1784." The standard was taken of this air for several samples and compared with "Air Taken at Hampstead at the Time of the Trial." Two years earlier, samples of air from a balloon were compared with air "taken out at Mr. Cavendish's S. window at Hampstead at the same time. Nov. 28, 1782." Henry Cavendish, "Path of Balloon," for Blanchard and Sheldon's ascent on 16 Oct. 1784. Cavendish Mss VIII, 9, 24. Henry Cavendish, "Result of Observations of Balloons," Blagden Collection, Royal Society, Misc. Notes, No. 86. Cavendish's papers contain a testimonial signed by Benjamin Franklin, among others, of a Montgolfier experiment on 21 July 1783, and also an extract, in Blagden's hand, about Montgolfier from the *Journal Encyclopédique*. Archibald and Nan L. Clow (1952, 156).

Jeffries to sample the air during his flight with Blanchard: Jeffries took with him jars filled with distilled water, which he emptied at various heights, bottling the air. On the ground with his eudiometer, Cavendish tested the samples and compared them with air taken on the ground at Hampstead, establishing that there is little systematic variation with height in the concentration of dephlogisticated air (oxygen) in the lower atmosphere.[36] For Cavendish balloons were a means of elevating his scientific observatory thousands of feet above the Earth, their principal value.

The inflammable-air balloon was fully understood on the basis of weight, but the hot-air balloon raised a question. To decide if hot air alone caused the balloon to rise or if the balloon also depended on a substance lighter than common air given off by the burning material, as the Montgolfier brothers thought, Cavendish and Blagden collected air from burning straw and leather. Determining it to be a mixture of gases heavier, not lighter, than common air,[37] they concluded that hot-air balloons ascend solely because of the rarefaction of air.[38] In practical terms, hot-air balloons were impractical and perilous, and Blagden expected nothing from of them, but he thought that inflammable-air balloons could bring about an "important revolution in human affairs."[39]

Balloons fulfilled an age-old dream of flight, creating a sensation in France and mixed feelings in Britain. Not without a touch of envy, the British spoke of "Balloon madness" or else of missed opportunity. Banks said that it was to be hoped that the English would "not rise to the absurd height we have seen in France."[40] Blagden regarded Sheldon and Blanchard's flight a failure, their having made no observations and using a worthless barometer,[41] chalking it up to vanity and foolishness.[42] After 1785 the enthusiasm for balloon flight abated for a number of reasons: sated curiosity, danger, expense, and uselessness as transportation (ballons could not be steered but drifted with the wind). Very few "philosophical observations" were made from balloons besides those that Cavendish planned.[43]

Water

In 1784 Cavendish published a paper "Experiments on Air," which is remembered today for its "first clear and incontestable proof of the compound nature of water and of the nature and relative proportion of its constituents." The proof is clear, but Cavendish's explanation of his experiments is not. His primary interest was not the composition of water but the diminution in volume of common air in various chemical reactions, to which his paper on the eudiometer

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[36] He did not publish this finding, the credit for it going to Gay-Lussac for his research twenty years later. Henry Cavendish, "Eudiometer Results of Air Taken by Dr. Jeffries," and "Test of Air from Blanchard Balloon," Cavendish Mss II, 9. Thorpe (1921, 22). Jeffries's air samples were numbered, but because Cavendish's manuscripts do not contain the explanation of the numbers, the test was believed lost. However, recently it was located in Jeffries' account of his flight, from which the earliest atmospheric profile, the "Cavendish-Jeffries profile," has been reconstructed. It shows that at the various sampling elevations, between one and three kilometers, the amount of oxygen in the air over London was virtually constant. Brimblecombe (1977, 365).

[37] Notations in both Blagden's and Cavendish's hand, beginning "Smoke of Straw," Cavendish Mss Misc.

[38] Charles Blagden to Claude Louis Berthollet, 5 Dec. 1783, draft, Blagden Letterbook, Yale.

[39] Charles Blagden to Claude Louis Berthollet, 19 Dec. 1783, draft, ibid.

[40] Joseph Banks to Charles Blagden, 22 Sep. and 12 Oct. 1783, Blagden Letters, Royal Society, B.29–30.

[41] Charles Blagden to Joseph Banks, 24 Oct. 1784, ibid., 83–84.

[42] Charles Blagden to Joseph Banks, 26 Oct. 1784, Blagden Letters, Royal Society, B.32.

[43] Hutton (1795–1796, 1:139).

can be seen as a preliminary. The wider setting was investigations by Priestley, Lavoisier, and Scheele into the air lost during phlogistication.[44]

Observed to occur in many chemical processes, the loss of air was an important question, and chemists had different opinions on its cause. In his paper of 1772, Priestley discussed the prodigious loss common air experiences when it is combined with his newly discovered nitrous air or when a candle is burned in it, consuming a gallon of air in a minute. He was curious about the diminution of common air accompanying other processes too: breathing of animals, putrefaction of animal and vegetable substances, calcination of metals, and exposure of steel filings and pounded brimstone to air. Cavendish was helpful to Priestley again, giving him an account of experiments he had made on the diminution of common air when it was passed it through a red-hot iron tube filled with charcoal dust. Priestley concluded from the many experiments he had made that the "cause" of the diminution of air is the "same in all the cases," the phlogistication of the air: as he put it, when common air is diminished, it has more than its "usual quantity of phlogiston." The decisive experiment was the calcination of metals, a process in which metals give up their phlogiston to the diminished air.[45] Cavendish took this to be a description rather than an explanation, falling short of the understanding he was looking for. He posed a related but different question, which incorporated Priestley's finding: "to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated."[46]

The immediate occasion of his new research was experiments carried out by Priestley and his colleague John Warltire. When Priestley, in what he called a "random experiment," electrically fired a mixture of inflammable air and ordinary air or dephlogisticated air, Warltire observed that moisture was deposited in the vessel. Priestley did not consider the moisture significant, and Warltire was interested in the experiment for another reason. As reported by Priestley, when Warltire electrically fired a mixture of inflammable and common air in a closed vessel, he observed a generation of heat and light and a loss of weight, which he attributed to the escape of a ponderable matter of heat. This was in the spring of 1781.[47]

In his 1784 paper, after mentioning his unsuccessful attempts "to find out what becomes of the air lost by phlogistication," Cavendish proceeded "to some experiments, which serve really to explain the matter." His first experiment had been carried out in the summer of 1781, which appears in his laboratory notes as "Explosion of Inflam. Air by El. In Glass Globe to Determine Mr Warltires Experiment."[48] Cavendish took Warltire's conclusion about the ponderability of heat seriously enough to carry out experiments using different proportions of the airs, finding that no more than one fifth of a grain was lost and commonly none at all. The absence of a weight loss could not have surprised him, since he believed that heat is not a ponderable matter.[49] It was the dew that interested him. He found by experiment that all of the inflammable air and about one fifth part of the common air lost their "elasticity" and "condensed" into the dew lining the vessel, and that the weight of the

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[44] Cavallo (1781, 401–420). It is indicative of the activity in pneumatic chemistry that in his 1784 paper Cavendish referred to eight current investigators: Bergman, Kirwan, Lavoisier, Priestley, Scheele, Senebrier, Warltire, and Watt.

[45] Priestley (1772b, 162–163, 210–212, 225, 228, 232).

[46] Henry Cavendish (1784b, 161). Thorpe (1921, 23).

[47] Joseph Priestley (1781, 395–398).

[48] Cavendish (1784b, 165). His laboratory accounts: "Experiments on Air," Cavendish Mss II, 5:115.

[49] This discussion draws on Russell McCormmach (1969, 305).

airs that disappeared equaled the weight of the dew that replaced them. To examine the dew qualitatively, he changed the apparatus. By conducting the two airs into an eight-foot long, narrow glass cylinder and firing them, he obtained a relatively large amount of dew, which he tested, finding that it had no color, taste, or smell and left no sediment upon evaporation; that "in short, it seemed pure water."[50] The lost fifth part of the common air was the new air that Priestley had announced in 1774, which was discovered independently by Scheele, "dephlogisticated air" (Figs. 14.9–14.10). Today we would say that Cavendish synthesized water by combining hydrogen and oxygen.

We might expect that just as he and Joseph Black had replaced the ancient element air with distinct gases, he would announce that the ancient element water is a combination of airs, but it is uncertain if that is what he thought his experiments showed. He concluded that dephlogisticated air is "in reality nothing but dephlogisticated water, or water deprived of its phlogiston," and that inflammable air is either "pure phlogiston," as Priestley and Kirwan thought, or in all probability "phlogisticated water" or "water united to phlogiston."[51] This statement has been read in more than one way. Wilson in his biography of Cavendish was concerned to establish two points: that Cavendish was the first consciously to observe the synthesis of water from the two airs, in 1781; and that he was the first to recognize that water is a compound substance, by January 1783.[52] The first point is generally accepted; the second point is Wilson's interpretation. As evidence, Wilson quoted Cavendish: "These two substances [inflammable air and dephlogisticated air] united together form pure water." By this wording, Wilson said, Cavendish is on record as saying that water is a chemical compound of two simple material substances, inflammable air, our hydrogen, which he identified with phlogiston, and dephlogisticated air, our oxygen. As further evidence, Wilson cited a paper in which Kirwan stated that Cavendish believed that water is formed by the union of phlogiston with the dephlogisticated part of common air. Cavendish criticized Kirwan's paper but he let pass his statement.[53] A contrary reading is that Cavendish rejected the simple nature of dephlogisticated air and of inflammable air or phlogiston, attributing the simple nature to water instead. Cavendish's language would seem to support this interpretation too.

Thorpe coming to the subject seventy years after Wilson said that it was "impossible to gather from [Cavendish's] statement as it stands, whether Cavendish was convinced that water was actually a compound substance." Thorpe thought that the difficulty lay partly in the uncertainty surrounding Cavendish's understanding of phlogiston, whether it was a material substance or a quality that is transferred from body to body determining their natures. A contemporary historian of chemistry William Brock writes that for Cavendish, the production of water from hydrogen and oxygen "was not a synthesis of water at all; instead, as a phlogistonist, he preferred to see inflammable air as water saturated with phlogiston and oxygen as water deprived of this substance," and when the two were united, nothing remained but water, a "simple substance."[54] This is a paraphrase of what Cavendish wrote in

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[50] Cavendish (1784b, 167).

[51] Ibid., 171–173.

[52] Wilson (1851, 435).

[53] Ibid., 329.

[54] Thorpe (1921, 35). The chemist Berry, Cavendish's biographer, writes that there is ambiguity in Cavendish's statement, "I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred." Brock writes, "The difficulty is centred around the question as to what Cavendish understood by phlogiston. … He seems to have regarded hydrogen as a hydrate of phlogiston." Brock (1992, 110). Berry (1960, 86–87).

his paper of 1784 and a plausible alternative to Wilson's interpretation. Chemists at the time differed on Cavendish's meaning. Kirwan agreed with Wilson, and apparently so did Watt and Deluc, but the Swedish chemist Berzelius held the opposite interpretation. Cavendish who valued clarity of expression failed to meet his standard in this instance, as the history of his paper of 1784 proves.

Exactly what importance Cavendish attributed to the nature of water we probably can never know with certainty. The object of his experiments was to find the cause of the diminution of air by all the ways it can be phlogisticated, and the production of water gave him his answer. It was the kind of answer scientists like, a single cause for an effect brought about by different agencies, and the cause in this instance did not depend on deciding the nature of water. In arriving at his answer, Cavendish relaxed another standard of his, caution. Normally he did not draw conclusions beyond what his experiments allowed, but in the case of his experiments on the phlogistication of air, he did. As a phlogistonist, he supposed that inflammable air explains every case of phlogistication, and consequently that the phlogistication of air always diminishes the bulk of the air because it always produces water. As generalizations, these statements are incorrect. We know that not every body that can be oxidized contains hydrogen and yields water when oxidized.[55] He generalized before he made the required experiments.

What Cavendish thought he did in his experiments on air was important to Wilson because of the rival claims in the water controversy. The interpretation that mattered to chemistry was Lavoisier's, which was not long in coming. Wilson observed that if the "discovery of the composition of water" had remained within phlogiston chemistry, it would have made very little difference to chemistry. It would have meant only that instead of transferring phlogiston from one body to another, water would be transferred and be decomposed and composed, as required. The discovery needed Lavoisier to see its significance.[56]

Following his conclusion that dephlogisticated air is water deprived of phlogiston and that inflammable air is phlogisticated water, Cavendish commented on Watt's statement that water is "dephlogisticated air and phlogiston deprived of part of their latent heat." In his paper the year before on the freezing of mercury, Cavendish had stated his difference with Black on "latent heat." Latent heat having come up again with Watt, Cavendish again rejected it, explaining that he thought that heat is not a fluid, and that the admission of latent heat would encumber the "language" of chemistry. For example, diluted mineral acids would be said to consist of "concentrated mineral acids united to water and deprived of part of their latent heat," and a similar way of speaking would be needed for other chemical combinations, for almost all of them are attended with an increase or decrease of heat. The term "latent" would lead to "false ideas" in chemistry and "cause more trouble and perplexity than it is worth."[57]

In his paper of 1784 Cavendish said that he found no role for fixed air in the various instances of phlogistication of common air, contradicting Kirwan, who published a criticism. In a second paper that year Cavendish answered Kirwan, correctly refuting his claim. A possible value of this digression is that Kirwan's reference to the effect of passing an electric

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[55] Wilson (1851, 326–328).

[56] Ibid., 248.

[57] Cavendish (1784b, 173–174).

spark through air may have caught Cavendish’s attention. In any case, the electric spark was the agency of his second discovery, our next subject.[58]

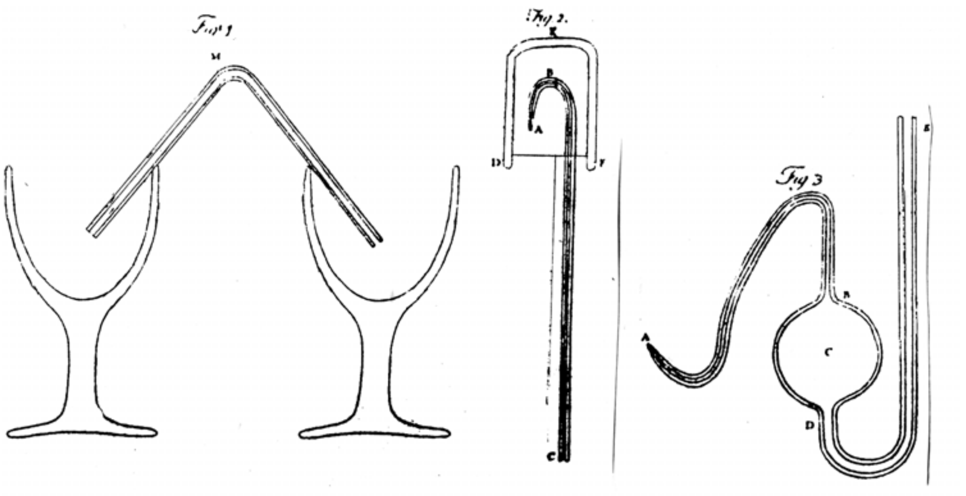

Figure 14.4: Apparatus for Experiments on Air. For converting phlogisticated air (nitrogen) into nitrous (nitric) acid, a spark is passed through air trapped in the bent tube shown in Figure 1. Filled with mercury, the tube is inverted into two glasses containing mercury. Figures 2 and 3 show small-bore tubes used to insert the nitrous air into the bent tube. “Experiments on Air,” PT 75 (1785): 348.

Nitrous Acid

Much of Cavendish’s paper of 1784 is taken up with a related question, the source of acidity of some of the dew. To further examine it, he repeated his experiments using a large glass globe; he found that when he substituted dephlogisticated air for atmospheric air in the experiments, the dew occasionally was acidic to the taste, and when the dew was combined with fixed alkali it yielded nitre, showing that it contained a small amount of nitrous acid. He also found that on these occasions, the dephlogisticated air was a little in excess of the inflammable air. He prepared dephlogisticated air from several sources, red precipitate, red lead, plants, and turbith mineral, finding that the condensed liquor was acidic in every case. He concluded that regardless of the source of dephlogisticated air, if it is in excess when it is exploded with inflammable air, the dew contains some nitrous acid. He explained the appearance of the acid drawing on his understanding that phlogisticated air is nitrous acid united to phlogiston: he supposed that dephlogisticated air (oxygen) contains a trace of phlogisticated air as an impurity, which it deprives of its phlogiston, turning it into nitrous (nitric) acid. By enhancing the effect, he produced the “decisive argument“: when he deliberately added a quantity of phlogisticated air (nitrogen) to the mixture of inflammable and dephlogisticated air and sparked the mixture, he found that the resulting dew was more

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[58] Henry Cavendish (1785). Thorpe (1921, 47).

strongly acidic. Because he clearly regarded water and nitrous acid as equally important products of phlogistication, until he understood both he was not finished with his investigation, accounting for the long delay in publishing his experiments on the production of water.[59]

The next year, 1785, Cavendish published a second paper under the same title, "Experiments on Air." In the first paper he said that an electric spark probably ignited some inflammable matter in the apparatus causing the airs to explode, but he had made no experiments. His new paper was about experiments he had recently made using the electric spark to detonate the airs. His apparatus was simple: a narrow glass tube bent at an angle was filled with mercury, and the two open ends were immersed in two vessels containing mercury. (The apparatus is similar to the one his father invented for Watson's experiment on electrical conduction across a vacuum.) He forced air (by an ingenious method) into the tube, collecting it at the bend, where he sparked it electrically. (Fig. 14.4). He found that dephlogisticated air by itself suffered very little diminmution in volume and phlogisticated air by itself none at all, but that a mixture of the two airs always did. When the mixture was five parts of dephlogisticated air and three parts of common air, nearly the entire air disappeared, leaving behind nitrous acid. Cavendish concluded from the experiments with the electric spark that dephlogisticated air and phlogisticated air "form a chemical combination," wording which shows that he viewed the two airs as distinct substances. He knew that the electric spark itself did not enter into chemical combination, and nothing in the experiments suggested that it contributed phlogiston, a subject of speculation at the time. His explanation was that dephlogisticated air aided by the heat caused by the spark deprived phlogisticated air of its phlogiston, reducing it to nitrous acid, agreeable with his explanation in his paper the year before. His new experiments on the production of nitrous acid paralleled those on the detonation of dephlogisticated air and inflammable air producing water, completing his investigation of the causes of the diminution of common air by phlogistication. To the interpretation of dephlogisticated and inflammable airs discussed above, another was added: the other part of common air, phlogisticated air, our nitrogen, is "nothing else" than nitrous acid united to phlogiston; Cavendish had said this in his paper the previous year, but now he had experimental proof.[60]

Word of Cavendish's new experiments traveled quickly. Two weeks after his paper was read to the Royal Society, Blagden heard from Berthollet that one part of Cavendish's experiments had been repeated in Paris, but given another interpretation: "we think that Mr. Cavendish has combined dephlogisticated air with phlogisticated air, instead of having decomposed the latter."[61] The different interpretations had to do with Cavendish's disagreement with Lavoisier on the formation of acids, described below.

In the course of his experiments, Cavendish had discovered nitrites and nitrates, which drew considerable interest and puzzlement. Lavoisier and his colleagues in Paris were unable to repeat the experiments; Cavendish could not imagine why but for "want of patience." In Holland Martin van Marum also failed, even with the help of his new electrical machine,

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[59] Cavendish (1784b, 169–171). Thorpe (1921, 33). Wilson (1851, 442). Berry says that "nowhere in his chemical work does the genius of Cavendish appear more clearly" than in his explanation of the appearance of nitric acid upon exploding the gases with an excess of oxygen. (1960, 73).

[60] Cavendish (1785, 191, 194).

[61] Berthollet told Blagden that his letter created great interest in Paris in Cavendish's "beautiful experiments." Claude Louis Berthollet to Charles Blagden, 17 June 1785, Blagden Letters, Royal Society, B.126.

the largest in existence; Cavendish again did not know why, though he thought that the apparatus might be faulty.[62] Instead of guessing what went wrong in experiments by others, Cavendish demonstrated what was right in his own. At his request, the clerk of the Royal Society George Gilpin repeated the experiments using the same apparatus during several days in late 1787 and early 1788, witnessed by ten or more fellows of the Royal Society, most of whom came to each part of the experiment.[63] Gilpin worked Nairne's electrical machine a half hour at a stretch, obtaining 2 or 300 sparks a minute, whereas Cavendish had only worked his machine for ten minutes at a time, but details of procedure aside, Gilpin's experiments fully confirmed Cavendish's. These repeated experiments were the substance of Cavendish's last publication in chemistry, in 1788.

Cavendish's contributions to pneumatic chemistry were widely separated, the first in 1766, the second in 1781–85. The first contribution was basic to the development of chemistry as a science; following Hales's early experiments on fixed air, it together with Black's study of magnesia alba opened a necessary field of investigation. His later contributions—the constancy of the composition of the atmosphere, and the conversion of airs into water and nitrous acid—were important too, though they were among other important contributions by a number of able investigators in a field that was well established. What was most important about Cavendish's last papers on chemistry may have been the example: to have studied them was to have taken a master class in the art of experiment. Jean Senebier, a Swiss pastor who examined the oxygen given off by plants, and who published insightful essays on the experimental method, wrote to Cavendish after reading his recent papers on airs to express his admiration for Cavendish's "exactitude," characterizing him as "a master and a great master in the difficult art of making experiments."[64]

Atmosphere

Daniel Rutherford, Black's and Cullen's student, wrote his medical dissertation in 1772 at the University of Edinburgh on Black's fixed air or, as Rutherford called it, "mephitic air." In the course of his experiments, Rutherford isolated another, similar air, phlogisticated air, our nitrogen, which he distinguished from fixed air, though he considered it as common air saturated with phlogiston rather than as a distinct air. Because Rutherford's dissertation was published, he is given credit for discovering nitrogen, but many years earlier Cavendish had studied this air. In a paper written for a correspondent, who had shown him a letter from Priestley discussing "mephitic air," by which Cavendish understood Priestley to mean air that "suffocates animals," Cavendish said that "in all probability there are many kinds of air which possess this property," and he knew of at least two, Black's fixed air and common air in which something had burned, or "burnt air" (nitrogen). Cavendish gave his correspondent the results of an earlier experiment of his, in which he reduced the volume of common air by passing it through a red-hot tube containing charcoal dust (the experiment mentioned earlier,

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[62] Martin van Marum to Henry Cavendish, 6 Jan. 1786; Henry Cavendish to Martin van Marum, undated, draft; in Jungnickel and McCormmach (1999, 622–625). Cavendish published this letter in his paper, "On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitrous Acid by the Electric Spark," (1788b, 232).

[63] The witnesses were Banks, Blagden, Heberden, Watson, John Hunter, George Fordyce, J.L. Macie, and Johann Caspar Dollfuss; William Higgins and Richard Brock came on the day after an "accident" happened, and Cavendish did not list them in his paper. T.S. Wheeler and J.R. Partington (1960, 33, 66).

[64] Jean Senebier to Henry Cavendish, 1 Nov. 1785; in Jungnickel and McCormmach (1999, 611–618)

given to Priestley). He removed the fixed air produced by the charcoal and measured the specific gravity of the remaining burnt air. What he referred to as "common air which has suffered a change in its nature from the fire" was nitrogen, which he gave the "first clear description" of. This paper by Cavendish is undated, but Priestley gave a version of it in his paper of 1772.[65]

In his paper of 1785, Cavendish said that little was known about the "phlogisticated part of our atmosphere," not even if there are "in reality many different substances confounded together by us under the name of phlogisticated air." To see if the phlogisticated air of the atmosphere contained anything other than "nitrous acid united to phlogiston," he repeatedly sparked a mixture of common air and dephlogisticated air until he was unable to diminish the volume any further. Using a standard method, he removed any remaining dephlogisticated air from the residue, leaving a small "bubble" of air unabsorbed in his apparatus, no more than 1/120 part of the whole, which he probably regarded as an experimental error or an impurity. A hundred years later, the bubble was recalled by William Ramsay, who had read George Wilson's biography of Henry Cavendish when he was a student. He drew the passage to the attention of J.W. Strutt, Lord Rayleigh, who like Ramsay was concerned with a third decimal difference in the density of the nitrogen in the atmosphere and the density of the nitrogen produced chemically.[66] On a larger scale, they repeated Cavendish's experiment, recognizing that Cavendish's bubble contained a new gas of the atmosphere, the chemically inert argon, later determined to contain traces of four other gases. In a jointly authored paper on the new inert gas argon in 1895, Ramsay and Rayleigh paid tribute to their predecessor: "Attempts to repeat Cavendish's experiment in Cavendish's manner have only increased the admiration with which we regard this wonderful investigation. Working on almost microscopical quantities of material, and by operations extending over days and weeks, he thus established one of the most important facts in chemistry. And what is still more to the purpose, he raises as distinctly as we do, and to a certain extent resolved, the question above suggested," the possibility that atmospheric nitrogen contains another gas.[67] In his history of the gases of the atmosphere, Ramsay observed that of all the experimenters, Cavendish was "undoubtedly the greatest."[68]

If Cavendish's later work is looked upon as a kind of chemical meteorology, it takes on an additional significance. The title he gave to his two chemical papers in 1784 and 1785, "Experiments on Air," referred to common air, the air of the atmosphere, a mix of dephlogisticated and phlogisticated airs. Cavendish intended the first paper to "throw great light on the Constitution and Manner of production of dephlogisticated air."[69] In his paper of 1785 he had a similar objective, only this time the air was phlogisticated air, the other half of common air. Blagden sent his brother three papers by Cavendish and Watt, which taken together seemed to him "fully to explain the nature of our atmosphere," noting that the most

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[65] Henry Cavendish, "Paper Communicated to Dr Priestley," Cavendish Mss, Misc. Vernon Harcourt, Presidential Address, British Association Report (1839), 3–68, on 64. Scheele too studied this gas, perhaps as early as 1771, but he did not publish his results until 1777. E.L. Scott (1975). Priestley came upon it independently too. Ihde (1964, 38).

[66] There are two versions of the way Cavendish's experiment came to the notice of Rayleigh. We have given Ramsay's. Rayleigh's was that he was first informed of Cavendish's experiment by James Dewar. Morris W. Travers (1956, 100–107).

[67] Berry (1960, 178–179).

[68] William Ramsay (1896, 143). Bruno Kisch (1965, 8).

[69] Cavendish (1784a, 161).

important of the three was Cavendish's paper on the origin of nitrous acid (and not the paper on the production of water, if that was one of the three papers), for it showed that the greatest part of the atmosphere "is nothing but that acid in aerial form."[70] Likewise Priestley wrote to Cavendish that his study of phlogisticated air was "one of the greatest, perhaps the very greatest, and most important, relating to the doctrine of air."[71]

We conclude our account of Cavendish's late chemical researches on air by reviewing his long-standing interest in the atmosphere. He improved the eudiometer, tested the composition of the atmosphere, and studied the nature of its components. At the request of the Royal Society he examined its meteorological instruments, and under his direction the Society instituted regular observations with their use. He experimented extensively with his own meteorological instruments as well. He measured the specific heat of air and the expansion of air with heat. He examined the hot-air balloon and observed the flight of balloons. Because of reports of extraordinarily low temperatures of the atmosphere in Russia, he made a study of the freezing point of mercury, the usual expansive agent of weather thermometers. He measured the temperature of spring water and water from deep wells as a means of determining the average climate of different regions. In connection with his geological tours he determined elevations on the Earth by means of a barometer, the measurer of the pressure of the atmosphere. He made optical studies of the refraction of light by the atmosphere. He studied the communication of electricity to the air. With reference to Newton's work, he made mechanical studies of the effect of the resistance of air on the trajectory of projectiles, of the motion of sound, and of the law of force of air particles responsible for Boyle's law.[72] Given what was knowable about the physical and chemical behavior of air in his time, there was little that Cavendish did not know firsthand.

New Chemistry

Had there been no "chemical revolution," the progressive development of techniques in chemistry in the eighteenth century would have gone on as it had under the old chemistry. But there was a chemical revolution—an assertion which is accepted by most historians of chemistry even as they disagree about what the revolution was, what its boundaries were, and what place the overthrow of phlogiston had in it[73]—and consequently the historical interest in Cavendish has been largely in relation to that event. Cavendish's contribution to chemistry was not among the conceptual changes that marked the chemical revolution. In contrast to Cavendish, Lavoisier broke with the science he had started out with; from the early 1770s he consciously strove to bring about a "revolution" in physics and chemistry, and twenty years later he had accomplished it or, depending upon one's interpretation, he had completed the first part of the revolution (Fig. 14.6).

A change of this magnitude in chemistry required a number of developments, one of which was pneumatic chemistry, which replaced the idea of elementary air with that of

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[70] Charles Blagden to Thomas Blagden, 8 Dec. 1785, Blagden Letterbook, Yale.

[71] Joseph Priestley to Henry Cavendish, 30 Dec. 1784. Priestley's letter was in reply to Cavendish's, written in late 1784, which summarized the main points of what would become the published paper of the following year. Henry Cavendish to Joseph Priestley, 20 Dec. 1784, draft; in Jungnickel and McCormmach (1999, 598–599, 602–603).

[72] Henry Cavendish, "Projectiles," "On the Motion of Sounds," Cavendish Mss VI(b), 14, 35. Cavendish (1771, 43).

[73] "Introduction," A.L. Donovan (1988, 5–12, on 5–6); Siegfried (1988, 34–50, on 34–35).

chemically active, distinctive airs, or the gaseous state of matter. Cavendish's production of water from gases was important for Lavoisier, who saw that it showed that water is a compound, giving him the answer to the critical question of what happens when metals are dissolved in acids: the inflammable air, or hydrogen, that flies off does not come from the metals, as the phlogiston theory said, but from the dissociated water. According to Thomson, Cavendish's discovery and Lavoisier's subsequent experiments "contributed more than any thing else to establish the antiphlogiston theory."[74] In order to build as well as destroy, Lavoisier had to work out a new chemical understanding and a new nomenclature to express it, and he had to win disciples. These things, of course, he did. His *Traité élémentaire de chemie* in 1789 would instruct the next generation of chemists in the new chemistry.[75]

Lavoisier was slow to recognize the importance of Cavendish's early chemical work. In his *Essays Physical and Chemical*, published in 1774, he showed a full appreciation of the work of Hales, Black, and Priestley, while his discussion of Cavendish was relatively brief. A historian of chemistry writes, "What is difficult to understand is how Lavoisier failed to grasp the significance of the work of Cavendish," which should have impressed him, especially Cavendish's determination of the densities of inflammable and fixed air, given the value Lavoisier placed on quantitative work in chemistry. Copies of Lavoisier's book were sent to the Royal Society and to Priestley, but not to Cavendish. What impressed Lavoisier at the time was Priestley's paper of 1772: more than any other modern work, it showed "how many new roads philosophy and chemistry still point out to travel over."[76] As a stimulus to a chemist who saw his vocation as the remaking of his field, Cavendish's careful measurements could not compete with Priestley's discovery of a new air and an up-to-date review of the field.

At the time of his new experiments on air, Cavendish was familiar with Lavoisier's efforts to eliminate phlogiston from chemistry and to introduce oxygen in its place. At the end of his paper of 1784, he conceded that the phenomena of nature could be about equally well explained on Lavoisier's chemistry as on the old. To show this, he described the phenomena of interest using Lavoisier's "theory." He did not adhere strictly to the theory, and he allowed additions and alterations to suit the phenomena, but he came close. By Lavoisier's "hypothesis," he said, water consists of "inflammable air united to dephlogisticated air," and he gave comparable reformulations of the composition of airs, acids, and metallic calces. Cavendish thought that it would be "very difficult to determine by experiment which of these opinions is the truest," but there was "one circumstance, which though it may appear to many not to have much force, I own has some weight with me." This was that on the phlogiston theory, plants give off phlogiston when they are burned, and it seemed obvious to Cavendish that plants are more compounded than their ash; on Lavoisier's theory, the ash, containing oxygen, is the more compounded.[77] Cavendish's "new modelled" phlogistic theory of 1784 was impossible to refute at the time, and French chemists did not try.[78]

It might seem that Cavendish was slow to see the superiority of Lavoisier's chemistry, but this would a misreading of the state of chemistry in 1784. It was not until the following year that the first French chemist, Berthollet, agreed with Lavoisier on the need to give up

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[74] Thomas Thomson (1830–1831, 2:115).

[75] Changes that underlay the Chemical Revolution are summarized in Brock (1992, 84–85).

[76] Greenaway (1776/1970, xxiii, xxix).

[77] Cavendish (1784b, 179–181). Thorpe (1921, 37).

[78] Thomson (1830–1831, 2:136–137).

phlogiston.[79] That year Blagden wrote to Berthollet that with Cavendish phlogiston was a "doubtful point." Whether the "old hypothesis of p" is right or Lavoisier's hypothesis that dephlogisticated air is a "simple substance," Blagden said, is a "question which I think cannot remain long undecided."[80] He thought that the English had not yet "given up" on phlogiston, and he mentioned its advocacy by Kirwan, explaining "it belongs to the temper & character of the philosophers of this country" to retain a familiar hypothesis "as long as they can explain the phenomena upon it." Of the recent work in France, Blagden wrote to Priestley, a staunch proponent of phlogiston, "I will not say [it] *overturns* the doctrines of phn but shakes it to its very foundations."[81] In a letter to William Cullen about the "question now warmly agitated relative to the existence of phlogiston," Blagden said that "which ever of the two systems, Stahl's or Lavoisier's," was adopted, Cavendish's experimental work was of equal importance in either.[82] Two years later, in 1787, Blagden told Berthollet that his memoirs had answered the "principal objections made by the supporters of the old doctrine of phlogiston." The arguments of the new chemistry were so much clearer than those of phlogistic chemistry that the "combat must soon be at an end."[83] In these letters written at the turning point of the Chemical Revolution, Blagden was expressing his own opinion, which was that of a convert, but we wonder to what degree, if any, it was in opposition to the opinion of the chemist he worked with daily. Following Cavendish's lead in his paper of 1784, when describing Cavendish's work to others, Blagden gave both explanations, the old and Lavoisier's.

If Kirwan is to be believed, by the time of the new chemical nomenclature, Cavendish had already given up the old chemistry. In a postscript to a letter of one of the authors of the *Nomenclature chimique*, Guyton de Morveau, Kirwan wrote that "Mr. Cavendish has renounced phlogiston." Kirwan did not give his source or elaborate, but what he said is consistent with what Blagden had been saying to and about Cavendish. The date was 2 April 1787, only a few weeks after Marum had told Lavoisier that he rejected phlogiston. Cavendish and Marum would seem to be the first chemists outside of France to abandon the foundation of the old chemistry, phlogiston, but there would soon be many.[84] In late 1787, Cavendish was busy disseminating the new chemistry in London: having received a bundle of Berthollet's anti-phlogistic memoirs sent over with the *Nomenclature chimique*, he directed the publications to the "difft gentl for whom they were intended," himself included, "all in the best manner he was able."[85] In 1788–89 the major exponents of the anti-phlogistic chemistry in France were elected foreign members of the Royal Society. Cavendish signed all of their certificates, and in the case of the leading chemist among them, Lavoisier, he was the *first* to sign.[86]

At the end of his paper in 1784, where he said that the principle of phlogiston and Lavoisier's hypothesis seemed to work about equally well, Cavendish raised a difficulty for Lavoisier's idea of dephlogisticated air, or oxygen, as the "acidifying principle." For

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[79] Ibid. 2:101, 130.

[80] Charles Blagden to Claude Louis Berthollet, 21 and 24 May and 28 June 1785, drafts, Blagden Letterbook, Yale.

[81] Charles Blagden to Joseph Priestley, 11 June 1785, draft, ibid.

[82] Charles Blagden to William Cullen, 5 July 1785, draft, ibid.

[83] Charles Blagden to Claude Louis Berthollet, 17 Nov. 1787, draft, Blagden Letters, Royal Society 7:85.

[84] Scottish chemists were receptive. Black early lectured on the new chemistry, though he did not commit himself until 1790. Thomas Charles Hope, who succeeded him at Edinburgh, lectured on the new theory after 1787.

[85] Blagden to Berthollet, 17 Nov. 1787.

[86] 3 Apr. 1788, Certificates, Royal Society 5.

some acids it worked, Cavendish said, but not for all, in particular not for marine acid, our hydrochloric acid,[87] and this was correct. The chemist Thomas Thomson thought that if the chemical world had not paid "total inattention" to Cavendish's criticism in 1784, the success of the anti-phlogistic school would not have been as rapid as it was.[88] If Cavendish abandoned the phlogiston theory three years later, as we have reason to think he did, he did not necessarily subscribe to Lavoisier's viewpoint, certainly not entirely. It is likely that he accepted the version he gave in his paper in 1784, or part of it, and that he qualified Lavoisier's acidifying principle.

Cavendish had strong feelings about the language of chemistry, as we know from his correspondence with Blagden, who was away from London on the French and English triangulation project in 1787. The French party crossed the Channel carrying anti-phlogistic chemical publications including a copy for Cavendish of the new *Méthode de nomenclature chimique* written by Lavoisier and his colleagues.[89] Having read the preface, Cavendish wrote to Blagden that the nomenclature was a move to impress the new theory on chemistry. Nothing, Cavendish said, serves "more to rivet a theory in the minds of learners then to form all the names which they are to use upon that theory." If this precedent were to succeed, every chemist with a new theory would present it along with a new language, with the result that no one could understand what was being said without first learning the new theory. Moreover, every experimental advance in chemical composition would be followed by renaming. A systematic nomenclature did not lead to clarity, as its proposers believed, but to "confusion," causing "great mischief." Because traditional names of chemical substances had no connection with their composition, no bias was built into them. Cavendish made an exception for uncommon neutral salts, accepting thatr naming them by their components made sense because there were so many of them. He apologized to Blagden for his uncharacteristic "long sermon" on the "present rage of name-making." He thought that the nomenclature would likely pass as a "fashion," a word Cavendish used three times in his "sermon."[90] Blagden's reaction was much the same. The authors of the chemical nomenclature had been seduced by the Linnean natural history, Blagden said, which was a false analogy. The objects studied by natural history remained the same over long periods, but in chemistry discoveries came so rapidly that names would have to change constantly. Like Cavendish, he saw "little danger that the systematic names will be adopted."[91] Other proposals of chemical nomenclature and shorthand around the same time were met with skepticism by Blagden and, we suppose, by Cavendish.[92] Cavendish clearly did not think about chemical nomenclature as its authors did, as a tool like mathematics capable of directing researchers to discoveries.

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[87] Cavendish (1784b, 181).

[88] Thomson (1830–1831, 2:348).

[89] From Dover, Blagden wrote to Cavendish in London that he had the book and would hold it if Cavendish planned to join him or forward it to Banks's address where Cavendish could pick it up. Because of foul weather, Cavendish did not go to Dover, with the result that he and Blagden discussed the nomenclature by letter. Charles Blagden to Henry Cavendish, 16 Sep. 1787; Henry Cavendish to Charles Blagden, n.d. [after 16 Sep. 1787], draft, in Jungnickel and McCormmach (1999, 634–635, 638–640). Charles Blagden to Claude Louis Berthollet, 17 Nov. 1787, draft, Blagden Letters, Royal Society 7: 85.

[90] Cavendish to Blagden, [Sept. 1787].

[91] Charles Blagden to Henry Cavendish, 23 Sep. 1787, in Jungnickel and McCormmach (1999, 641–644).

[92] "Dr Black has just made a new chl nomenclature: I think he might have been better employed"; J.-H. Hassenfratz's chemical shorthand was thought to serve no "useful purpose" in England; and James Watt risked his reputation with his chemical algebra. Charles Blagden to M.-A. Pictet, 12 Feb. 1790, draft, and to James Watt, 6 Dec. 1788, draft, Blagden Letters, Royal Society 7:402 and 7:185.

In 1788 an English translation of the new nomenclature came out. Adoption of it was relatively slow, given British reluctance to use French words or their Anglicized versions and, in some cases, to part with phlogiston. In his treatise on chemistry in 1790, William Nicholson said that the phlogistic and anti-phlogistic hypotheses were equally probable; in his dictionary of chemistry in 1795 he regarded the anti-phlogistic hypothesis as the most probable, but he did not use the new nomenclature because he did not want to "anticipate the public choice." Priestley never adopted the new language or gave up phlogiston. Black soon gave it up, but he accepted the new language only selectively and invented a partially new one of his own. In the 1790s the French nomenclature was commonly used in Edinburgh and in London.[93] In a letter in 1794 Blagden wrote of Thomas Beddoes's apparatus and the "dephlogisticated dog" inside it; he crossed out "dephlogisticated" and wrote instead "oxygenated."[94]

Late in life, Cavendish used Lavoisier's names on occasion.[95] Around 1800, he returned to an experiment he had carried out much earlier, probably in or around 1783, on the distillation of charcoal producing fixed and inflammable airs. Upon carrying out new computations, he concluded that the experiment showed that either the "charcoal contains hydrogen as well as carbon & water or else that the charcoal after distillation containd some oxygen." In this passage he used terms he had not used at the time he made the experiment, "carbon," "hydrogen," and "oxygen." He also spoke of "phl. Air" not "azote" in the same place. His chemical vocabulary was a mix in this case.

Chemists

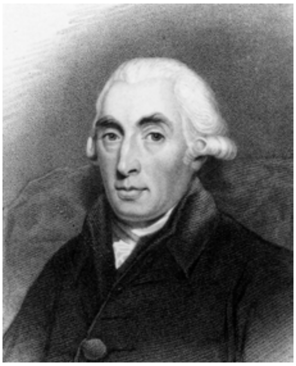

Figure 14.5: Joseph Black. Painted by Henry Raeburn, engraved by T.A. Dean. Courtesy of the Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

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[93] Nicholson (1790, viii; 1795, 1:vii). Maurice Crosland (1962, 193–206).

[94] Charles Blagden to Georgiana, duchess of Devonshire, 4 Jan. 1794, Devon. Coll.

[95] In computations around 1800, Cavendish used "hydrogen" and "oxygen": Henry Cavendish, "Experiments on Air," Mss II, 5: 390. In a letter to Blagden about a paper by Humboldt on the eudiometer, Cavendish used Lavoisier's name for phlogisticated air (our nitrogen) "azote." This was in 1798, some ten years after his "sermon" on Lavoisier's new chemical nomenclature. Henry Cavendish to Charles Blagden, 18 Dec. [1798], Blagden Papers, Royal Society.

Figure 14.6: Antoine Laurent Lavoisier. Drawing by J. Boilly, engraving by Nargeat. Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

Figure 14.7: Joseph Priestley. Leeds portrait of Priestley around 1765. Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

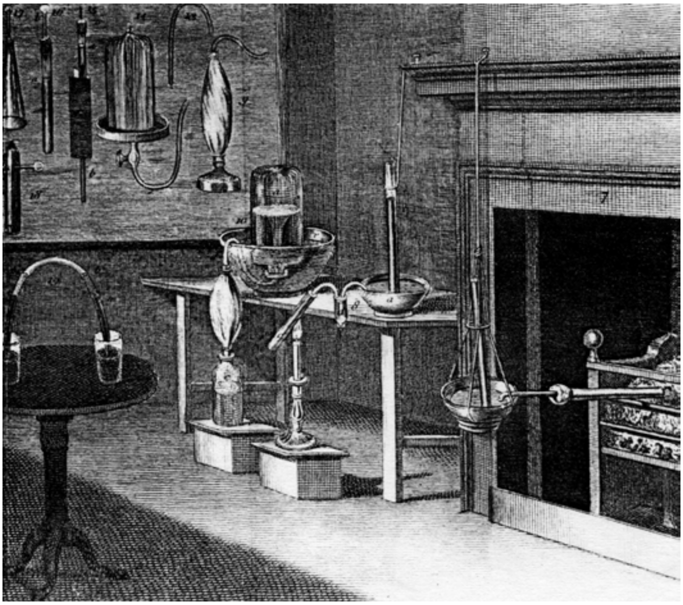

Figure 14.8: Priestley's Chemical Apparatus. From the first volume of Priestley's *Experiments and Observations on Air*, 1774.

Figure 14.9: Carl Wilhelm Scheele. Engraving. Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

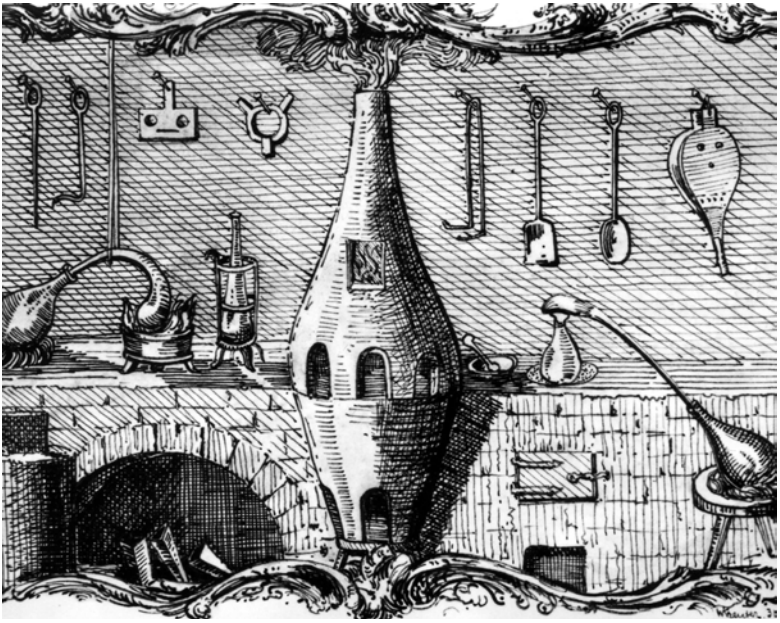

Figure 14.10: Scheele's Laboratory. Woodcut by W. Kreuter. Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

Earlier in this book where we take up Cavendish's chemical work in the 1760s, we discuss phlogiston. Having considered Cavendish's explanation of his experiments in the 1780s, we return to the discussion here. The Stahlian principle provided a theory that covered a wide range of chemical behaviors, not only combustion but also acidity, alkalinity, chemical reactivity, and chemical composition. As with most any general theory, it met with difficulties. One difficulty was accounting for the gain in weight of combustibles when burned and of metals when calcined, since according to the theory they have lost something, phlogiston, and so there ought to be a loss of weight rather than a gain. Not all chemists accepted that metals gained weight, and chemists did not consider it a serious problem until they studied gases from the 1760s, when they began to attribute odd properties to phlogiston to explain what they observed. In 1785, the year after Cavendish's paper on experiments on air, Lavoisier published an essay on the principle of phlogiston. Like Cavendish, he recognized that chemical phenomena could be explained with or without phlogiston, but he drew a different conclusion, casting doubt on the existence of phlogiston, which he saw as a vague principle, always changing to meet the explanations demanded of it. He gave examples: sometimes phlogiston has weight, sometimes not; sometimes it is free fire, sometimes it is fire combined with an earth; sometimes it passes through the pores of vessels, sometimes it finds them impenetrable. It explains at once causticity and non-causticity, transparency and opacity, colour and the absence of colour, a "veritable Proteus" that changes its form every instant.[96]

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[96] Brock (1992, 83–84, 111–112).

Like other phlogistic chemists, Cavendish observed the gain of weight of the product, the caput mortum, of burned and calcined substances without seeing it as an important problem. In his first surviving chemical research, he explained the weight of the caput mortum of arsenic by its retention of some of the aqua fortis used in making it, even performing an experiment to support his conclusion.[97] Not drawn to speculations about phlogiston like those mentioned by Lavoisier, Cavendish regarded phlogiston as a respectable chemical substance, which happened to be wrong.

Wilson, Cavendish's biographer, was educated in the chemistry that Lavoisier's revolution set in motion. Much closer to the era of phlogiston chemistry than we are, his opinions were correspondingly stronger. He considered Stahl's theory to be nearly empty of content, scarcely deserving "to be called a scientific hypothesis," amounting to no more than "the assertion, that a body was combustible because it contained something combustible." He likened it to "vulgar belief," "poetical" thought, and a child's idea of power as inhabiting objects "resembling a living or vital agent." Instead of recognizing it for what it was, a chimera, chemists used it as "a perfect theory." Phenomena that conflicted with it, such as the increase in weight of combustibles when burned, they overlooked or downplayed. This was the "crude and clumsy hypothesis" that passed for a fundamental principle of chemistry at the time Cavendish began his experiments.[98]

With hindsight, what Lavoisier and Wilson said about phlogiston is reasonable, for what came after was ever so much better. Yet something can be said in defense of phlogiston, which Cavendish's later biographer Berry recognized: "Cavendish was able to use the phlogistic hypothesis successfully for his own purposes, and that for him was sufficient. The progress of science has shown that its pathways are littered with discarded theories, which, nevertheless, rendered good service in bygone times."[99] Phlogiston provided chemistry with a theory, and without a theory Cavendish would not have done any work in chemistry. With the theory available to him he was able to make major contributions to the science. If there had been a better theory when he started out, he would have used it, and the results he obtained would have reflected it. We can say that his experiments on air were a late success of phlogiston chemistry, while acknowledging that the significance of his late experiments became evident with Lavoisier's anti-phlogistic chemistry.

Lavoisier and his colleagues brought together British pneumatic chemistry with Continental analytical chemistry in a "new synthesis of chemical theory." Cavendish helped prepare the groundwork for the new theory of chemistry, but he had no part in formulating it. After his experiments on air in the early 1780s, he published no more new experiments in chemistry, and he said nothing more in print about the changes that chemistry was undergoing. It has been said that chemistry after Lavoisier emerged as "a self-contained discipline, with its own theoretical problems, its own methods of thought and inner logic."[100] If that description fits, it remains that Lavoisier's synthesis did not resolve all the important questions of chemistry or deny that it raised new ones.[101] Cavendish disagreed with at least two

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[97] Cavendish, "Arsenic," Cavendish Mss II, 1(b), 14.

[98] Wilson (1851, 36–38).

[99] Berry (1960, 183).

[100] Henry Guerlac (1959, 109, 112).

[101] Following the publication of Lavoisier's *Treatise*, writings of chemists revealed "widespread confusion and uncertainty" over the nature of elementary substances and the naming of compounds, as if they "knew they couldn't go back to the old way of thinking, but were quite unsure of which way was forward." Robert Siegfried and Betty Jo Dobbs (1968, 275–276).

parts of it, the theory of acids and the caloric theory of heat, and he would be proved right about both.

The Jacksonian professor at Cambridge, Isaac Milner, saw the handwriting on the wall; in his final lecture in 1788, he discussed Lavoisier's experiments, commenting that the "ancient hypothesis of Phlogiston seems overturned at one Stroke, and a new and simple theory substituted in its place—a Theory founded on direct and satisfactory Experiments."[102] In 1792, the Scottish physician and chemist George Fordyce wrote in a paper on the gain in weight of metals when they are calcined that "many chemists are at present satisfied of the nonentity of what was formerly supposed to be a body, called phlogiston." He justified going over the subject again on the grounds that phlogiston "has interwoven itself so much into chemistry in general, and has been so universally received."[103] Fifteen years later, in his Bakerian Lecture in the Royal Society, Davy said that the "discovery of the agencies of the gases destroyed the hypothesis of Stahl."[104] The rejection of phlogiston was not the whole of the Chemical Revolution, but it played a significant part. Cavendish, it seems, went that far but possibly no farther, showing no inclination to join the vanguard of the new chemistry. He was fifty-four years old in 1785, possibly a relevant fact.

Water Controversy

In a paper in early 1784, as we have seen, Cavendish identified the product of the explosion of two airs with water. He, Watt, and Lavoisier, the principals in the "water controversy," had different explanations. If it had been about these differences, it would have been a controversy of a familiar kind in science, but this one was also about character.

Basically a priority dispute, the water controversy arose from the following events, which are partly familiar to us. In the spring of 1781, Priestley and Warltire made experiments on the electrical detonation of inflammable air with common and dephlogisticated airs, noting a deposit of dew. That summer Cavendish repeated the experiments, determining that the dew was pure water.[105] He mentioned the experiments to Priestley, who repeated them and drew up a paper for the Royal Society. Learning of Priestley's experiments, Watt wrote to Priestley with his explanation of them: when inflammable and dephlogisticated airs are detonated, the two airs unite and then disappear, and the weight of the water that takes their place is equal to the weight of the airs. He concluded that "water is composed of dephlogisticated air and phlogiston, deprived of part of their latent or elementary heat."[106] In the summer of 1783, Blagden made a trip to Paris, where he told Lavoisier about Cavendish's experiments and Watt's explanation. Lavoisier and Laplace repeated Cavendish's experiments, and Lavoisier made further experiments dissociating water into its component airs. In a report on his experiments, he concluded that "water is not a simple substance but is composed, weight for weight, of inflammable and vital air."[107] Deluc went to Paris at the end of 1783, returning to England early the next year. In the meantime, Cavendish's paper

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[102] L.J.M. Coleby (1954, 256).

[103] George Fordyce (1792, 374).

[104] Humphry Davy (1808, 33).

[105] Wilson (1851, 282).

[106] Ibid., 285, 290–293.

[107] Ibid., 337, 344–345.

was read to the Royal Society. Blagden gave an imperfect account of the paper to Deluc, who asked to see it, and Cavendish gave him permission.

The water controversy begins here. After reading Cavendish's paper, Deluc wrote to Watt that Cavendish "expounds and proves your system, word for word, and makes no mention whatever of you." He wrote a second letter a few days later cautioning that "it is yet possible Mr. Cavendish does not think he is pillaging you, however probable it is that he does so." Deluc told Watt that Cavendish must have read the letter he wrote to Priestley, which circulated among fellows of the Royal Society, before drawing up his own paper. Cavendish was a plagiarist.[108] Watt accepted Deluc's suspicions about Cavendish. By not revealing all of what Blagden had told him about Cavendish's and Watt's work, Lavoisier also laid himself open to the charge of plagiarism. Distressed by Lavoisier's representation of Cavendish's work, Blagden took a variety of measures, public and private, to set matters right. Lavoisier readily acknowledged that Blagden had told him about Cavendish's experiments before he carried out his own.[109] He stood corrected; he did not covet a discovery so much as all of chemistry, and the experiments on water had told him how to get it.

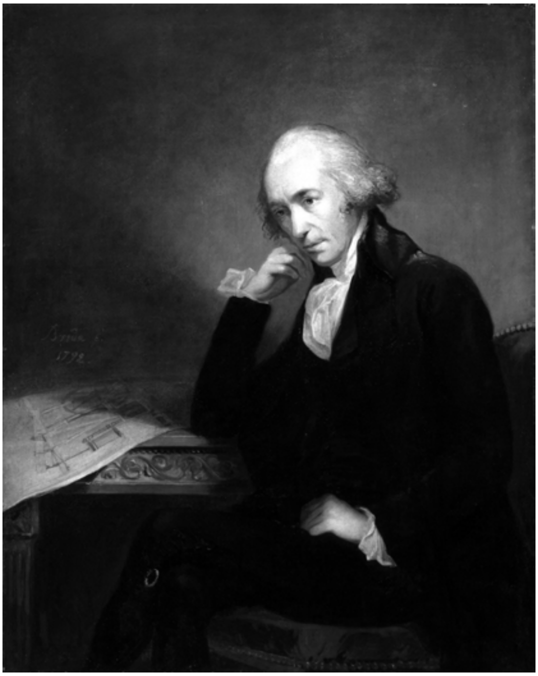

Figure 14.11: James Watt. Painting by Carl Frederik von Breda. Engineer and inventor, best known for his improvement of the Newcomen steam engine. Wikimedia Commons.

The passion behind the water controversy was decidedly Watt's. He told Deluc that he did not depend on the favor of "Mr. C: or his friends; and could despise the united power

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[108] Jean André Deluc to James Watt, 1 Mar. 1784, Watt (1846, 48–49). Wilson (1851, 407–408).

[109] Wilson (1851, 362).

of the *illustrious house of Cavendish*, as Mr. Fox calls them."[110] Cavendish was a rich man with a mean spirit, Watt wrote to another correspondent.[111] When Watt saw Cavendish's paper he recognized that it was different than his, and next year, 1785, Watt and Cavendish met in Birmingham, where they discussed steam engines, a subject on which Watt was the authority. That year Cavendish recommended Watt for fellowship in the Royal Society, his name appearing third after Smeaton's and Priestley's in the long list of Watt's supporters.[112] The same year, Watt came to London, where he "was received very kindly by Mr. Cavendish and Dr. Blagden."[113] (Fig. 14.11). Clearly there were no lasting hard feelings.

Much of the controversy revolved around datings of experiments, publications, and meetings. The datings were genuinely tangled, as this brief review will indicate. Soon after Warltire's experiments on the ponderability of heat were published in 1781, Cavendish began his experiments on the production of water from the explosion of airs. Before 26 March 1783 his experiments were communicated to Priestley; before 26 April 1783 Priestley's repetition of Cavendish's experiments was communicated to Watt, who promptly sent Priestley an explanation of them; and before 24 June 1783 Cavendish's experiments were communicated to Lavoisier by Blagden. Cavendish's own account of his experiments was only read to the Royal Society on 15 January 1784. A further complication came from the Royal Society's practice of permitting authors to make changes in their papers between the time they were read and their publication. Cavendish's paper contained three insertions, made at different times, in one of which he said that his experiments on the explosion of inflammable air with ordinary or dephlogisticated air were made in the summer of 1781. The year 1781 was an important date because the Royal Society did not learn of the experiments until 1784. Watt and Lavoisier did their researches later than Cavendish, but since they made their views known earlier, they appeared, Wilson said, "with a *primâ facie* character of priority to him [Cavendish], as claimants of the disputed discovery."[114]

The controversy was started by Deluc. Wilson thought that Deluc was an honorable man "whose motives are beyond suspicion," but who was guilty of the "grave charge" of accusing Cavendish of stealing Watt's theory without informing himself thoroughly. The charge was baseless, as Deluc could readily have determined.[115] Deluc and Cavendish had a long association. When for financial reasons, Deluc left his native Switzerland to settle in England, Cavendish brought him as his guest to a meeting of the Royal Society a month before his election.[116] He and Deluc served together in the Society, performed experiments together, corresponded, and disagreed civilly. Drawing on a common fund of knowledge about human behavior, we can imagine that the reason for Deluc's intervention was more complex than carelessness alone, but lacking evidence as to its nature, we accept Wilson's appraisal.

Like Deluc, Blagden had a role in the water controversy not as a claimant to the discovery but as an intermediary between persons who were. As Deluc's complicity was built into his relationship with Watt, Blagden's was with Cavendish. Blagden's association with Cavendish was his scientific passport, while at the same time his zealous regard for the rep-

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[110] James Watt to Jean André Deluc, 6 Mar. 1784, Watt (1846, 48–49)

[111] James Watt to Mr. Frey of Bristol, 15 May 1784, ibid., 61.

[112] 24 Nov. 1785, Certificates, Royal Society 5.

[113] Watt, quoted in Samuel Smiles (1874, 169).

[114] Wilson (1851, 60–61).

[115] Ibid., 408.

[116] 13 May 1773, JB, Royal Society 28:132.

utation of Cavendish was a vulnerability, which was compounded by his duty as secretary of the Royal Society of editing papers for publication in the *Philosophical Transactions*. Latter-day champions of Watt made out Blagden to be a villain, but he was guilty not of the unfairness and venality with which he was charged but only of neglecting his own best interest. Nor was Cavendish guilty of exploiting Blagden's dependent position to get him to commit fraud on his behalf.

With the remote exception of Deluc, there was no malice on the part of anyone. When the steps leading to the dispute are examined one by one, as Wilson and others have done, this conclusion seems inescapable: a major reason for the "controversy," as distinguished from a common scientific disagreement, was the casual way scientific information was communicated in the eighteenth century. The discovery of the nature of water was timely, and the stakes were high, so that otherwise tolerable exchanges by letters, conversations, and visits, with their indifferent datings, could, with proper incitement, seem darkly suspicious. As it turned out, precisely because there was also disagreement of the usual kind, different interpretations of the same experiments, there was recognition to go around. Cavendish was the first consciously to produce water by detonating airs; Lavoisier was the first to analyze water into its component airs; and Watt and Lavoisier were first to state unequivocally the compound nature of water.

A second water controversy arose long after the participants in the first were dead, prompted by the secretary of the French Academy D.F.J. Arago, who in his *éloge* of Watt asserted that Priestley was the first person to prove that airs could be converted into water and that Watt was the first person to understand it.[117] The consequent furor was initiated by Vernon Harcourt in his presidential address at the British Association for the Advancement of Science meeting in 1839. Because the revived controversy was the occasion for Cavendish's unpublished scientific work to begin to be made public and for a biography of Cavendish to be written while persons who knew him were still alive, it had that value if perhaps no other.

In his biography of Cavendish in 1851, after meticulously examining all the documents relevant to the water controversy Wilson reached the conviction he began with, that Cavendish was the discoverer of the compound nature of water and that his character was blameless. He returned to the subject in 1859, after two new documents had come to light, strengthening his argument. One of them was a publication on meteorology by Deluc, who related a conversation with Priestley in 1782, which Wilson said removed "all trace of charge against the fair-dealing of Cavendish." The other document was by Laplace, who said that Cavendish was the first to point out that water is produced by the combination of hydrogen and oxygen and that the weight of the water is equal in weight of the gases. Wilson published this further vindication of Cavendish in the *Proceedings of the Royal Society of Edinburgh*.[118]

Keeping up with Chemistry

As a result of the water controversy, Cavendish and the German journal *Chemische Annalen* had started off on the wrong foot. The editor Lorenz Crell published two accounts of the discovery of the production of water in which Lavoisier was named the discoverer and Cavendish the confirmer. For more information about Cavendish's work, Crell wrote

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[117] As Harcourt summarized Arago's claim, in his Presidential Address, British Association Report (1839), 15.
[118] Berry (1960, 87–88).

to Banks, who passed the letter to Blagden. The latter replied to Crell with a "short history of the discovery," correcting the claims of Lavoisier, who had "suppressed part of the truth." Blagden complimented Crell on the quick publication of translated extracts from Cavendish's paper containing the true discovery and for Crell's correct dating of the paper, 1784, instead of 1783, as the separately printed cover of the paper had erroneously put it. In a note printed with the extracts, Crell graciously acknowledged that he was under an obligation to Cavendish because he, like others, had made an "error" in ascribing the discovery to Lavoisier, whereas the "*first Discovery*" belonged to Cavendish. This initial letter from Blagden to Crell included the latest scientific news from Britain, meant to entice Crell to join in a regular scientific exchange between the two countries.[119]

Crell wanted to publish Blagden's short history of Cavendish's discovery, and although Blagden had not intended it for the public, he had no objection, since it was "strictly true." He only hoped that Crell's German translation would rather "soften than strengthen the expressions," since however poorly Lavoisier had behaved in this affair, he was "upon the whole a very respectable character & eminent as a philosopher." In keeping with his invitation to Crell, Blagden enclosed scientific news having to do with "Mr. Cavendish, whose name I shall so often have occasion to mention in this correspondence." This time it was about Cavendish's new work on the freezing of mercury rather than the history of his old work.[120]

The German chemist knew that Cavendish was an aristocrat but little about English titles. "The Honourable Henry Cavendish (not My Lord)," Blagden corrected him, "desires to become one of your subscribers." To this end, Cavendish had given directions to the post office to ensure that he received the journal promptly.[121] Six months later, Blagden wrote to Crell that the postmaster at Amsterdam had told him that some of Crell's packets were held up because of their large size and were probably irrecoverable, a problem which could have been anticipated, since Banks had gone through it with Crell the year before.[122] Crell had sent the material not by post but by stagecoach or wagon, Blagden said, conveyances which were not "connected with but in opposition to the Post." When Cavendish succeeded in receiving a few issues of the *Chemische Annalen* and its supplement, the *Beiträge*, by post, Blagden instructed Crell to send Cavendish the rest by post as well. However, when after three months the remaining issues had not yet arrived in London, Blagden complained to the post office and then to Crell: "Mr.Cavendish pays many times the original value of the work to have it in this manner quick by the post; but the various delays have entirely frustrated that object."[123] The post office proved not to be a better way. Two years later the business of delivery was at last settled and the correspondence on that subject ended: "Mr.Cavendish

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[119] Among Cavendish's manuscripts is a translation into English, not in Cavendish's hand, of Crell's translation into German of extracts from Cavendish's paper of 1784, with Crell's retraction of his earlier error. "Translation from Mr. L. Crell's Chemical Annals, 1785. part 4, 324." Charles Blagden to Lorenz Crell, 28 Apr. 1785, draft, Blagden Letterbook, Yale. Blagden's letter, in English, clarifying the discovery to Crell was translated into German by Crell and translated back into English by Wilson (1851, 362–363). Wilson's translation is reproduced in Berry (1960, 81–82).

[120] Charles Blagden to Lorenz Crell, 2 Dec. 1785, draft, Blagden Letters, Royal Society 7:738.

[121] Charles Blagden to Lorenz Crell, 20 Jan. 1786, draft, ibid. 7:742.

[122] Lorenz Crell to Joseph Banks, [1785], 17 Dec. 1785, 1 May 1786, 4 Mar. 1790, BL Add Mss 8096:69–70, 239–240, 284–285, and 8097:296–297.

[123] Charles Blagden to Lorenz Crell, 4 July, 12 Aug., and 13 Oct. 1786, drafts; Charles Blagden to Charles Jackson at the post office, 10 Oct. 1786, draft, Blagden Letters, Royal Society 7:26, 44–45. By 4 July Cavendish had received the first and second issues of the *Annalen* and the fourth issue of volume 1 of the *Beiträge*. On 6 August, he was

finds it more convenient to get the Ch Annalen," Blagden wrote to Crell, "in the common way, tho' a little later, then to be perplexed with the post office; he [...] will not give you any further trouble on the subject."[124]

There were other complications, for example, the manner of payment for the subscription, of how much and to whom; Blagden told Crell to appoint some person to collect Cavendish's money. Kirwan and Banks wanted to subscribe, and the journal could not be sent to everyone "through the same channel under one cover." In addition to the journal, there were other publications by Crell that Cavendish wanted. He had ordered Crell's *Auswahl aus den neuen Entdeckungen*, but his German bookseller had disappointed him. Crell offered to copy out the material Cavendish wanted, but Cavendish wanted the entire volumes.[125]

To convey scientific publications from Britain to Germany was not simpler. Blagden sent a copy of Cavendish's latest paper to Crell in a packet, which he gave to William Herschel, who was going to Gőttingen to erect one of his telescopes. From Gőttingen, Herschel forwarded the packet by the nearest conveyance to Helmstadt, where Crell picked it up. Blagden apologized to Crell: "It is extremely difficult to get an opportunity of sending you any thing from England, otherwise you should be furnished sooner with such publication."[126] The business of Cavendish and Crell was not unrelieved frustration. Cavendish thanked Crell for offering "the Old Hock," and Blagden assured Crell that "we shall endeavour to form such a party of gentleman as would be required."[127]

Cavendish took evident interest in Crell's *Chemische Annalen*, a monthly journal that in 1784 replaced the quarterly one he had been editing. It had the support of German chemists and favored, as he still did, the phlogistic approach to chemistry. As Cavendish's negotiations with Crell bear out, it was no simple matter to obtain foreign journals in England in the eighteenth century. In this episode we see the trouble Cavendish went to to keep abreast of chemical research and to keep his library current. We also see the value to him of having an associate, Blagden.

Theory

In his work in the basic fields of electricity, heat, and chemistry, Cavendish developed general theories for two of them, electricity and heat. His publications and manuscripts do not contain a comparable theory of chemistry. This could mean that he was content with the given theory or that chemistry was a different kind of field and in a different state of development. In this section, we return to several subjects, phlogiston, forces, and affinities, and to several chemists we met earlier, whose connections to chemical theory come up in the following brief account.

Chemists mostly agreed that the object of chemistry was to separate compounds into their parts, to study the parts, and to reunite them to form the original compounds or to produce new compounds. That describes what chemists did in the laboratory, but it fails to mention the help they had from outside, which included theory. In his *Dictionary of*

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still waiting for the third through the sixth issues of the *Annalen* and the first through the third issues of volume 1 of the *Beiträge*.

[124] Charles Blagden to Lorenz Crell, 4 Apr. 1788, draft, Blagden Letters, Royal Society 7:137.

[125] Blagden to Crell, 4 July and 12 Aug. 1786.

[126] Blagden to Crell 4 July 1786.

[127] Charles Blagden to Lorenz Crell, [1786], draft, Blagden Letterbook, Yale.

Chemistry, Macquer said that theory and experiments necessarily went hand-in-hand: "If experiments, undirected by theory, are only a blind feeling; theory without experiments is a deceitful and uncertain vision," since advances in chemistry owed to the "joint assistance of both."[128]

It was common for textbooks on chemistry to present the "theory" of chemistry followed by the "practice" of chemistry, as is seen by their titles: Stahl's *Fundamenta Chymiae dogmaticae et experimentalis*, Boerhaave's *New Method of Chemistry: Theory and Practice of That Art* and Macquer's *Elements of the Theory and Practice of Chemistry* are examples. The "practice" part of Macquer's text, which was a separate volume in the original French, gives operations performed on mineral, vegetable, and animal substances, a common classification of the time. The "theory" part is organized by types of substances—for example, saline substances, metallic substances, and acids—followed by types of fermentation and chemical decomposition. Macquer based his discussion of these substances on "affinity" and on four primary "principles"—the ancient elements of air, water, earth, and fire—and "secondary principles," which are combinations of them.[129] Because Cavendish did not write a treatise on chemistry, he had no occasion to organize the science as a whole, and so we do not know what he thought about elementary substances or what his general "theory" of chemistry in this sense would look like.

We know a good deal, however, about Cavendish's thoughts on "specific theories"[130] of chemistry, which showed how chemical phenomena of certain kinds were related to one another. Examples of specific theories are the "theory of neutral salts," which Lavoisier considered the "most certain and complete part of chemistry"; Lavoisier's "theory" of acids, which was that acids are composed mainly of pure air, or oxygen; and Black's "theory" of fixed air in earths that are reducible to quicklime by calcination. For Cavendish, the most important specific theory was the theory of phlogiston, which originated in a general theory of chemistry.

We briefly recall the original theory. According to the chemist and historian of chemistry Thomas Thomson, the first chemist to establish a general "theory" of chemistry "by which all the known facts were connected together and deduced from one general principle," was Becher, whose theory contained an interesting fatty earth responsible for combustible properties of bodies. Stahl adopted and modified Becher's theory in 1718, renaming his fatty earth "phlogiston"; his major improvement in the theory was to show how phlogiston explains both combustion and calcination. So convincing was Stahl's theory, according to Thomson, that it was "adopted by every chemist without exception." Of Stahl's theory, Macquer said that it was "of all theories the most enlightening, and the most conformable with the phenomena of chemistry, "making it the "surest guide that we can take for our conduct in chemical researches," as proven by experiments made daily. Having Stahl's theory of phlogiston to direct their work, and knowledgeable in the art of experiment, Macquer said, chemists had the "advantage of seeing the best days of chemistry."[131] Because nearly all chemical processes were explained by phlogiston or its absence, Stahl's theory of chemistry came to be regarded as a theory of phlogiston and combustion. Over the next six decades

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[128] Macquer (1771, 1:xi–xii).

[129] Macquer (1758, 1:15–18).

[130] Term used by Mi Gyung Kim (2003, 5).

[131] Macquer (1771, 1:xi–xii).

chemists worked with, extended, and altered his theory. They raised questions about phlogiston, but generally they thought that its existence was proven by experiments.

In Berlin Stahl created a school of phlogistic chemists, who included Caspar Neumann, professor of chemistry at the Royal College of Physic and Surgery.[132] Well known to the Royal Society as a foreign member and contributor to the *Philosophical Transactions*, Neumann's lectures were published after his death in an abridged edition in English translation in 1759. Without elaboration, Neumann introduced "phlogiston," the "inflammable principle," which was the same in metals and throughout the mineral, vegetable, and animal kingdoms.[133] In this edition of his lectures, his treatment of chemistry was practical, giving straightforward descriptions of operations and reactions, and having little to say about the theory of chemistry. His lectures were considered an excellent introduction to chemistry, which were still of value at the time of Thomson's history of chemistry in 1830.

As mentioned earlier, unless they read foreign works, English chemists learned about phlogiston through Neumann's lectures and a successful textbook on chemistry by Macquer, *Elements of the Theory and Practice of Chemistry.* Macquer's version of phlogiston differed considerably from Stahl's. One of the traditional four elements or principles—fire, air, water, and earth—Macquer's fire existed in two states, one fluid and one fixed, a combination of elementary fire with another substance constituting a "secondary principle," or "phlogiston."[134] Macquer's text appeared in English translation in 1758, one year before Neumann's, and six years before the earliest dated chemical researches of Cavendish's, which originated with Macquer.

Cavendish's chemical researches made extensive use of phlogiston; more than that, they were *directed* to phlogiston and its activity, the core of the current theory of chemistry descended from Becher and Stahl. The most important result of his publication on factitious air was his determination of the properties of inflammable air, which he identified with phlogiston. When he returned to experiments on air, it was to determine what happens to air in all the ways it can be phlogisticated. Phlogiston was the cornerstone of his researches in chemistry, both in their planning and in their interpretation. To hold a specific theory of phlogiston, Cavendish did not have to accept other parts of Stahl's theory.

There is the beginning of a specific theory in Cavendish's paper "On the Solution of Metals in Acids," which he intended for his paper on arsenic and then rewrote for his first published paper, on factitious air. Bringing together two of the main chemical concepts, affinities (discussed below) and phlogiston, he described and explained the action of acids on perfect and imperfect metals. It was accepted by chemists that when imperfect metals dissolve in acids they lose their phlogiston, but it had "usually been thought" that acids do not deprive perfect metals (gold and silver) and mercury of their phlogiston. Cavendish disagreed, giving an argument from the relative strengths of affinities to show why: these metals actually have so great an affinity to phlogiston that they reacquire it from the substance that is added to separate them from the acid ("there seems no reason to think that the pure fixed alkali, or even lime, is quite free from phlogiston"). By a more intricate argument, he explained why gold requires two acids, nitrous (nitric) and marine (hydrochloric), to dissolve it. He called his explanation of the solution of perfect metals and mercury in

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[132] Thomson (1830–1831, 2:250–263).

[133] Casper Neumann (1759, 53, 165).

[134] Macquer (1758, 1:9–10).

acids a "hypothesis," which had not yet been proven "true."[135] When he made a theory, he usually began with a hypothesis and then confirmed it; in the case of metals and acids he lacked the confirmation that would turn his hypothesis into a theory.

We turn to another line of chemical theory, introduced earlier in this book. Newton was a main authority for the physical approach in chemistry. He published only one paper on chemistry, but in his *Opticks* he made a good many statements about it, with consequences for chemistry into Cavendish's time. The last Query in the late editions of the book begins: "Have not the small Particles of Bodies certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the Rays of Light … but also upon one another for producing a great Part of the Phaenomena of Nature?"[136] Newton speculated that such forces, which extend only over extremely short distances between particles, are capable of explaining chemical reactions and many physical processes. His first example was salt of tartar running per deliquium, which he explained by the attraction of the particles of the salt to the particles of water. By similar reasoning, he explained why metals replace one another in acid solutions, naming the order of replacement of six metals in nitric acid. The examples of the last Query are concerned with the heat of chemical activity as much as with the chemical attraction. When aqua fortis (nitric acid) is poured on iron filings, "great Heat and Ebullition" are produced by a "violent Motion of the Parts," caused by the attraction between particles. The violence of the internal commotions of chemical bodies contrasts with the stately motions of the planets, yet both are compatible with the "Tenor and Course of Nature." Attractions move the planets and they move the particles of bodies in chemical interaction, for nature "is very consonant and conformable to her self."[137]

Newton and his followers considered forces that obey different laws than the inverse square. In 1708 John Keill, one of the first to teach the Newtonian philosophy, published a theory of forces, which included a force that falls off faster with distance than gravitation, confining its action to minute distances. Reasoning from principles that belong to the "foundation of all physics," Keill laid down theorems about attraction and about particles of different figures, demonstrating how chemical and physical actions can be seen to arise from the intimate force.[138] The next year the professor of chemistry at Oxford John Freind said that chemistry had made progress in experiment but not in theory until Keill showed the true principles of chemistry, which were mechanical, "taken from the very Nature of Things." Freind applied Newton's laws of nature, as modified by Keill, to the main operations of chemistry. Like Keill, he did not state a mathematical law for the new force, postulating only that it falls off faster than the force of gravity, and in references to it he relied on common sense to make his points.[139] Keill's and Freind's work can be called "theoretical chemistry,"[140] which relates chemical practice to concepts and laws that order the universe. Their theorems were remote from the activity of the laboratory, but they were not forgotten.

The most important early developer of Newtonian chemistry was Stephen Hales, whose experiments on air were published in his *Vegetable Staticks* in 1727, discussed earlier. Hales

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[135] Henry Cavendish (1921e, 2:305–307).

[136] Isaac Newton (1952, 375–376).

[137] Ibid., 376–77, 380–81.

[138] John Keill (1708).

[139] John Freind (1712). Translated from *Praelectiones Chymicae* (London, 1709).

[140] Kim's term, which applies here (2003, 4).

said that his experiments on air proved that there are particles "capable of being thrown off from a dense body by heat or fermentation into a vigorously elastick and permanently repelling state," and that by actions of the same kind the particles can be reunited in a dense body. Freind had given an "ingenious Rationale" for calcination, distillation, and other chemical processes, Hales said, and he applied the same principles to the study of air, which he regarded as responsible for "the main and principal operations of Nature."[141] Cavendish was well acquainted with Hales's *Vegetable Staticks*.

In 1727, the same year as Hales's book, there appeared an English translation of Herman Boerhaave's *New Method of Chemistry*, which influenced British chemists, especially the Scottish. Boerhaave laid down as "laws, or axioms of the art," the general truths that chemists had arrived at by experiment, which constituted the "theory" of chemistry. He contrasted his presentation of chemistry with those by other authors, which were "without any certain design or coherence." Another representative of the physical approach in chemistry, Boerhaave wrote that "every *change* which chemistry produces in bodies, is the effect of *motion*," and chemists "who deny the changes produced by chemical operations to be *mechanical*" are misled. Instead of "force" and "attraction," he spoke of "uniting" and "separating," words which described what chemists actually observed. In their annotations, his translators Peter Shaw and Ephraim Chambers discussed the forces producing the motions, following Newton who had established that "there are such motions in the *minima naturae*, and that they flow from certain powers, or forces not reducible to any of those in the great world." They referred to Keill and Freind who explained all chemical phenomena by a novel attraction, "but this seems a little too precipitate," for the laws of the intimate motions of the world were not yet known,[142] a hard fact that successive chemists would rediscover.

The law of universal gravitation together with the definition of quantity of matter—all matter has weight, which is proportional to its quantity and remains constant[143]—entered the thought and practice of chemistry. The instrument that measured the quantity of matter by its weight, the chemical balance, received a theoretical foundation in the new natural philosophy, and the corpuscular philosophy of chemistry was adapted to the new understanding of forces. By analogy, the law of gravitation, which was deduced from motions of the large bodies of the universe, was applied to the smallest parts of the bodies, or corpuscles, an implication of its universality. This comparison appealed to the scientific intuition, an affirmation of the unity and simplicity of nature, and it supported a physical approach to chemistry. A principal advocate of this design for chemistry was the French naturalist Georges-Louis Leclerc, Comte de Buffon. His answer to chemists who like Keill and Friend thought that chemical forces obey different laws was that the action of gravitation is modified by the figure of the particles of chemical substances. Newton strangely had overlooked the full implications of gravitation in the "plan of the world" by failing to recognize the effect of the figure of particles in his speculations on chemical forces in his *Opticks*. Eminent chemists such as Guyton de Morveau, Baumé, and Bergman agreed with Buffon

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[141] Stephen Hales (1727). Thomson (1830–1831, 2:303).

[142] Herman Boerhaave (1727, 1:170–174). This book was based on student lecture notes, 1724. In 1732 Boerhaave published a treatise, *Elements of Chemistry*, which was translated by Peter Shaw in 1741. J.R. Partington says that in this book Boerhaave maintains that acid dissolves substances by motion, and that although he quotes Newton and uses mechanical analogies, the motion has a cause that is not mechanical. "Chemistry through the Eighteenth Century," in *Natural Philosophy*, supplement to *Philosophical Magazine*, 1948, 47–66, on 48. The popularity of *Elements of Chemistry* warranted another English edition, 1753.

[143] Newton, *Principia* 1:1.

about the central role of gravity in chemistry.[144] Macquer in the article "Gravity" in his *Dictionary of Chemistry* wrote that the law of gravitation, which has been found to differ from the inverse square law for very small distances, is at the same time the force of falling bodies and the force between particles, producing chemical "combinations and decompositions," and that for this reason gravitation is "undoubtedly the most important and decisive object for the general theory of chemistry." Macquer thought that this speculation is the "true key of the most hidden phenomena of chemistry, and consequently of all natural philosophy." Chemical attractions are not to be thought of as real, for there is only one true law giving rise to a great variety of effects, the law of universal gravitation, which chemists should keep in mind, though chemical theory and practice could not be built on it at the time.[145] One day someone sufficiently versed in both mathematics and chemistry may "lay the foundation for a new physico-mathematical science" of chemistry, though it is also possible that chemistry is too complicated, "beyond the reach of human understanding."[146]

Cavendish was a chemist such as Macquer described, one of the very few who were skilled in both mathematics and chemistry. Although he did not transform chemistry into a "physico-mathematical science," he accepted that chemical behavior arises from the forces of particles, as understood by mechanics. His ideas on the forces of particles were first put forward by Newton and later by the Croatian Jesuit Roger Joseph Boscovich in a treatise published in 1758, and in English translation in 1763. In this work, Boscovich developed a theory of natural philosophy based upon a law of force more complicated than the inverse-square law at close distances, which could explain in principle all physical and chemical phenomena. Any explanation had to be qualitative though, since there was no mathematical law describing attractions and repulsions at close distances to particles. Chemists who thought in terms of forces knew that they could not deduce chemical reactions from them in the present state of the science. To get on with their work, they did not need to concern themselves with laws of forces or configurations of particles, and it seems that the most productive chemists did not.[147] Cavendish's papers contain no attempt to calculate chemical processes from forces.

Newton's discussions of chemical actions and heat as a combined subject were continued by Cullen and Black and further developed by Cavendish, who believed that all bodies that have an affinity for one another generate heat when they are combined. The connection he saw between chemistry and heat is seen in their juxtaposition in a number of his writings. The manuscript of his researches on air contains experiments on chemical combinations of airs and also on physical experiments on the expansion of airs with heat. The manuscript of his paper on specific and latent heats contains experiments on the heats of changes of state and also on the heats of chemical reactions.[148] A separate paper presenting the law of latent heats as a "hypothesis" states that the main cases of a change from an inelastic to an elastic state were "the evaporation of liquors & the separation of fixed air from alkaline substances."[149] Likewise his manuscript on the theory of heat contains a section "On the Heat & Cold Produced by Chymical Mixtures & by a Change from a Solid to a Fluid Form."

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[144] Buffon explained his ideas about gravitation in his *Histoire naturelle* in 1765. Hélène Metzger (1930, 57–60, 63). A.M. Duncan (1962, 228).

[145] Metzger (1930, 61).

[146] Macquer (1771, 1:324).

[147] A.M. Duncan (1970, 31).

[148] Henry Cavendish, (1921c, 347).

[149] Henry Cavendish, "Hypothesis All Bodies in Changing from a Solid State …," Cavendish Mss, Misc.

Parallel statements are made about chemical change and physical change of state: "It seems a natural consequence of this theory [of heat as the motion of particles] that the mixture of two substances which have a chemical affinity should commonly be attended by an alteration of sensible heat"; likewise "it seems a necessary consequence of the theory" that the change of state of a body, from a solid to a liquid or from either of those forms to an elastic fluid or air, should be accompanied by a change in the heat.[150] The phenomena of chemical reactions and those of changes of state are different, but from the point of view of particles and their forces there is no difference between chemistry and physics.

In importance, calorimetry, the quantitative study of heat, was a "close rival to the new pneumatic chemistry in the eyes of chemists and other men of science during the last quarter of the eighteenth century."[151] This statement applies to Cavendish's researches. Through his theory of heat, he acquired a degree of understanding of chemical phenomena such as the release of air fixed in bodies. He faced the familiar difficulty in developing chemical theory starting from mechanics, the forces and arrangement of particles being unknown, but in his theory of heat the exact laws of the forces did not need to be known. His starting point in studying the behavior of particles of bodies was the law of conservation of energy including the energy equivalent of heat, which is compatible with an infinte range of central forces. With the help of the conservation law, and reasoning from particles interacting through unspecified attractions and repulsions, he could explain the chemical behavior of bodies. In addition to the phlogiston theory, which explained chemistry at the level of phenomena, Cavendish had an explanation at the level of invisible particles. Because it was based on the identification of heat with energy and on the law of conservation of energy, it would become a valuable theory for chemistry in the next century. Cavendish and Lavoisier, once past their differences on phlogiston and oxygen, were close in their recognition of the importance of heat in the advance of chemistry. Lavoisier explained different states of matter and chemical processes as the outcome of the attractive forces of particles and the repulsive force of the material of heat, "caloric," and he looked to calorimetry to determine the strength of the attractions. He and his collaborators sought a "theoretical structure for chemistry" in which chemical constitution would be explained by the "interplay of heat and affinities." They did not yet know how to realize their plan quantitatively and mathematically.[152] Cavendish made a start.

The word "affinity" has come up several times. In place of "attraction," some chemists preferred "affinity," which implied nothing about laws of force. They recognized that substances have specific affinities for one another, forming unions, which are discovered in the laboratory. Historically, affinity was associated with the alchemists' animistic sympathy or love, which was still a way of thinking in chemistry in the eighteenth century. Stahl thought that chemical combinations came about because of a similarity between the combining substances. In his *Elements*, Macquer agreed with Stahl, but later in his *Dictionary of Chemistry* he stated simply that affinity is a tendency to unite and adhere. In the second half of the eighteenth century, both "affinity" and "attraction" were understood to stand for the empirical fact of chemical combination. Bergman spoke of "attraction," as did Hales, Cullen, and Black, while Boerhaave, Priestley, Kirwan, and Cavendish spoke of "affinity."[153] By the

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[150] Henry Cavendish, "Heat," in Russell McCormmach (2004, 182–183).

[151] Robert Fox (1971, 22).

[152] Kim (2003, 15, 387, 392–393).

[153] A.M. Duncan (1962, 184–185; 1970, 33–34). Macquer (1771, 1:22–23).

use of the word "affinity," they did not necessarily forfeit the analogy of nature, for attraction could be taken as implicit in chemical affinity.

Affinities drew the attention of chemists around the same time as phlogiston, in the middle of the century.[154] After the writings by Stahl and Boerhaave early in the century, there was a lull in theoretical interest, as chemists concentrated on building the factual basis of chemistry, but by the time of Macquer's *Elements*, chemists were again seeking patterns in their work. Proved by "all the experiments hitherto made," affinity "whatever be its cause, will enable us to account for, and connect together, all the phenomena that Chymistry produces," Macquer wrote in his text.[155] After introducing the four elements and phlogiston, he laid down six "fundamental truths" about affinity, from which he set out to "deduce an explanation of all the phenomena of Chymistry."[156] Twenty-five years later in his book on "elective affinities," Bergman wrote that they were the "key to unlock the innermost sanctuaries of nature; the "whole of chemistry" rests on the doctrine of affinities, a "solid foundation at least if we wish to have the science in a rational form."[157] Throughout his chemical work, Cavendish made use of affinities in his reasoning.

When Cavendish took up chemistry, affinities were arranged in empirical tables, many versions of which were proposed through the eighteenth century, the majority of them in the middle decades, the 1750s through the 1770s. They served chemists as a guide to chemical processes, an advance over Newton's displacement series of metals and Stahl's "order in which metals dissolved."[158] The first table was published in 1718 by the French physician and chemist Etienne-François Geoffroy, who used a neutral term "rapport," which dissociated his table from the two main ways of referring to chemical combination, affinity and attraction. His table consists of sixteen columns and nine rows, each column headed by a substance or a group of substances, written compactly using the old symbols rather than words for chemicals; beneath each head is a list of substances it combines with, ranked in order of strength of affinity, which is the order of displacement, the guiding observation of which was that one substance has more disposition than another to unite with a third. The construction of affinity tables was laborious. Bergman, who made the most important improvement in them, said that his sketch of a completed table in his book on affinities in 1775 would require over 30,000 experiments to perfect, and he hoped for a long life and assistance. He said that to determine the order of attractions in a table took "all the patience and diligence, and accuracy, and knowledge, and experience of the chemist," who was required to analyze the smell, taste, form, solubility, tendency to effervescence, and other properties of his reacting substances.[159] With a few exceptions such as Cullen, Black, and Lewis, chemists who made or modified affinity tables were European not British.

It took some time for Geoffroy's table to receive notice in Britain. Peter Shaw referred to it in his lectures from 1733 and in his translation of Boerhaave in 1741. In Scotland, William Cullen included it in his lectures from 1747. The table itself was published by the physician Robert Poole in 1748, by Macquer in 1749, and by William Lewis in 1753.[160] At the end of his publication on magnesia alba in 1756, Black published a revised version of

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[154] Kim (2003, 222). Duncan (1970, 190).

[155] Macquer (1758, 1:12).

[156] Ibid. 1:14. Kim (2003, 207).

[157] Torbern Bergman (1785b, 9).

[158] Metzger (1930, 50). Duncan (1970, 177).

[159] Bergman (1785b, 65–70).

[160] Georgette Nicola Lewis Taylor (2006, 61–63).

Geoffroy's table. John Hadley included it in his plan of chemical lectures at Cambridge in 1758. From English and French sources, probably from both, Cavendish became acquainted with affinity tables. On a page of his laboratory experiments on arsenic, he drew two groups of four symbols each, the two groups being identical except for one symbol, the one for arsenic replacing the one for regulus of antimony (pure antimony). (Fig. 14.12). The first group gives us a clue which table of affinities Cavendish used at that time. It was not Geoffroy's, which lacks an entry for arsenic, but probably the expanded, twenty-eight column table published in 1751 by Christlieb Ehregott Gellert in his textbook on metallurgical chemistry,[161] (Fig. 14.13) which although it was directed to industry was of interest to chemists generally. It was talked about in the Royal Society, which apparently requested an English translation. Gellert used Stahl's phlogiston, and he drew on the work of chemists Cavendish was interested in, Marggraf among others. Macquer referred to Gellert frequently, and the translator of his *Dictionary* James Keir added both Geoffroy's and Gellert's tables. Gellert's table was referred to by Macquer's collaborator Baumé in his *Manuel de chymie* in 1763 and by other chemical authors.[162]

Tables of affinity were overly simple, occasionally exceptionable, and because of the complexity of salts they were incomplete, among other shortcomings. Most chemical reactions involved more than two substances, and the effect of the circumstances of a chemical reaction, particularly the temperature, qualified the usefulness of the tables, though later ones corrected for some of the deficiencies. Imperfect as they were, tables nevertheless were highly useful. They showed the building blocks of chemical compounds and the known compounds corresponding to a given substance, bringing order to the bewildering variety of chemical operations, analogous to the later periodic table, and like the periodic table they were predictive.[163] They had the additional virtue of not being linked to a particular theory of chemistry, instead providing common ground for chemists holding different views. Chemists sometimes associated affinities with Newtonian natural philosophy, but this was not fundamental, as is shone by other chemists who used affinities to make chemistry an autonomous science independent of natural philosophy. Chemists were more likely to speak of the "doctrine" of affinity than of the "theory," though the ultimate goal of affinity tables was theory, and the tables received criticism for lacking a theoretical base. A historian of chemistry writes of affinities as a "theory domain," which by the 1770s, around the time Cavendish first published on chemistry, was the "frontier of theoretical chemistry."[164] Affinity tables did not represent nature in the way that theories of natural philosophy did, identifying the causes behind the phenomena, but they ordered and foretold the phenomena that chemists regularly dealt with in their daily work, accomplishing what theories do, qualifying perhaps as a theory of a different kind or a proto-theory.

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[161] Christlieb Ehregott Gellert (1751).

[162] Gellert's book was translated by John Seiferth in 1766 as *Metallurgic Chymistry*, though it was not published until ten years later. Fathi Habashi (1999, 34–35). Duncan (1962, I:187–189; II:220–221). Antoine Baumé (1763, 7).

[163] Ursula Klein and Wolfgang Lefèvre (2007, 152).

[164] Duncan (1962, I:181; 1970, 34). Taylor writes that though affinity was not tied to a certain theory, affinity tables were "guided and determined by theoretical assumptions," and that different chemists had different theories of affinity (2006, 8, 16, 21, 28). Kim (2003, 222).

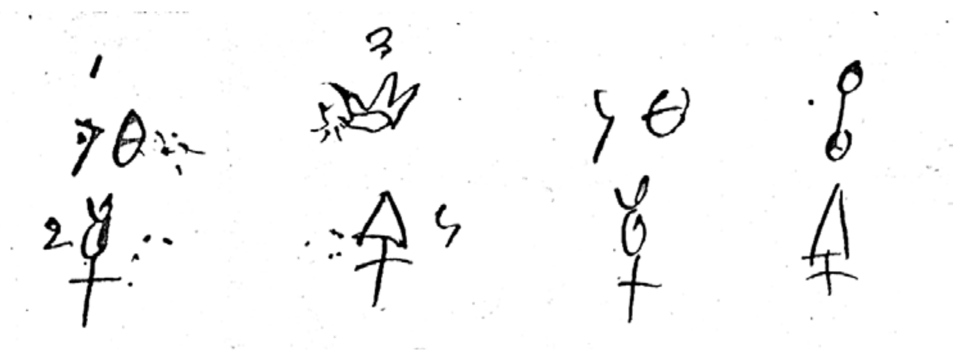

Figure 14.12: Affinities. Normally Cavendish spelled out the names of chemical substances, with the exception of mercury, for which he used the standard symbol. His unusual use of symbols to describe chemical reactions reproduced here is from his experiments on arsenic, Cavendish Mss II, 1(a), 3.

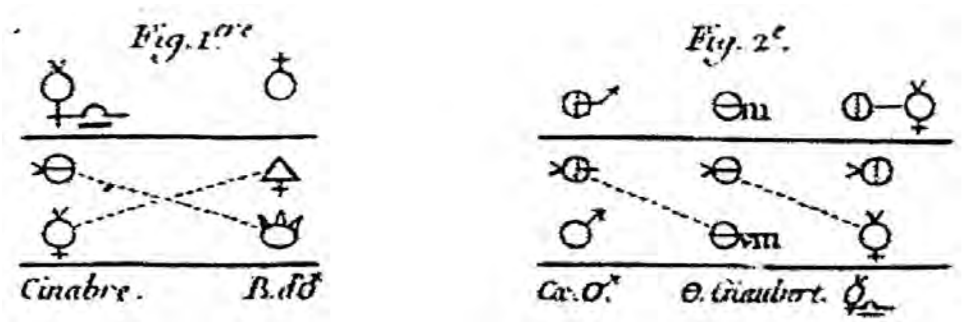

Figure 14.13: Affinities. The two figures showing examples of chemical reactions are a detail in the lower right corner of Gellert's table of affinities in 1750. The similarity with Cavendish's drawing in the previous figure is seen. The table is reproduced in Kim (2003, 223).

Some chemists foresaw a central role for affinities in a perfected science. Macquer's and Bergman's opinions on affinities as the foundation of the entirety of chemistry are quoted above. Antoine Fourcroy, Macquer's successor as lecturer in chemistry at the college of the Jardin du Roi, wrote in 1787 that "all chemistry" reduces to elective attractions, and "when the strength of this force between all natural substances has been determined, chemistry will be as complete as it possibly can be."[165] Lavoisier wrote that when sufficient chemical facts were known, the "geometer will be able to calculate, in his study, the phenomena of any chemical combination whatever, so to speak in the same manner as he calculates the movement of celestial bodies." At the time, he was collaborating with the mathematician and astronomer Laplace, planning experiments to measure chemical attractions.[166] In his treatise

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[165] Fourcroy quoted in Arnold Thackray (1970, 202).

[166] Duncan (1970, 29).

on the new chemistry in 1789, Lavoisier wrote that "the part of chemistry most susceptible, perhaps, of becoming one day an exact science, is that which deals with affinities or elective attractions." His list of elements resembles an affinity table, supplemented by hydrogen, oxygen, nitrogen, light, and heat. He had some criticisms of the current affinity tables, but his thinking was rooted in the tradition of affinity chemistry. He added a column for oxygen to affinity tables.[167]

Newton's ideas did not have much effect on the practice of chemistry or on the Chemical Revolution, but their legacy extended through the first half of the nineteenth century in the form of affinities defined as attraction at short range. During this time the attention of chemists was redirected to atoms and definite and multiple proportions in chemical combinations. The existence of atoms (or corpuscles or indivisible particles) had long been commonly accepted, and attempts had been made to relate their shapes and positions to the observed properties of substances. John Dalton selected a different property to relate to the observed properties, the weights of the atoms, a change which one historian of chemistry called the "most important step ever taken in the quantification of chemical theory." Dalton's first published statement on atomic weights came in 1805, and in 1808, two years before Cavendish's death, Part One of his *New System of Chemical Philosophy* set out his atomic theory in detail.[168] In the second half of the nineteenth century, chemists returned to the project of quantifying chemical affinities.[169]

Chemists in the eighteenth century had different opinions on the nature of chemistry. Some thought that it was a science, others that it was not yet, the former opinion gaining numbers as the century progressed. Some who thought that chemistry was a science thought of it as a separate science while others thought of it as a part of natural philosophy. Stahl regarded chemistry as a body of theoretical and practical knowledge distinct from mechanics, a chemical as opposed to a physical viewpoint. Macquer regarded the "science" of chemistry as a "fundamental and essential part of natural philosophy."[170] He was criticized by his translator James Keir for conflating affinity with attractions belonging to "natural philosophy" such as cohesion and gravitation, arguing that they may have the same cause but their effects are so different that they should be treated separately.[171] William Lewis likewise thought of chemistry as a science distinct from natural philosophy: in some cases "no boundaries can be established between them," but in other cases there were "essential and important differences." He said that whereas natural philosophy considers bodies whose parts have the same properties, "subject to mechanical laws, and reducible to mathematical calculation," chemistry considers different species of matter whose properties give color, taste, and smell, and are "not subject to any known mechanism, and seem to be governed by laws of another order"; "attraction" in natural philosophy is different from "chemical attraction," or affinity, by which two bodies become one, the properties of which are not "discoverable by any mathematical investigation," and the failure to distinguish between chemical and mechanical effects has resulted in error in the past.[172] Cavendish said nothing directly about how he thought of chemistry in relation to natural philosophy, but pneumatic

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[167] Ibid., 5, 41–42. Kim (2003, 342–343).
[168] Henry Guerlac (1961, 206–207).
[169] Kim (2003, 14, 16, 220).
[170] Macquer (1771, 1: Advertisement)
[171] Ibid. 1:22–23.
[172] William Lewis (1763, iii–iv).

chemistry, his special field, was by its nature a bridge between chemical and physical approaches. Airs, its subject, were freed and combined by chemical means, and they were studied individually by weights and volumes. Cavallo began the second part of his treatise on air with a prescription: the "branches of natural philosophy are so intimately connected with one another" that to advance one branch required knowing the others; in particular, to advance pneumatic chemistry, not only chemistry but mathematics, electricity, and hydrostatics needed to be known.[173] Cavendish studied phlogiston in its chemical combinations, and he studied it separately as an air in the same way he studied common air, as a ponderable elastic fluid. To analyze chemical activity, he used affinities, a concept equally useful in chemical and physical approaches, allowing him to proceed beyond bare facts without losing himself in unproductive theory. The evidence suggests that Cavendish did not concern himself with the distinction between chemical and physical approaches. He was a natural philosopher who carried out chemical researches, using what means were called for. In the catalog of his library, chemical books did not have a category of their own but were listed under "natural philosophy."

When chemists spoke of wanting their science to be like natural philosophy, they usually had in mind useful mathematical laws of chemistry deduced from experimental facts.[174] Black thought that chemistry lacking first principles was "not yet a science," and that the present task of chemists was to form a "general law" by induction, following Newton's *Opticks* as a model; they should avoid all "pretensions of a full system."[175] Cavendish looked for regularities, not for a complete system of chemistry, in implicit agreement with Black. His work on equivalent weights can be seen as implying a general law. His hypothesis that *all* metals including the perfect metals and mercury are deprived of their phlogiston when they are dissolved in acids is a potential law. In his most sustained series of chemical experiments, he looked for the cause of the diminution common air in *all* the ways it can be phlogisticated and for what becomes of the air, and he thought he found the answer, again a law. He showed little if any interest in reforming the nomenclature of chemistry or deciding on the chemical elements, matters which would have come up in any proposal of a full system of chemistry. His chemical researches were systematic in a different sense. Recognizing the implication of Black's work, that for chemistry to advance it was necessary to study factitious airs, he made a systematic examination of the properties of distinct factitious airs, the subject of his first publication in chemistry.

Given Cavendish's appreciation of the importance of theory to scientific advance together with his ability in making theories, it may seem strange that he did not take a more constructive interest in the theoretical side of chemistry. If chemistry lacked first principles, as Black said, and if for this reason it was insufficiently developed to consider making a theory of chemistry as a whole, Cavendish might have considered making a theory of the part of chemistry he knew best, pneumatic chemistry. Priestley had that in mind when he said that his goal was a "*general theory* of all the kinds of air," only the experimental evidence was lacking.[176] Cavendish might have agreed about the evidence, but in any case he showed no inclination to attempt Priestley's theory. He looked for general results with the help of

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[173] Cavallo (1781, 157).

[174] Duncan (1970, 26).

[175] Quoted in Brock (1992, 271).

[176] John G. McEvoy (1968, 117).

affinities within the framework of the phlogiston theory, which he did not have to invent but only modify.

Lavoisier was inferior to Cavendish in experimental skill, but he was superior in his "ability to relate parts to the whole, and so erect a large theoretical structure."[177] There is no way of knowing exactly why Cavendish did not do something, but it is worth noting that in the two comprehensive theories he worked out, for electricity and heat, he began with a hypothesis about the cause of the phenomena, which he then elaborated mathematically. Chemistry in his day did not offer him a comparable opportunity to exercise this skill.

Exactitude

When after completing his paper on the theory of electricity, Cavendish found that Aepinus had published a theory based on nearly the same hypothesis, he justified publishing his own because he had developed it much further and, as he said, "I flatter myself, in a more accurate manner."[178] When his first paper, on factitious air, was read to the Royal Society, he was thanked for his "Accurate paper."[179] In the year he published his last chemical paper, a colleague referred to him as "that most accurate philosopher."[180] "Possessing depth and extent of mathematical knowledge" Humphry Davy said that Cavendish "reasoned with the caution of a geometer upon the results of his experiments."[181]

One key to Cavendish's accuracy was his understanding of instruments, which in turn rested on his understanding of the science underlying them. This is shown by advice he gave on how to operate an air pump (Fig. 14.14). John Smeaton claimed that his improved air pump gave rarefactions of 1000 or 2000 times instead of the previous limits of under 150. Implicit confidence was placed in his claim until the instrument maker Edward Nairne discovered a fallacy, which he recognized after obtaining incredible rarefactions of 100,000. By making comparisons with other standard gauges, Nairne saw that the error lay in Smeaton's new gauge, a pear-shaped bulb holding mercury, but not knowing the reason for it, he performed an experiment with the air pump before Smeaton and other interested fellows of the Royal Society. One of them was Cavendish, who recognized that the discrepancy between the gauges was due to water vapor. He explained that to get the gauges to agree the air pump must be as free as possible of all traces of water, since Smeaton's gauge did not measure vapor pressure in addition to air pressure as other gauges did. When Nairne took this precaution, the gauges agreed, and the rarefaction proved to be a believable 600. Cavendish's explanation was based on his father's experiments, which showed that whenever the pressure of the atmosphere on water is reduced to a certain level, which depends on temperature, the water is immediately turned into vapor and is as immediately turned back into water upon restoring the pressure.[182]

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[177] Frank Greenaway (1776/1970, xii).

[178] Henry Cavendish (1771, 33).

[179] 6 Nov. 1766, JB, Royal Society 25:927.

[180] James Keir, quoted in Joseph Priestley (1788, 327).

[181] Humphry Davy (1812, 37).

[182] This clarification of the air pump in 1776 was described by Nairne in a paper and by Charles Hutton in his entry "Air" in *Mathematical and Philosophical Dictionary* (1795–1796, 1:56–57).

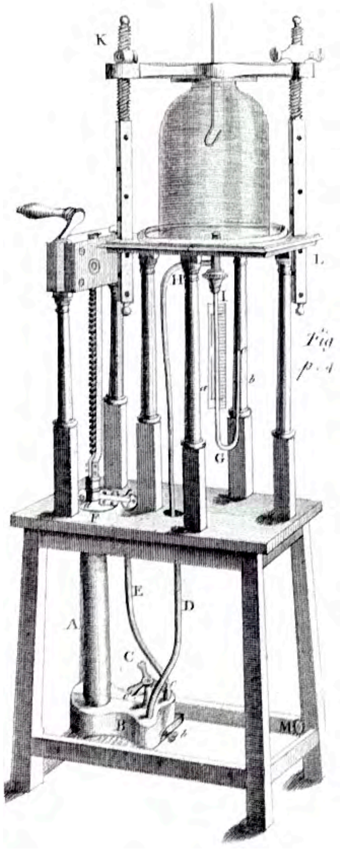

Figure 14.14: John Smeaton’s Air-Pump. The left-hand figure shows A barrel, B cistern, C handle of cock, D pipe communicating from cock to receiver, E pipe between cock and valve, GI siphon gauge. The right-hand figure shows the new gauge, a glass holding about a half pound of mercury, held up by the brass piece DE and open at A; the graduated tube BC is closed at C. While the receiver is being exhausted, the gauge is suspended in it. When the pumping is done, the gauge is lowered so that its open end is immersed in a cistern of mercury. The air is then let in, driving mercury up into the gauge until the air remaining in it is of the same density as the external air. The rarefaction of the air in the receiver can then be read off from the number of divisions occupied by the air at the top. Cavendish noted that the air trapped in the gauge contains water vapor; compressed by the mercury, the vapor at a certain point is turned into liquid water, eliminating the partial vapor pressure and thus allowing readings of unprecedented rarefactions. In other gauges of the time, this phenomenon did not occur. The gauge is described by its inventor, John Smeaton (1752b, 421); illustration of the air-pump opposite, 424. Cavendish’s analysis of the pair-gauge is given by Edward Nairne, *PT* 67 (1777): 622.

When someone objected to his explanation of the difference between the gauges, Cavendish said that the objection would be credible except for a "circumstance" he neglected to mention: "while any air is left in the receiver the pressure therein will be greater than if it contained only the vapor of water."[183] The circumstance was the principle of partial pressures, which Cavendish used in various calculations, but which would only become generally known in the next century with the work of John Dalton.[184]

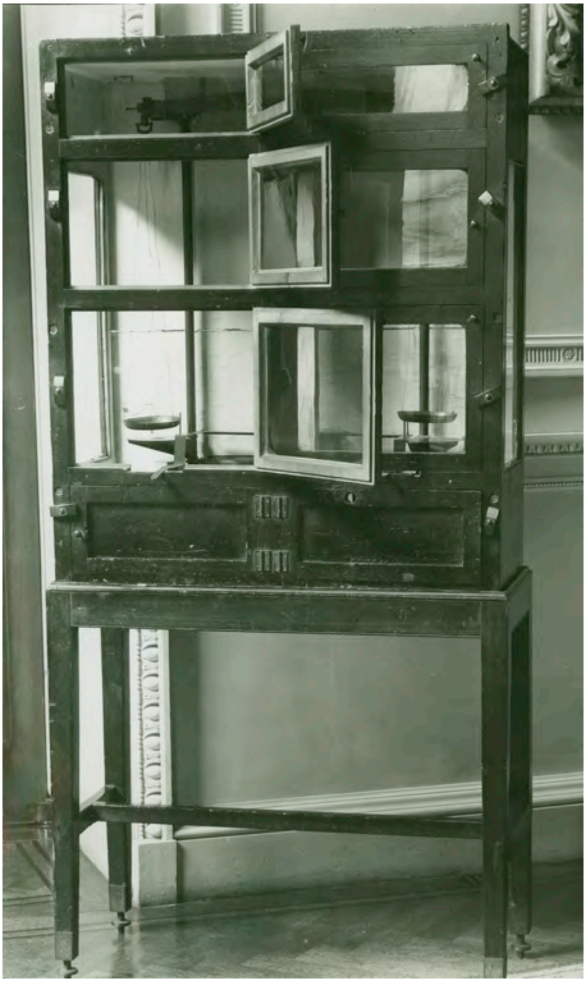

Figure 14.15: Chemical Balance. Built by "Harrison," this instrument is the earliest of the great precision balances of the eighteenth century. Reproduced by permission of the Royal Institution of Great Britain.

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[183] The person Cavendish addressed is not named. Cavendish Mss IV, 4.

[184] S.A. Dyment (1937, 473).

The instrument that typifies Cavendish's exactness in chemistry is the balance. We have mentioned that Cavendish owned the first of the great precision balances of the eighteenth century (Fig. 14.15). The beam of his balance was not suspended, as it was in earlier balances, but rested on a hardened steel knife edge standing on a steel plate. It was made of a wide flat iron bar, triangular in form, and $19\frac{1}{2}$ inches long. Suspended from its ends by thin wires attached to brass universal joints were weighing pans measuring about 1 foot across. There was a threaded rod with a nut on each arm of the beam to adjust the center of gravity of the beam to compensate for unequal weight in the arms. The index instead of being at the center of the beam was placed at one end and oriented horizontally, increasing the size of the movement of the beam over a graduated arc. The balance was enclosed in a rough wooden case with glass windows standing on a table. It was capable of weighing to an accuracy of 5 milligrams.[185] There is no date on it, but the maker's name is known to be "Harrison," who may have been William Harrison, who was employed by Cavendish. This Harrison had worked under Jesse Ramsden, who built the one other great precision balance in Britain, owned by the Royal Society.[186] Cavendish's preference in chemistry for weighing is explained by what Blagden said about this form of measurement: "quantities can be determined to much greater exactness by weight than by any practicable way of measurement."[187] The determination of weights, William Nicholson wrote, was "half the business of the chemist."[188] Wilson singled out Cavendish's weighing as the one certainty among the debated events of the "water controversy."[189]

In addition to Cavendish's and the Royal Society's early precision balances, there was a third one, owned by Lavoisier. His balance had the same innovative features as the other two, though they were realized differently.[190] With the help of the precision balance, chemistry was becoming a science exact enough to attract an astronomer. When Lavoisier learned of Cavendish's experiments on water, he made his own with the assistance of Laplace.[191] So caught up in chemistry was Laplace that Blagden asked a French colleague if what he was told was true, that Laplace "had renounced his mathematical studies, & was applying himself solidly to chemistry."[192] When Laplace read Cavendish's paper on water, he wrote to Blagden that Cavendish's experiments were "infinitely important," made with the "preci-

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[185] Ernest Child (1940, 79). Maurice Daumas (1972, 134, 222–223). Precision balances first appeared in assaying offices, in the 1770s.

[186] From a list of Henry Cavendish's servants at his death in 1810, we know that his instrument maker's name was William Harrison, who was sixty-one at that time. It could be another Harrison. Cavendish's balance is attributed to Thomas Harrison in Mary Holbrook (1992, 169). It is attributed to John Harrison by Maurice Dumas, who says that only a clock-maker would have had the skill to make it (1972, 134, 222).

[187] Charles Blagden (1790, 325).

[188] William Nicholson, in his translation of notes by French chemists to the French edition of Richard Kirwan (1789, viii).

[189] Wilson (1851, 363).

[190] Daumas (1972, 225–226).

[191] Lavoisier could be careless at times. When he and Laplace burned oxygen and hydrogen to obtain water, they did not keep track of the exact quantities of the gases, assuming that the weights of the gases and of the water formed from them were equal. According to Blagden, who witnessed it, Lavoisier and Laplace's first experiment on the production of water was "good for nothing as to determining the proportions of air & water," and their only dependable result was the test of the purity of water; they intended to repeat the experiment with the "necessary precision," but the account of this first experiment was read before the Academy of Sciences anyway. Charles Blagden to Joseph Banks, 25 June 1783, BM(NH), DTC 3:56–58. Henry Guerlac (1975, 78).

[192] Charles Blagden to Claude Louis Berthollet, 8 Dec. 1789, draft, Blagden Letterbook, Royal Society 7:377.

sion and finesse that distinguish that excellent physicist."[193] This may be taken as a tribute from one astronomer to another, both of whom were working in chemistry. What Laplace said of Cavendish's work others who commented on it said too: it was distinguished for its exactitude.

Measuring and weighing, the recording of objects in numbers, presuppose standards. In his work with the eudiometer, Cavendish introduced a "standard" of goodness (oxygen) based on the goodness of common air, which was itself a standard; as he had shown, the composition of common air is constant, the criterion of a standard. In his experiments on electricity, he introduced a standard for capacitance, that of a conducting globe of a certain diameter. In his experiments on freezing solutions, he determined the specific gravities of acids of various strengths, which he specified by the weights of a standard substance, marble, dissolved by a unit weight of the acids. In his experiments on specific heats, he introduced pure water as a standard substance; using a specified weight of water as reference, he calculated the equivalent weights of substances in terms of their heating effect.

Cavendish introduced the word "equivalent" in chemistry, though the concept went back to the turn of the eighteenth century, to Wilhelm Homberg's quantitative experiments on the neutralization of alkalis by acids.[194] In his experiments, Homberg ignored the weight of gases absorbed and given off, a deficiency which Black pointed out in his work on magnesia alba.[195] James Keir corrected Homberg's table of the equivalent weights of acids referred to salt of tartar with numbers he took from Cavendish's 1766 paper on factitious air.[196] In that paper Cavendish introduced marble as a standard equivalent weight: he first determined the proportion of fixed air in marble, and then with reference to 1000 grains of marble he found the equivalent weights of fixed air in several other alkalis, volatile sal ammoniac, pearl ashes, and salt of tartar. Following is an example of Cavendish's determination of equivalent weights and of their use in explaining chemical reactions. By combining a sample of the alkali volatile sal ammoniac (solution of ammonium carbonate in alcohol) with the acid spirit of salt (hydrochloric acid), Cavendish determined two weights: 1661 grains of the sample saturated as much acid as 1000 grains of marble did. In this case, 1661 grains and 1000 grains were equivalent weights of the two alkalis. He determined that 1000 grains of marble contained $407\frac{1}{2}$ grains of fixed air, and that 1661 grains of volatile sal ammoniac contained 885 grains of fixed air. These numbers show that volatile "sal ammoniac contains more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion […] 885 to $407\frac{1}{2}$." This result accounted for a "remarkable phenomenon," Cavendish said. To a solution of the alkali chalk in the acid spirit of salt he added a solution of volatile sal ammoniac, producing a considerable effervescence, which "surprised" him since the acid was perfectly neutralized by the chalk. The explanation was that volatile sal ammoniac contained more fixed air in proportion to the quantity of acid that it could saturate than chalk did (chalk taking the place of marble in the previous example). In the solution, the volatile sal ammoniac united to the acid (it having greater affinity to the acid than the chalk did), losing its fixed air, part of which united to the chalk that was separated from the acid, and part of which "flew off in an elastic form," since the chalk was not able to absorb

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[193] Pierre Simon Laplace to Charles Blagden, 7 May 1785, Blagden Letters, Royal Society, L.181.

[194] Marie Boas Hall (1972). J.R. Partington (1961–62, 3:44–45).

[195] Black (1898, 17–18).

[196] Partington (1961–62, 3:320).

all of the fixed air.[197] Equivalent weights were weights with which the chemist could make physically meaningful comparisons.

Cavendish applied the concept of equivalents throughout natural philosophy. In his theory of heat, he proposed an experiment to measure the mechanical "equivalent" of heat, the term he used.[198] In his study of compounds of tartar, he determined the "equivalent" weights of several alkalis.[199] In his experiments on the specific heats of various substances, he stated the results in terms of the "equivalent" weight of water. In his experiments on the heats generated by mixing acids and different alkalies, each of the alkali solutions had as much alkali as "equivalent" to a specified weight of marble.[200] In his experiments on electrical resistances, he prepared solutions of neutral salts in which the amount of acid in each was "equivalent" to that in a solution of sea salt of a certain concentration.[201] By the use of the balance together with standards and equivalents, Cavendish gave to his work its characteristic stamp of exactitude.

In several experimental fields, Cavendish introduced the law of reciprocal proportions, which he called "a constant rule" of nature. He began the account of his electrical experiments with the rule that when two charged bodies are successively connected to a third body, the proportion of the charges in the two bodies is the same as the proportion of the charges of the third body successively connected to one and the other body. If he said that the charge of a thin circular plate is to the charge of a globe of the same diameter as 1 to 1.57, he meant that if the two bodies were successively connected to a trial plate, the two charges on the trial plate would bear the same proportion, 1 to 1.57.[202] This was the justification of the trial plate, the instrument he used throughout his experiments on the charges of bodies. He established a similar rule about the heating effects of two bodies on each other and on a third body, the justification of the principal instrument in his researches on specific and latent heats, the mercury thermometer.[203] He did not take his reciprocity rules in heat and electricity as self-evident but carried out experiments to prove them. In his chemical work on freezing mixtures Cavendish assumed without comment the rule of reciprocal proportions, later proposed by Richter.[204] The rule was a precondition of "strict reasoning" about nature, the habit of thought behind Cavendish's reputation for exactitude.

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[197] Cavendish (1766, 93).

[198] McCormmach (2004, 134–135).

[199] Cavendish Mss II, 2(b). Unnumbered pages at the end.

[200] Cavendish Mss III(a), 9:82.

[201] In addition to neutral salts, Cavendish prepared solutions of fixed alkali and acids. Maxwell found that when the amounts of the salts and acids are expressed in pennyweights, they are very nearly equal to their equivalent weights in modern chemistry, where the equivalent weight of hydrogen is taken as 1. The remarkable agreement is not just in ratios but in absolute numbers, which comes from Cavendish's practice of using as a standard the equivalent weight of marble, the modern value of which is 100. By taking 100 pennyweights of marble as the standard, the equivalent weights of the other salts and acids come out as Cavendish stated them. Henry Cavendish (1879h, 329–330; 1879k, 360–361). Maxwell's commentary, ibid., lxii–lxiii.

[202] Henry Cavendish (1879b, 114–115).

[203] Henry Cavendish (1921c, 340).

[204] Wilson pointed out Cavendish's use of the "law of reciprocal proportion." A certain quantity of sulfuric acid saturates a given quantity of a particular alkali, and a different certain quantity of nitric acid saturates the same quantity of alkali. This quantity of nitric acid dissolves 33 parts of marble. It follows from the rule that the above quantity of sulfuric acid also dissolves 33 parts of marble. Cavendish estimated the strength of acids by the quantity of marble they dissolved, but because in the case of sulfuric acid, the quantity of marble it dissolved was not an accurate method, he estimated its strength by comparing it with nitric acid, which was accurate. This is why he took the roundabout way, making use of reciprocity. Wilson (1851, 465).

It is clearer to us than it was to Cavendish's contemporaries that his direction in chemistry would prevail. British resistance to Lavoisier's anti-phlogistic chemistry was partly based on distrust of his claims for quantitative accuracy and even of the relevance of those claims to the disputed issues in chemistry, and as well of his geometric model of reasoning in chemistry.[205] Cavendish's direction may not have had unanimous support, but his marked preference for quantitative methods represented an influential opinion both in Britain and abroad. Black, it is said, was not so much a chemist as a natural philosopher, who looked for general laws and made use of quantifying instruments, the balance and the thermometer. Lavoisier applied to chemistry the methods of experimental physics, with its reliance on instruments of measurement; without measuring and weighing, he said, "neither physics nor chemistry can any longer admit anything whatever."[206] Precision was not yet of decisive importance, but the recognition of an indispensable role for weighing and measurement in chemistry insured that it was coming.

As indicators of the trend, we note several examples of measuring and weighing in the late eighteenth century and the beginning of the nineteenth century, a time when about a third of chemical publications were quantitative.[207] The examples have been mentioned earlier in different contexts. Carl Friedrich Wenzel, director of Freiburg foundries, having observed that acids and bases combine in constant proportions published a table of equivalent weights in 1777.[208] In 1781–83 Kirwan made specific gravities of compounds and their ingredients a measure of their affinities, and he made equivalent weights a measure of the affinites of acids for bases, in his opinion the foundation of "chymistry, considered as a science."[209] Richter's weights of bases required to neutralize a given quantity of an acid were published in tabular form in 1802. Following Lavoisier's *Treatise* in 1789, much of the research in chemistry was directed to determining by weight the constituents of compounds with ever greater accuracy. J.L. Proust presented the law of definite proportions in 1797. Dalton published the law of multiple proportions in 1804, and the next year he first mentioned in print his atomic theory of chemistry, which made atomic weights a way of distinguishing substances quantitatively. In 1809 Gay-Lussac determined the law of definite proportions for volumes of reacting gases. From the end of the eighteenth century, physical constants and measurable properties of substances entered standard chemical textbooks.[210] Quantitative differences took on increasing importance in differentiating and combining substances, always a principal objective of chemistry. Cavendish lived long enough to see his approach successfully pursued by many chemists.

Concern with accuracy was at the same time concern with error. Cavendish would seem to be preoccupied with error, but he was practicing good science as it was increasingly done. Francis Wollaston wrote to Herschel, "I believe we both of us have the advancement of science too much at heart to decline acknowledging an error." Errors could be looked at as opportunity. Herschel thought that a theory by John Michell was fundamentally in error but also that it was "of the utmost importance, its being contrary to facts being a point of

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[205] Jan Golinski (1992, 130–152).

[206] A.L. Donovan (1975, 201, 215, 220–221; 1993, 49). Brock (1992, 117).

[207] H. Gilman McCann (1978, 143–146).

[208] Ihde (1964, 96).

[209] Richard Kirwan (1781, 8–9; 1783, 34, 36, 38).

[210] Guerlac (1961, 197, 199, 203–205, 211).

almost as much consequence as its agreeing with them."[211] James Hutton said that errors are not grounds for skepticism; on the contrary, they "contribute for establishing the certainty of science, when these are properly corrected."[212] "Errors may lead to truth,"[213] Dalrymple said simply. Cavendish took precautions to reduce, not unrealistically to eliminate, the "error of the observer," the "error of the instrument," and overall the "error of the experiment." His work in chemistry belonged to a common world of scientific practice, which valued accuracy, considered errors, introduced standards of measurements and practice, and improved instruments.

Cavendish's direction in science may have had sources outside of science as well. The caution with which he moved through his life has a parallel in his analysis of the circumstances of experiments, a point we take up in the last chapter. There may have been an additional source arising from his place in society. Measurement has long been a trademark of authority and sovereign power. Measures are legislated, and standards that secure them are kept by a central authority. Governments impose uniform measures ensuring an orderly commerce by providing all parties with a common language. Scientific organizations desire uniform measures for similar reasons, and the same measures are used in the activities of civil society and in scientific work. The desirability of exactness in weighing was recognized in commerce and in science in Cavendish's time. As a member of the ruling class, Cavendish might have instinctively imbued his work with the common language of authority: number, weight, and measure.[214]

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[211] Francis Wollaston to William Herschel, 22 Mar. 1789, Royal Astronomical Society, Herschel Mss W 1/13, W. 193. William Herschel to Samuel Vince, 15 Jan. 1784, ibid., W 1/1, 92–95, on 93.

[212] James Hutton (1794, 6).

[213] Howard T. Fry (1970, xiii).

[214] Witold Kula (1986, 18). Kisch (1965, 8).

Chapter 15
Mercury

After chemistry and electricity, heat was the third major experimental field in the eighteenth century. Benjamin Thompson, a leading investigator in the field, compared heat with gravity as a principal mover in nature: "The effects produced in the world by the agency of Heat are probably just as extensive, and quite as important, as those which are owing to the tendency of the particles of matter towards each other," and "its operations are, in all cases, determined by laws equally immutable."[1] Heat, Joseph Black told his students, "is certainly the chief material principle of activity in nature," and if it were removed, "a total stop would be put to all the operations of nature."[2] His student William Cleghorn said that without heat, "Nature would sink into chaos." Of fields of investigation, he said, "nothing will seem more deserving of the attention of philosophers" than heat.[3] Heat awaited its Newton, who would lay down its laws and erect a system to stand beside the theory of gravitation and the system of the Sun and planets. As he did in electricity, Cavendish set out on this quest.

Specific and Latent Heats

Heat was a difficult field. The chemist and physician Adair Crawford, a pioneer in the measurement of specific and latent heats, explained the difficulty of performing repeatable experiments in heat: "A change in the temperature of the air in the room, a variation in the time that is employed in mixing together the substances which are to have their comparative heats determined, a difference in the shape of the vessel, or in the degree of agitation that is given to the mixture, will often produce a considerable diversity in the result of the same experiment."[4] In his experiments on heats, Cavendish made corrections, took the mean of repeated trials, and followed up every source of error. With his precautions, and with the help of good thermometers, he achieved, in Crawford's words, a "very near approximation to the truth." Wilson said that Cavendish's experiments on heat showed "all the precision and accuracy" we have come to associate with him.[5]

At about the same time that Cavendish carried out his first dated chemical experiments and began preparing for his electrical researches, he undertook a series of experiments on specific and latent heats, which he recorded in an untitled, indexed packet of 117 numbered octavo sheets. Because the first and earliest date, 5 February 1765, occurs near the end of the record, we assume that the experiments began in 1764.[6] Their sequence follows more or less

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[1] Benjamin Thompson (1798); in (1870–1875, 1:491).

[2] Joseph Black (1803, 1:11–12).

[3] Douglas McKie and Niels H. de V. Heathcote (1958, 13–15).

[4] Adair Crawford (1779), advertisement.

[5] George Wilson (1851, 447).

[6] Cavendish Mss III(a), 9:89. On pp. 92 and 94, there are two more dates, both in 1776; the experiments involve freezing mixtures.

a progression of questions and answers. Cavendish sometimes reordered experiments, but usually he cross-referenced them, and in any case the interruption of chronology is minor and obvious. The bundle of sheets conveys the feel of experimental research leading to important, sometimes unanticipated results. This work was comparable in thoroughness to his experiments on air and on electricity. Because heat enters into the phenomena of most branches of experimental science, we need to know how Cavendish treated it to understand how he approached natural philosophy.

The sheets are not the original slips containing measurements recorded in the laboratory but an intermediate record, from which Cavendish wrote a paper, fifty quarto pages in length, "Experiments on Heat" (not Cavendish's title). Wilson said that if Cavendish had cared to publish this paper, it "might at once have been printed."[7] The paper is not that close to publication,[8] but Wilson was right that if Cavendish had wanted to publish it, he had a draft of much of it and most of the material for the rest of it. As it stands, the paper was written for an unidentified specific reader in mind, whom we know only as "you."

When Cavendish came forward as a researcher in the 1760s, the experimental field of heat had begun to be developed as a quantitative science. Central to this development was the distinction between thermometer readings and quantities of heat, on which the quantitative concepts of specific and latent heats depended. Although the immediate stimulus for Cavendish's heat experiments is unknown, a reasonable speculation can be made about it.

Apart from Cavendish's own work, the important researches on heat were not made in London. He mentioned only one name in his experimental notes, which comes at the very end of the packet, "Martin,"[9] clearly a reference to the Scottish physician George Martine, who in 1740 published an account of rates of heating and cooling.[10] In his paper "Experiments on Heat," Cavendish mentioned three names in connection with latent heat. One was the French physical scientist Jean Jacques Marain, who observed the generation of heat in the freezing of water.[11] The other two were the Scottish chemists Cullen and Black, whose work was current.

Cullen, the older of the two, was professor of medicine and lecturer in chemistry at the University of Glasgow, in whose laboratory Black worked for a time. When Cullen moved to the University of Edinburgh Black succeeded him in Glasgow, and ten years later Black again succeeded him in Edinburgh as professor of medicine and chemistry, a position he held for over thirty years.[12] Prompted by the simple observation by a student that a thermometer cools when it is removed from a solution, and suspecting that evaporation is the cause, Cullen made a series of experiments to find out. He evaporated some thirteen acidic and alkaline liquids, listing them in order of their power to produce cold and obtaining cold of "so great a degree" that he suspected no one had observed it before. He thought that the whole subject should be "further examined by experiment."[13]

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[7] Wilson (1851, 446).

[8] This paper is published: Henry Cavendish (1921c). The manuscript of the paper consists of 41 numbered pages followed by 9 unnumbered pages. The numbered pages are complete, but the remaining ones are sketchy.

[9] Cavendish Mss III(a), 9:114.

[10] George Martine (1740).

[11] Cavendish (1921d). His source was probably J.J. d'Ortous de Mairan (1749).

[12] In Glasgow Black was professor of anatomy but soon exchanged duties with the professor of medicine. In Edinburgh Cullen took over the chemistry chair in 1766, freeing the chair of medicine and chemistry, which Black took over. In Scottish universities, there was a good deal of shuffling of chairs. Ramsay (1918, 31, 47).

[13] Cullen's paper was first published in 1755 in *Edinburgh Philosophical and Literary Essays* and was republished together with Black's essay: *Experiments upon Magnesia Alba, Quick-lime, and Other Alkaline Substances; by*

Stimulated by Cullen's experiments and by an observation of Daniel Gabriel Fahrenheit's on super-cooled water, reported in Herman Boerhaave's *Elementa Chemisticae*, perhaps as early as the winter of 1757–58 Black lectured on the heat accompanying changes of state of substances. To convey the concept, he gave a homely and effective example: if snow and ice were to melt immediately at the melting temperature, the commonly held view, then every spring the world would suddenly be overwhelmed by floods, which "would tear up and sweep away every thing, and that so suddenly, that mankind should have great difficulty to escape from their ravages." The reason why this did not happen is that it takes time for ice and snow to absorb the heat that originally is lost in the change of state of water to ice and snow; the heat that is latent in the water does not register on the thermometer. In 1761 Black measured the heat of fusion of ice, reporting on it to the local scientific club in Glasgow the next year.[14] In 1764, Black together with his student William Irvine measured the latent heat of steam by condensing water vapor in a worm tube immersed in a cold water bath. He extended the investigation to substances other than water: at his request, Irvine measured the latent heats of metals such as tin and soft substances such as spermaceti and beeswax. The term "latent heat" is Black's, standing for the heat absorbed or generated in a change of state.[15]

In 1760 Black arrived at his second important discovery, specific heats. He was guided to it again by an experiment of Fahrenheit's reported in Boerhaave's text on chemistry and also by an experiment in Martine's essay, both experiments pointing to different heating effects of water and mercury. Black recognized that different kinds of matter communicate heat differently, having different heat "capacities," another name for which is "specific heats."[16] Specific heat is the heat required to raise the temperature of a given weight of a specific substance one degree; Black used water as the standard substance. Latent and specific heats were new, permanent, and characteristic properties of substances.

Black published nothing of his work on heat, but student notes of his lectures were in circulation by 1767, and an anonymous account of his lectures was published in 1770; in addition his students Adair Crawford and William Cleghorn published his views on heat.[17] Irvine too published nothing of his work on heat. His papers were collected and published after his death, but by then his work was well-known. By the late 1770s, a serious investigator of heat would have known about Black's and Irvine's work in some detail.[18]

Black's work can be seen as the beginning of the quantitative study of heat. He agreed with Boerhaave that the thermometer measures heat, but what it measures is the intensity of heat, not the quantity; and he agreed with Boerhaave that heat seeks equilibrium, though in equilibrium the intensity, or temperature, is the same, not the quantity of heat, a confusion Boerhaave made. Black was able to discover specific and latent heats because of his sound method of measuring heat.[19]

Joseph Black. To Which Is Annexed, An Essay on the Cold Produced by Evaporating Fluids, and of Other Means of Producing Cold; by William Cullen (1898, 132).

[14] McKie and Heathcote (1935, 16, 35). Henry Guerlac (1970, 177). A.L. Donovan (1975, 238–240).

[15] Donovan (1975, 240–246).

[16] Guerlac (1970, 178–179).

[17] Robert E. Schofield (1970, 186).

[18] The Swedish physicist Johan Carl Wilcke discovered latent heat independently of Black and later, in 1772. Unlike Black he published his work on latent and specific heat, discussed in McKie and Heathcote (1935, 54–121).

[19] Schofield (1970, 188–189). By his method, Black measured "heat exchanges in terms of a temperature change of so many 'degrees of heat on Fahrenheit's scale' for an equal mass of water." McKie and Heathcote (1935, 122).

If not from the beginning, by the time he wrote up his heat experiments as a paper, Cavendish knew about Cullen's work on evaporation.[20] He could have come across it in a publication from Edinburgh in 1756, or in conversation with Scottish guests at meetings of the Royal Society and its dining club. He may also have heard about Cullen's experiments from his colleague John Hadley, who repeated one of them in the presence of Benjamin Franklin in 1762.[21] Cavendish knew something about Black's work too. He was "informed" that Black had made observations on distilling water in a worm tube, and not knowing how the experiment came out, he repeated it. In addition to whatever information he acquired informally about Black's or his students' work on heat, he undoubtedly read the same book as Black, Boerhaave's text on chemistry, which was recommended reading at Cambridge when he was a student.[22] He would have read there about Fahrenheit's experiments on hardening and melting, which showed that a change of state of a substance involves a heat that does not register on the thermometer, and about Fahrenheit's demonstration that mercury and water have different heat capacities.[23] He also may have known about Brook Taylor, who in the *Philosophical Transactions* in 1721 published a study of thermometers, in which he mixed given quantities of hot and cold water and measured the resulting temperature.[24] With a similar intention, Cavendish began his researches with experiments to insure that the mercury thermometer is an accurate, uniform measurer of temperature.[25]

Cavendish's experiments on heat were contemporary with Black's or slightly later. Because Black did not describe his method of measuring specific heats, we do not know how close his was to Cavendish's. The equipment Cavendish used for his experiments consisted of thermometers, lamps for heating substances and mixtures, containers made of glass or tin, scales, and a time-keeper. He took three readings three minutes apart to determine the rate of cooling, and he did a separate experiment to determine the heating effect of the container. His method was that of mixtures. He first experimented with the simplest mixture, hot and cold water, which had been studied before him by Fahrenheit, Taylor, and Black.

The paper containing the results, "Experiments on Heat," is reproduced nearly in entirety in Cavendish's *Scientific Papers*, including the first page, which in the manuscript is largely crossed out by Cavendish. On the bottom of that page, separated by a line from the text, perhaps indicating a footnote, is a detail that is not reproduced. The detail, which explains what is otherwise hard to understand by a verbal description, gives the only equation I have found in Cavendish's scientific papers. It expresses the basic law behind the method of mixtures, as applied to the simplest mixture, hot and cold water:

$$m(H + C) = hH + cC, \tag{15.1}$$

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[20] Cavendish wrote: "Dr Cullen has sufficiently proved that most if not all fluids generate cold by the first species of evaporation." By "first species," Cavendish meant evaporation produced by heating a liquid without boiling it, which he attributed to absorption by the air. Cavendish (1921c, 344).

[21] Benjamin Franklin to Ebenezer Kinnersley, 20 Feb. 1762, in Benjamin Franklin (1941, 360).

[22] Boerhaave's *A New Method of Chemistry* is listed in Christopher Wordsworth (1968, 79).

[23] Guerlac (1970, 177–178). Fahrenheit was an instrument maker, a friend of Boerhaave's, and a fellow of the Royal Society, who published papers on meteorological instruments in the *Philosophical Transactions*.

[24] Brook Taylor was a mathematician and fellow of the Royal Society, whose experiments were reported in the *Philosophical Transactions* for 1721; they are described in A. Wolf (1961, 1:189–190). Wilson (1851, 447).

[25] Cavendish (1921c, 327). Wilson (1851, 447). Black began his experiments the same way, by examining the thermometer. Guerlac (1970, 177–178).

where h is the temperature of the hot water, c is the temperture of the cold water, H is the weight of the hot water, C is the weight of the cold water, and m is the temperature of the mixture. He does not work with the equation but by rearranging the terms he writes $(m - c) : (h - c) :: H : (H + C)$, which is a proportion, the mathematical relation he always works with. Cavendish does not call it a law; instead, he says that "it seems natural to suppose," crossing that out, "it seems natural to imagine," and crossing that out, "it seems reasonable to imagine" that the equation correctly describes what happens; the object of his experiments is to find if the proportion "really" is correct. In words, his "experiments were made with an intent to see whether the excess of the heats of the hot water and the mixture above the cold water really bore that proportion [the sum of the weights of the hot and cold water to the weight of the hot water] to each other or not." (His verbal description is the inverse of the proportion written in symbols above.) He expects the proportion to be confirmed (because it is "reasonable," elsewhere because it is a "theory") "if the expansion of the mercury in the therm. is proportional to the increase of heat."[26] It is the instrument that he is investigating here.

He next experimented with a mixture of hot mercury and cold water, finding that the heating effect of mercury is equivalent to 31.35 times its weight of water, the standard substance. He then reversed the temperatures, mixing cold mercury with hot water, obtaining a water equivalent for mercury close to the first value. His measured heats of mixtures and the theoretically computed heats agreed to within a half degree, a realistic accuracy for experiments of this kind.[27] He continued his experiments with an improved apparatus consisting of a funnel tightly joined to a pan, with stirrers and thermometers inserted in both the funnel and the pan.

He carried out experiments on a variety of liquids and solids, taken in part from his shelves of chemical reagents: besides water and mercury, they were spirits of wine, oil of vitriol, solution of pearl ashes, sand, iron filings, shot, pounded glass, marble, charcoal, brimstone, coal, and spermaceti. He also estimated the specific heat of air using a different method, blowing cold air through a worm tube surrounded by hot water.[28] His results were contrary to what was expected, as he explained in "Experiments on Heat": "One would naturally imagine that if cold [mercury] or any other substance is added to hot water the heat of the mixture would be the same as if an equal quantity of water of the same degree of heat had been added; or, in other words, that all bodies heat and cool each other when mixed together equally in proportion to their weights"; his experiment on mercury and water showed "that this is very far from being the case."[29] From this statement, Wilson said, it was plain that Cavendish did not know about the experiments by Black and his pupils;[30] we are inclined to agree that he knew nothing specific about them at this time. His own experiments were original, and judging from the way he described and analyzed them, he clearly believed that his findings were new. "The true explanation of these phenomena seems to be that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees

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[26] Cavendish, "Experiments on Heat," Mss III(a). On p. 1 in the manuscript. In the equation and proportion, the parentheses are added.

[27] Cavendish Mss III(a), 9:48–56.

[28] Cavendish (1921c, 341–343). He used a worm tube again in his experiments on latent heat, finding the heat generated by condensing water vapor, and mentioning Black. Ibid., 346–347.

[29] Ibid., 332.

[30] Wilson (1851, 447).

by the thermometer than it does to raise other bodies by the same number of degrees."[31] With this statement, Cavendish had a theory of specific heats, which explained the unexpected outcome of mixing diverse substances.

He paused at this juncture in the flow of his experiments to carry out an extended investigation of one substance, spermaceti, and with it he changed subjects: "Concerning heat & cold produced by hardening & melting of spermaceti" is the heading of his first experiments on latent heats.[32] In the first experiment of this group, he poured melted spermaceti into cold water, hardening it. He calculated that the observed heat communicated to the water would have raised an amount of water equal in weight to the spermaceti by 93.32°. From experiments on the specific heat of spermaceti, he further calculated that if no heat had been generated in hardening, the spermaceti would have communicated 26° of heat to that same quantity of water. The difference of the two numbers gave him the contribution of heat from the change of state: the "heat gen. by hardening of sperm. is sufficient to communicate 67⅓ ° of heat to an equal weight of water." In the converse arrangement, mixing cold spermaceti with hot water, he found a value for the latent heat close to the first: the cold produced by melting spermaceti "is sufficient to cool a quantity of water equal to it in weight about 70 degrees."[33]

The place that spermaceti had in his researches is evident: it was one of the substances he used to establish a general law or rule of nature. What is unclear is the place it had in his understanding of heat. When he began his experiments, he would have known about the cold produced by evaporation and by melting ice, but we have no way of knowing if he had the idea of a general law of latent heats. It is conceivable that his experiments with spermaceti suggested the idea, in which case there is an element of discovery in his experiments. It is at least equally likely that he already had the idea and that he began with spermaceti for reasons of convenience: it melted at a modest temperature, it had physical qualities he was interested in, and it was a substance at hand. In favor of the second explanation are the substances that he chose to experiment with, which were the same ones that Irvine experimented with: in addition to spermaceti, they were beeswax, another soft substance, and tin and other metals. Against the explanation is Cavendish's failure to mention any experiments on these substances done by Irvine or anyone else.

Cavendish's experiments on latent heats established inductively a second law valid for all bodies, which he stated at the beginning of Part 2 of "Experiments on Heat": "As far as I can perceive it seems a constant rule in nature that all bodies in changing from a solid state to a fluid state or from a non elastic state to the state of an elastic fluid generate cold, & by the contrary change they generate heat." As in the case of specific heats, Cavendish had an explanation of latent heats: "The reason of this phenomenon seems to be that it requires a greater quantity of heat to make bodies shew the same heat by the thermometer when in a fluid than in a solid state, and when in and elastic state that in a non-elastic state."[34] With his rules of nature and physical explanations, Cavendish had a theory of both specific and latent heats.

Cavendish's explanation of the change state of a body might be mistaken for Irvine's. Irvine thought that latent heat depends on specific heat, and to go from one to the other, he

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[31] Cavendish (1921c, 340).

[32] Cavendish Mss III(a), 9:22, 27.

[33] Ibid., 32.

[34] Cavendish (1921c, 343).

introduced a third heat, the "total" heat of a body. He theorized that the specific heat, or "heat capacity," of a body measures the total heat in a body, the body acting as a container holding the heat. For example, because water has a larger measured heat capacity than ice, it takes more heat to fill its container than it does the same quantity of ice with its smaller container, when both are at the same (freezing or melting) temperature; that is, it takes more heat to maintain water at that temperature than it does ice, the additional heat being the measured latent heat.[35] Cavendish rejected Irvine's theory, but his wording in "Experiments on Heat" is compatible with it.[36]

He followed his statement of the law of latent heats with a discussion of experiments that supported it, beginning with experiments on boiling. Cullen had "sufficiently proved" that fluids generate cold when they evaporate at a temperature below the boiling point and are absorbed in the air. Cavendish treated the other "species of evaporation," boiling, which is independent of the air, finding that 982 degrees of cold are generated in the conversion of water to steam. His discussion of the generation of cold in the change from an inelastic to an elastic state ends with a brief "sketch of the other experiments," one of which was an attempt to find if cold is generated by dissolving alkaline substances in acids, releasing fixed air. This experiment was original; he gave no details, and his method could not have yielded accurate results, but the principle was sound.[37] He next discussed the cold generated in the change from a solid to a liquid state, beginning with the cold generated by melting snow in solutions of sea salt, pearl ashes, spirits of wine, and aqua fortis. He followed this with a discussion of the cold generated by melting spermaceti and beeswax and then by melting "simple metals," lead, bismuth, and tin, and "mixtures" of these metals. The latter, "alloys," differed from the simple metals in that they changed state over a range of temperatures rather than at a fixed temperature, analogous to spermaceti and beeswax. He briefly discussed the inverse change of state of these substances, from liquid to solid, generating heat.[38] Cavendish's long series of experiments on specific and latent heats ended here.

Wilson and others have suggested that Cavendish did not publish his experiments on heat because he did not want to enter into rivalry with Black.[39] That may be, but he published on factitious air even though Black said that he intended to do more work on the subject. The two cases differ in a way that may be relevant: Black published his original experiments on fixed air, whereas he published nothing on heat. Not fear of rivalry but eventual knowledge of Black's work is the more likely reason Cavendish did not publish his experiments on heat; after writing his paper he probably learned more about Black's lectures and realized that his own work was not new. Black's lectures were, in effect, a slow but sure publication, and

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[35] Thomas L. Hankins (1985, 76).

[36] Nowhere does Cavendish mention Irvine by name, but his manuscripts contain two short memoranda directed to principal points of Irvine's theory. One states a proposition that any heat that appears in bodies depends entirely on their heat capacities and changes in them. The proposition is expressed in the language of the material theory of heat, its only appearance in any of Cavendish's papers: heat is said to be "absorbed" by bodies and "united" to them. The second memorandum is an experimental "compleate proof" that the absolute heat in bodies is not proportional to their specific heat, as Irvine's theory requires. There is a second copy in Cavendish's hand of the second memorandum in Blagden's papers, suggesting Blagden as a possible reason he wrote out the proof. Henry Cavendish, "That All the Heat Which Appears in Bodies …"; "A Complete Proof that the Quantity of Heat …" Cavendish Mss Misc. Blagden Collection, Misc. Notes, Royal Society, 93.

[37] "On heat produced by sat. alkalies with acids," Cavendish Mss III(a), 9:82–83. Wilson (1851, 347). Berry (1960, 144).

[38] Henry Cavendish (1921d). This is a section of "Experiments on Heat."

[39] Wilson (1851, 446). McKie and Heatcote agree with Wilson (1935, 52).

a number of researchers in Britain worked with concepts of heat that Black communicated through his lectures. In addition, important work on specific and latent heats was carried out abroad, in particular by Wilcke in the 1770s. When Cavendish published on the freezing point of mercury in 1783, he invoked the rule of latent heat in a discussion of the freezing of water, giving neither an argument nor a citation for it but simply remarking that it was a "circumstance now pretty well known to philosophers." The "circumstance," he explained, was "that all, or almost all, bodies by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; and that cold is produced by the contrary process," wording taken from his paper "Experiments on Heat" based on his experiments from the 1760s.[40]

Another question is Cavendish's satisfaction with his experiments and their explanation. Part I of "Experiments on Heat," which deals with specific heats, is complete and apparently ready to be rewritten in fair copy. The experiments in the incomplete Part II, which deals with latent heats, move beyond heats involved in a change of state of bodies to heats involved in mixing interacting fluids and in chemically releasing fixed air, for which he did not have a "general rule in nature." The same happened with his experiments on electricity: he completed Part II of his work insofar as it was about experiments explained by the theory of Part I, but he had gone on to make experiments on electrical conduction, which his theory had not addressed. As with his experiments on electricity, he did not bring his experiments on heat to a natural conclusion, but on the contrary, he expanded them.

Finally, we need to consider the theoretical side of his experiments. His paper on heat ends with "Thoughts Concerning the Above Mentiond Phenomena," which reads: "There are several of the above mentiond experiments which at first seemd to me very difficult to reconcile with Newtons theory of heat, but on further consideration they seem by no means to be so. But to understand this you must read the following proposition."[41] The proposition is not given. Cavendish held two theories of heat, a mechanical theory which he called "Newton's theory" and the theory of specific and latent heats which he worked out at the time of his experimental researches. It is clear from the report of one of the experiments that he had both theories in mind. The change in temperature generated by a mixture of water and spirits of wine was caused either by the "commotion made by the particles of one uniting with those of the other" or by the "mixture of spts & water requiring a greater quantity of heat to make it raise the thermom to a given degree than the 2 liquours separately do."[42] The first explanation referred to the mechanical theory of heat, the second to the theory of specific and latent heats applied to mixtures of substances that have an affinity for one another. Cavendish almost certainly did not have a theoretical explanation for all of his heat experiments at the time. He did in the 1780s, but by then there was no point in publishing the experiments.

Cavendish developed a special theory for a specific change of state, evaporation and boiling of water, which he carefully drafted but did not publish.[43] The theory was an explanation of his recommendation on setting the upper fixed point of the thermometer, which he wrote out to show to Deluc, a member of his committee. Deluc returned the paper with a letter in French, thanking Cavendish "for the pains you have taken to introduce me to your

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[40] Henry Cavendish (1783b); in *Sci. Pap.* 2:145–160, on 150.

[41] Cavendish (1921c, 351). "Thoughts" is an unnumbered page in the manuscript.

[42] Cavendish Mss III(a), 9:39–40.

[43] Henry Cavendish (1921h).

theory which makes you favor the vapor of boiling water to the boiling water itself for fixing the upper point of the thermometer." He had read the paper "three times with much care," without finding in it "any reasons to abandon my own theory," which explained all of Cavendish's experiments. He did not argue the merits of their theories, he said, because he would then have to introduce material not just about boiling and the thermometer but also about the barometer and the hygrometer, and also because any discussion of matters "not susceptible to geometric demonstration" but only to probability can be endless. He limited his comments on Cavendish's paper to the constancy or variability of the temperature of the vapor of boiling water. He closed the letter saying that he would come to Cavendish's house the next day, no doubt to witness experiments and perhaps to participate in them.[44]

A preparation for Cavendish's theory of boiling was his paper on specific and latent heats; a section from "Experiments on Heat" reappears verbatim in "Theory of Boiling." We assume that by this time he had abandoned his intention to publish "Experiments on Heat," treating it as a resource. His theory of boiling brings together several major strands of his development as a natural philosopher. It pays us to look at it.

Around the time of Cavendish's theory, Cavallo wrote in his treatise on air that the explanation of evaporation was still unsettled. His own opinion was that evaporation is the absorption of water by air, facilitated by heat, but to the question of how air assists in evaporation, the "present knowledge of philosophy [...] does not afford a satisfactory answer." A good many hypotheses had been proposed, for example, capillary attraction and chemical attraction, but none was satisfactory. Cavallo thought that "a vast number of experiments is still requisite, in order not only to discover its [evaporation's] real cause, but also to ascertain its laws."[45]

Cavendish referred to evaporation and boiling as two "species" of evaporation. Earlier in the century, the first species of evaporation had been explained by a hydrostatic theory, which held that small quantities of water are expanded by heat and rise through the heavier surrounding water. Cavendish accepted the alternative explanation of Charles Le Roy's in 1755, according to which water is dissolved in air in the same way that salts are dissolved in water. This species of evaporation, Cavendish said, "is intirely owing to the action of the air."

The phenomena of the second species of boiling depend on four "principles," on which Cavendish based his theory. The first principle, which his father had demonstrated the "truth" of, is that if the water is in contact with steam or air in a closed vessel, it is immediately turned into steam once it is heated ever so little above what is required for steam. He called this heat the boiling point, which depends on the pressure on the water. The second principle is that if the water is not in contact with steam or air, it bears a considerably higher heat before it converts into steam, what Deluc called the heat of ebullition (from hissing to rolling boil). Cavendish said that Deluc had confirmed this experimentally. The third principle is that steam contained in closed vessels and not in contact with air is immediately converted back into water when it is cooled ever so slightly below the heat required to produce the steam. This was proved by Cavendish's father and also by the boiling point committee, and Cavendish performed an experiment of his own to put the "matter out of doubt." The fourth principle is that in the conversion of water to steam, a great quantity of heat is lost, and in the conversion of steam to water an equally great quantity of heat is

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[44] Jean André Deluc to Henry Cavendish, 19 Feb. 1777; in Jungnickel and McCormmach (1999, 546, 549).

[45] Cavallo (1781, 505–507).

acquired. For confirmation, Cavendish referred to his own experiments on latent heat and to similar experiments by Cullen, Black, and Deluc.[46] From the principles of the theory, Cavendish explained the "chief phenomena of boiling water." When water begins to boil, the lamina of water at the bottom of the vessel heats up until small bubbles of steam are formed by ebullition. The bubbles can never be hotter than the boiling point of water, but the water itself can be and generally it is. Just how much the temperature of the water exceeds the boiling point depends on a variety of factors such as the amount of air dissolved in the water and the rate of application of heat. The theory confirms the committee's opinion that steam is a more exact method than boiling water for setting the upper fixed point of the thermometer.[47]

To explain the difference between the temperature of boiling and of ebullition, Cavendish introduced a hypothesis. In developing other theories, Cavendish began with a hypothesis, but this time he began with a set of principles, which he did not regard as hypothetical, and he placed the hypothesis at the end. The hypothesis is that particles of water repel one another over a minute distance beyond which they attract one another, and that the repulsion but not the attraction increases with heat.[48]

"Theory of Boiling" is a compendium of Cavendish's scientific practices. It shows him as a natural philosopher who makes theories and hypotheses and performs experiments. He bases his theory in part on his father's experiments, a sign of the continuing importance of his father. The importance of the Royal Society is seen in his work with a committee called to consider the accuracy of the fixed boiling point on thermometers, in itself an inducement to develop a theory of boiling. He draws on his earlier experiments on latent heat, reflecting his practice of using results of experiments he has not published. His hypothesis relates heat to the forces of particles, an idea Newton and Boscovich have discussed extensively. With his theory, he takes up an unexpected behavior, the heating of water above its boiling point, showing that it is explained by the laws of the normal course of nature, and in this respect it is similar to other topics he takes up such as the electric shock of a fish and the freezing of the mercury in thermometers. He writes the paper for an intended reader from the Royal Society, and he does not publish it.

Cold

Extremely cold temperatures were reported from the frigid North. The natural historian Johann Georg Gmelin recorded -120° in Siberia. He said that such a temperature was scarcely believable "had not experiments, made with the greatest exactness, demonstrated the reality of it."[49] Commenting on these temperatures, William Watson said that however "extraordinary" Gmelin's observations were, they were "scarce to be doubted," since they were made with "all possible exactness" and agreed with readings made by others under his direction in different parts of Siberia.[50] Pyotr Simon Pallas reported - 70° there, noting that the mercury froze to the glass stem of his thermometerand that the mercury began to melt when the

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[46] Cavendish, "Theory of Boiling," 354–356.

[47] Ibid., 358–360.

[48] Ibid., 361–362.

[49] John Fothergill's extracts from Gmelin (1748, 260).

[50] John Fothergill (1748, 258–260). William Watson (1753a, 108–109).

thermometer stood at - 45°. Cavendish copied out the parts of Pallas's account of his travels in Siberia dealing with the freezing of mercury.[51]

In St. Petersburg in 1759, on a day when the temperature was -34°, a member of the Academy of Sciences J.A. Braun found that the temperature of a freezing mixture of nitric acid with snow sank below -350°. When he removed the thermometer from the mixture he saw that the mercury was immovable, and he put its freezing point roughly at a hundred degrees below zero.[52] He and his colleagues tested the solid mercury, finding that it could be hammered and drawn like any other metal. One of his colleagues Aepinus observed a change in the surface of the mercury in the thermometer, a sure indication of contraction upon freezing. Braun's experiments attracted attention in Europe; in the *Philosophical Transactions* for 1761, William Watson published an enthusiastic account of them and of this "intirely new" subject.[53] In the journal the previous year, Nicolas de Himsel said that the Petersburg experiments mostly agreed that mercury becomes solid when it drops to around -500°, but they did not "sufficiently agree as to deduce any thing certain about it."[54] In the same volume, Keane Fitzgerald observed that Himsel's own experiments on the freezing of mercury made the mercury thermometer unfit for measuring great cold.[55] There no longer could be any doubt that mercury, the substance once regarded as the essence of fluidity, could be solidified, but beyond that fact little was known for certain about the behavior of mercury at very low temperatures.

Cavendish made an extract of a paper with Braun's repetition of Fahrenheit's experiments: surprised when the mercury in the thermometer fell hundreds of degrees, and unable to arrive at a consistent freezing point of mercury, Braun said he was confident that it could not be at a "less cold than -346" degrees.[56] Braun's experiments were repeated by Thomas Hutchins, governor of Albany Fort at Hudson's Bay, using instruments and instructions sent to him by the Royal Society. In the winter of 1774–75, Hutchins froze mercury, and like Braun he found the experiments inconclusive on the freezing point. He could find no instant of freezing, and without changing its appearance, the mercury continued to fall to below -400°. He asked the Royal Society for more tubes of mercury capable of graduation to *1,000 degrees below zero.* He continued making experiments up to 1777–78, using freezing mixtures of nitrous (nitric) acid and snow, and comparing a spirit (alcohol) thermometer with a mercury thermometer. Although the temperature was cold enough to freeze mercury, the alcohol thermometer never fell lower than -46°, indicating that the freezing point of mercury was nowhere near as low as Braun supposed.[57]

The reason for Hutchins's findings was evident to two persons in Britain who had clarified to themselves the principles of latent heat, Black and Cavendish. In a letter in 1779 about Braun's and Hutchins's experiments, which was forwarded to Hutchins, Black said that frozen mercury could not record its own freezing temperature, and although he did not give his reason for this opinion, it doubtless included the contraction of mercury on freez-

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[51] "Account of Freezing of ☿ from Pallas Journey into Siberia," extract in Cavendish's hand, Cavendish Mss III(a), 15. Pyotr Simon Pallas (1771–1776).

[52] Wilson (1851, 456).

[53] William Watson (1761). A.W. Badcock (1960, 100).

[54] Nicolas de Himsel (1760, 673).

[55] Keane Fitzgerald (1760, 833).

[56] This extract in Cavendish's hand is an account of experiments by several Petersburg academicians following Braun's discovery; in English translation from the French by James Parsons (1760).

[57] Thomas Hutchins (1776). Berry (1960, 146).

ing. To get around the difficulty, he proposed immersing the thermometer bulb in a mercury bath. Hutchins informed the Royal Society of Black's proposal, which he made the basis of his next series of experiments. Unknown to Black, Cavendish had already proposed the same method to the president of the Royal Society Joseph Banks. Black did not publish on this subject, as usual, but this time Cavendish did.[58] In his paper on Hutchins's experiments, Cavendish explained the method: "If a glass of water, with a thermometer in it, is exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze till it is intirely congealed, and will then begin to sink again. In a like manner, if the thermometer is dipped into melted tin or lead, it will remain perfectly stationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it will again begin to descend; and there was no reason to doubt that the same thing would obtain in quicksilver."[59]

Cavendish drew up a list of experiments to be performed at Hudson's Bay on the freezing of mercury and on the change of volume of other fluids with temperature.[60] In 1781 the Royal Society sent thermometers for use in the experiments, and Cavendish sent an apparatus—a thermometer with the bulb and part of the stem enclosed in a narrow cylindrical cup for holding the mercury to be frozen—for determining the "precise degree of cold at which quicksilver freezes."[61] One day in December 1781, after taking a reading every twenty seconds for about an hour in weather colder than 20° below zero, Hutchins recorded that he "went away to warm myself," an indication of the rigors of the climate and the limits of endurance of the experimenter.[62] In the course of ten experiments on both natural and artificial cold in which he read three instruments—a mercury thermometer, an alcohol thermometer, and the apparatus—Hutchins determined the freezing point of mercury. His experiments were "very accurate," Cavendish told John Michell.[63] Hutchins said that his "excellent instructions" left him with "nothing to do but to follow them."[64] Cavendish, Blagden said, was the "real author and first mover of the whole business."[65]

Hutchins's paper appeared in the *Philosophical Transactions* for 1783, followed by a paper by Cavendish giving his "observations" on Hutchins's experiments. The experiments confirmed Cavendish's hypothesis, which was that the great sinking of mercury in thermometers in extreme cold is due to the contraction of mercury. If the earlier reports had been true, the intense cold produced by freezing mixtures would have been "really astonishing," but they were actually reports of the contraction of mercury. Submerged in freezing mixtures, Hutchins's thermometer fell to hundreds of degrees below zero, but the cold of the freezing mixture was never less than 46° below zero. The essential point was clearly

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[58] Joseph Black to Andrew Graham, 5 Oct. 1779, published by Thomas Hutchins, in "Experiments for Ascertaining the Point of Mercurial Congelation" (1783, 305–306). Black did not know that Cavendish had recommended a similar apparatus to Banks. Henry Cavendish (1783b).

[59] Cavendish (1783b, 146).

[60] There are several drafts of instructions in Cavendish's papers, most of them in Cavendish Mss III(a), 4 and 14. The first group is mainly concerned with Hutchins's experiments published in 1783, though it contains some subsequent instructions sent in 1784. The second group is concerned with the next series of experiments at Hudson's Bay Company, conducted by John McNabb, published in 1786 and 1788. In addition, there are unclassified papers on the Hudson's Bay experiments in the miscellany of Cavendish's manuscripts.

[61] Cavendish (1783b, 145, 148–149).

[62] Hutchins (1783, 317).

[63] Henry Cavendish to John Michell, 27 May 1783, draft; in Jungnickel and McCormmach (1999, 568).

[64] Hutchins (1783, 304).

[65] Charles Blagden (1783, 346).

and simply demonstrated. Because the thermometer in the container of mercury fell to -40°, where it stayed while the surrounding mercury was gradually freezing, the only possible conclusion was that mercury freezes at that temperature. Hutchins came to England and demonstrated his apparatus before Cavendish and Blagden at Cavendish's house in Hampstead.[66] Hutchins returned the thermometers to the Royal Society, where in the best practice of the time, in the presence of witnesses—in addition to Cavendish, they were Hutchins, Banks, Blagden, and Nairne, who made the apparatus—they were examined following the procedure recommended by the boiling point committee of 1777 (Fig. 15.1). Upon making corrections for the fixed point on Hutchins's thermometers, the adjusted freezing temperature of mercury was declared to be $-38\frac{2}{3}°$ or, in round numbers, -39°, in close agreement with the modern value, -38.87°. Hutchins probably did not freeze mercury solid, since the mercury in his thermometer did not fall as far as Braun's; from Braun's experiments, Cavendish concluded that upon freezing, mercury shrinks by almost 1/23 of its bulk, a figure close to modern measurements.[67]

The new understanding of mercury entered the scientific literature at once. In 1783, the year of Hutchins's and Cavendish's publications on mercury, there appeared an English translation of Bergman's treatise *Outlines of Mineralogy*, which had been published in Swedish the year before. Under the entry for mercury, Bergman wrote that it has been "erroneously ranked among the brittle metals, for at 654 degrees below zero it freezes, and then spreads under the hammer like lead. But as such an extreme degree of cold rarely happens unless artificially produced, we cease to wonder why it is always liquid or rather melted." The translator William Withering commented that recent experiments at Hudson's Bay seem to give the freezing point as 39° below zero, and he altered Bergman's "Table of Metals" accordingly: the "melting heat" of mercury now read, "-39 or -634" degrees Fahrenheit.[68] Cavendish's observations on the Hudson's Bay experiments put an end to credible reports of extravagant cold from the frozen parts of the Earth. Michell said to Cavendish that "indeed I think you are bound to find something else in it's stead, having robbed us of so excellent a measure of heat & cold, as the Quicksilver was supposed to be for so many degrees below - 39."[69] Experiments on the freezing of mercury combined several of Cavendish's interests: the work of the Royal Society, latent heats, climates of the Earth, and the workings of a principal instrument of quantitative science, the mercury thermometer.

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[66] Thomas Hutchins to Charles Blagden, n.d., "Monday Morning," Blagden Letters, Royal Society, H.59.

[67] Cavendish (1783b, 157).

[68] The disparity between the two numbers for the low reading, -654 and -634, is in the text. Torbern Bergman, *Outlines of Mineralogy*, trans. W. Withering (Birmingham, 1783), 71, 83.

[69] John Michell to Henry Cavendish, 2 July 1783; in Jungnickel and McCormmach (1999, 575).

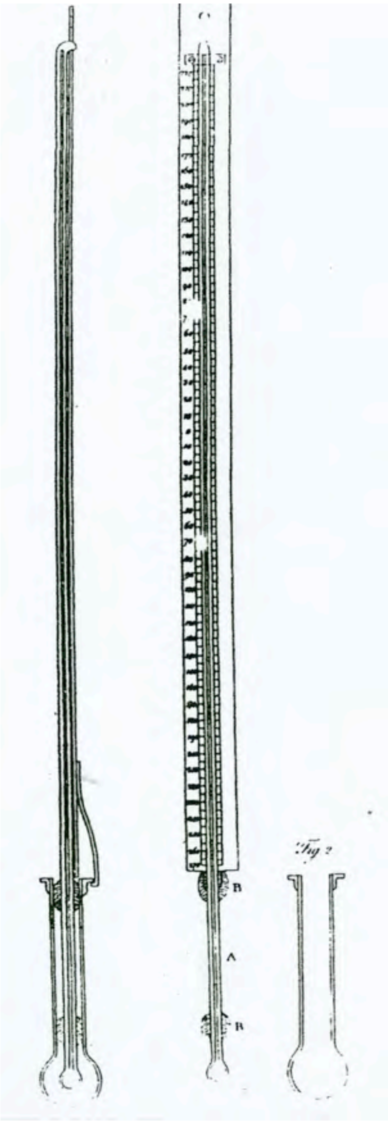

Figure 15.1: Thermometers for Extreme Cold. The stem and bulb of the middle thermometer extends below the scale. The figure on the left is a side view of the thermometer with the extended stem and bulb inserted into a cylinder holding mercury to be frozen. Thomas Hutchins (1783, *370).

Cavendish made experiments of his own too. Hutchins's first experiments were read at the Royal Society in the winter of 1775–76, and in January 1776 Cavendish performed experiments on artificial cold, using a mixture of snow and aqua fortis, recording a temperature of -25°F.[70] In February 1783, two months before Hutchins's paper on the freezing of mercury was read at the Royal Society, Cavendish froze mercury at his house in Hampstead. In an air temperature of 20°, and using a freezing solution of nitrous (nitric) acid, the mercury in his thermometer fell to -110°, part of the mercury being frozen. He then placed an alcohol thermometer in the freezing solution, obtaining a reading of about -45°.[71] In the same year he built an apparatus that cooled air by rarefying it mechanically.[72] In 1786 Blagden said that this way of producing cold was "lately much talked of, in consequence of experiments by Mr. Cavendish, Dr. Crawford, & I believe some other gentlemen."[73]

Interested in knowing the greatest cold that could be produced by a freezing mixture of snow and various chemical solutions, and in finding the cause of the cold produced by freezing mixtures, Cavendish arranged for more experiments at Hudson's Bay. The experimenter this time was John McNab, master at Henly House, a station on the Albany River 150 miles from Fort Albany. Like Hutchins, McNab earned praise from Cavendish for his "utmost attention and accuracy." Carried out in weather that reached -50°, using mainly alcohol thermometers, McNab's experiments developed "degrees of cold greatly superior to any before known" as well as insight into the "remarkable" way nitrous (nitric) and vitriolic (sulfuric) acids freeze. With a mixture of snow and dilute vitriolic acid, McNab measured a temperature of $-78\frac{1}{2}°$. Braun claimed that a thermometer filled with spirit of wine (alcohol prepared by distilling wine) sank to -148°, but McNab found that spirit of wine thermometers could not nearly approach that degree of cold. Cavendish published his account of McNab's experiments in 1786.[74] Because in that paper the freezing points corresponding to different strengths of the acids "were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction," Cavendish asked McNab to carry out more experiments "to ascertain the truth" of his earlier result. These experiments became the subject of his last paper on heat, published in 1788.[75]

We see in Cavendish's researches on cold qualities of his work we have come to expect. In connection with McNab's experiments, he published a table of specific gravities of nitric and sulfuric acids corresponding to a range of strengths at a temperature of 60°, which agree with modern theoretical values to the second decimal place. Thorpe considered the table "a striking exemplification of the care, patience and manipulative skill which he spent upon all quantitative determinations."[76]

We end this discussion with further conclusions Cavendish drew from McNab's experiments. The acids could be cooled far below their freezing points without freezing, but once they froze their temperatures rose to the freezing points. In one kind of freezing, nitric

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[70] Entry for 22 Jan. 1776, Cavendish Mss III(a), 9:94–96.

[71] Blagden was with Cavendish. The following day he recorded in his diary that Cavendish had frozen mercury, and he mentioned it in his history of the freezing of mercury. 27 Feb. 1783, Charles Blagden Diary, Royal Society, 1. Blagden (1783, 359–360).

[72] Henry Cavendish (1921b).

[73] Charles Blagden to Erasmus Darwin, 14 Sep. 1786, draft, Blagden Letters, Royal Society 7:34. Charles Blagden to Mrs. Grey, 30 Jan. 1788, ibid. 7:111.

[74] Henry Cavendish (1786, 195, 197–198, 210).

[75] Henry Cavendish (1788a).

[76] Thorpe (1921, 59–60).

and sulfuric acids froze as a whole; in another kind, the watery part of the acids separated out and froze. The acids had complex freezing points, which depended on their strengths. The freezing point of dilute nitric acid was not as low as it was when the acid was made more dilute and also when the acid was not dilute; there was a definite strength of the acid at which it froze with less degree of cold than when the strength was stronger or weaker, which Cavendish called "a point of easiest freezing." Drawing upon Newton's method of interpolation, Cavendish determined that the point of easiest freezing was -2.4° Fahrenheit, and that the strength at which nitric acid froze with the least degree of cold was .418 according to his marble scale (he stated the strengths of his acids in terms of the weights of marble dissolved by a unit weight of the acids).[77] Sulfuric acid had an even more complicated pattern of freezing. Cavendish found that it had not only a strength of easiest freezing, as James Keir had recently shown in a paper communicated by Cavendish to the Royal Society, but also at greater strengths it had another point of flexure beyond which the freezing point increased again. Cavendish's biographers, the chemists Wilson and Berry, were impressed by his two papers on the freezing of acids. Wilson singled out Cavendish's implicit use of the laws of constant and reciprocal proportions in constructing a table of sulfuric acid strengths. Berry said that the accuracy of Cavendish's elaborate investigations of freezing points of acids were confirmed a century later, and that his findings "are of theoretical interest, and of fundamental importance for the recognition of the various hydrates of nitric and sulphuric acids."[78]

Cavendish's first paper on McNab's experiments cited Lorenz Crell's *Chemische Annalen* and *Neue Entdeckungen in der Chemie.* It was read to the Royal Society in February 1786, and in an earlier chapter of this book we learned that in January 1786 Cavendish (through Blagden) told Crell that he wanted to subscribe to his journal. We see that he had use for it, a possible reason for his impatience at the delays in receiving it.[79]

All told, Cavendish published three papers on heat (or cold, which belongs to the same subject), all three presenting experiments done by others, by Hutchins and McNab. He carried out experiments on freezing mixtures and on the freezing of mercury at his house, but he was content to limit his public contribution to planning, commenting on, and drawing inferences from experiments done by observers working in a cold climate, in close association with the Royal Society.

Heat

The first sentence of Blagden's contribution to the family obituary of Cavendish reads: Cavendish made himself master of "every part of Sir Isaac Newton's philosophy." Cavendish's researches in heat support this observation. He studied latent heats, but he did not use Black's word *latent*, as he explained in a footnote to his paper on the freezing point of mercury in 1783. The word "relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; and as I think Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is gener-

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[77] Cavendish (1788a, 218).

[78] Wilson (1851, 461–465). Berry (1960, 150–154, quote on 154). Cavendish (1788a, 223).

[79] Cavendish (1786, 211).

ated."[80] In his paper "Experiments on Air" the following year, in connection with a recent paper in which Watt spoke of latent heat, Cavendish said that he avoided Watt's "form of speaking" because he thought it "more likely that there is no such thing as elementary heat" and because it could "lead to false ideas."[81] The passage on Watt in 1784 and the footnote on Black the year before are Cavendish's only published statements on the nature of heat. The manuscripts he left at his death are found to contain two references to Newton's theory of heat. One appears in a corollary to a theorem in a paper on the theory of motion, which begins, "Heat most likely is the vibrating of the particles of which bodies are composed." The other, quoted earlier, appears in his experimental paper on latent and specific heats: some of his experiments appeared to conflict with "Newton's theory of heat," but they could be reconciled by a proposition. The paper ends without the proposition, about which he may have had second thoughts. Until recently, these references, two published and two unpublished, were the only known explicit statements by Cavendish on the nature of heat. Since heat was one of his major fields of research, and since it had connections with other fields such as electricity, magnetism, pneumatic chemistry, pneumatics, and meteorology, what was missing from his scientific papers was a fully developed theory of heat comparable to his theory of electricity. He had indeed worked out such a theory, only it had been separated from his scientific manuscripts. It came to light in 1969, when a direct descendent of Cavendish's principal heir put it up for sale.

The new manuscript was inside a folded sheet labeled in Cavendish's hand "Heat." It is a theoretical paper, which he definitely wrote for publication. The first draft he referred to as the "foul copy," to which he appended a number of pages of additions and alterations, and the revised second draft is a nearly fair copy with some crossings out and certain paragraphs marked for rearrangement for the next writing, which he apparently did not carry out. He referred to the "text," to which he supplied an apparatus of footnotes, and he called the whole a "paper." The paper is a mathematical, mechanical theory of heat complete with the principle of conservation of energy and applications to the principal branches of physical science.[82]

The idea of heat as vibratory motion had received a number of formulations by Cavendish's time. To the question of what it is that moves, a variety of answers had been proposed: the ordinary particles of bodies, the air and acid sulfur in bodies, the subtle ether, and the fluid of fire. Newton's authority was invoked in support of more than one of them, but to Cavendish, Newton's theory meant the vibrations of the ordinary particles of bodies. Many examples of heat in the queries of Newton's *Opticks* agree with his answer to the question.[83]

By the time Cavendish worked out his Newtonian theory of heat, a good many arguments had been marshalled against the view of heat as motion, and we should know what he

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[80] Cavendish (1783b, 150–151). The discussion of heat in this section draws on Russell McCormmach (1988). We acknowledge permission to use the material: University of Chicago Press, copyright 1988 by the History of Science Society, Inc., all rights reserved.

[81] Cavendish (1784b, 173–174).

[82] The revised draft of "Heat" consists of forty-three pages of text and notes, one page of diagrams with an accompanying page of explanation, and one page of additions and alterations. The original manuscript of both drafts of "Heat" is located in the Public Archives of Canada in Ottawa. The manuscript is published in Russell McCormmach (2004, 153–193). The page numbers of "Heat" in the following footnotes refer to the manuscript unless otherwise specified.

[83] Robert E. Schofield (1970, 13, 37, 48, 77–78, 84–85, 139, 160, 179, 183). Isaac Newton (1952, 348–349, 375–406).

was up against. One of the arguments was that cold is produced by mixing sal ammoniac and water, whereas in the mixing, particles are set in motion, which should register as heat rather than as cold. A related criticism was the apparent failure of liquids and gases to generate heat upon being agitated.[84] The specific heats of bodies were found not to be proportional to their densities, as the motion theory was understood to require. More objections to the motion theory were pointed out by the Jacksonian Professor of Natural Philosophy at Cambridge Isaac Milner in lectures he delivered in 1784–88. A basic objection, Milner said, was that vibrations of particles had not been proven to exist. Another objection was that heat was not observed to be proportional to motion. Another was that when oil and grease eliminate friction, heat seems to be eliminated too, although motion is communicated to the particles. Heat was observed to pass slowly through bodies, as a liquid might, rather than rapidly, as motion does. Heat should not spread at all, since the momentum of a system of particles is unaffected by their mutual actions and collisions. The passage of heat across the vacuum should be impossible, since there are no intervening particles to be set in vibration. The liberation of heat during the solidification of a liquid cannot be explained by motion, nor can the generation of cold during evaporation. The objections were serious, but Milner had answers, for as it happened he was a believer in the motion theory and a critic of the opposing material theories of heat. "The arguments against this [motion] Theory have of late Years been esteemed so numerous and weighty that it has almost been given up by Philosophers," but it has been given up "a little too precipitately," and Milner wished that "somebody else had endeavoured to shew the truth" of it by contrasting it with the fashionable material fluid theories of heat.[85] Cavendish set about to do that.

The difficulties of the motion theory could be seen as one general difficulty: new ideas for the theory had not kept pace with the rapid development of the experimental foundation of heat in the late eighteenth century, while the fluid theory of heat had developed together with the experiments.[86] Heat was one of a number of hypothetical fluids that had come to characterize British speculative natural philosophy from about the middle of the eighteenth century.[87] They were usually taken to be imponderable, indestructible, subtle, and closely associated with fire, and their particles were usually assumed to repel one another and to be attracted to the particles of ordinary substances. The fluid of heat had one quantitative property, the conserved quantity of heat, which was sufficient to account for the equilibrium of heat in bodies in contact and for most of the phenomena of heat.[88] The theory was readily grasped, plausible, and predictive, and like the motion theory it was considered to be Newtonian. Black's former students William Cleghorn and, if tentatively, Adair Crawford accepted it. Black himself was cautious on the subject of the nature of heat, but he said that Cleghorn's theory was the most likely to be true of any he knew.[89]

Investigators rarely needed to declare themselves for one or the other theory of heat, as they could carry out their experiments very well without doing so. A case in point is Lavoisier and Laplace's joint paper on calorimetry in 1783. Lavoisier almost certainly held the material theory of heat; what Laplace thought is uncertain, and he was later to hold the

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[84] William Irvine (1805, 21–23).

[85] L.J.M. Coleby (1954, 242–252).

[86] Robert Fox (1971, 19, 22–23).

[87] J.L. Heilbron (1993a, 5–33. Schofield (1970, 157–190); P.M. Heimann (1981, 67–73). Arthur Quinn (1982, 127); McKie and Heathcote (1958). Fox (1971, 19–20, 22, 25).

[88] Schofield (1970, 185).

[89] McKie and Heathcote (1935, 28).

material theory, but in any case it was he who described the motion theory in their joint paper. Side-by-side with the motion theory, the authors presented the material theory, without deciding between the two.

Black and his followers had the common difficulty of being unable to form an idea of the internal motions of bodies that could account for the phenomena of heat, but Black's main objection to the motion theory was that none of its supporters had shown how to apply it to the entirety of the phenomena of heat,[90] a complaint which could not have been made about the material theory of heat after Cleghorn's theory in 1779. With his paper "Heat," Cavendish supplied what was missing from the side of the motion theory, Newton's theory together with comprehensive supporting evidence drawn from many parts of physical science.

Before proceeding further, we should consider a question readers might have. Because Cavendish successfully developed a theory of electricity based upon a fluid distinct from ordinary matter, it seems that a fluid of heat would have appealed to him as the starting point of a theory of heat. In the case of fluids the analogy between electricity and heat is obvious, but the analogy does not depend upon a fluid of heat, applying as well to heat as motion. In whatever way electricity and heat are conceived, their theories require two quantitative concepts, quantity and intensity: charge and potential in the first case, quantity of heat and temperature in the second. Also in both subjects, varieties of matter have defining characteristics: specific inductive capacities and conductivities in the first case, specific and latent heats (and thermal conductivities, but Cavendish did not investigate these) in the second.

We need to clarify a point in mechanics at the start. G.W. Leibniz, Newton's German contemporary and co-inventor of the calculus, made a distinction between "living force," or vis viva, and "dead force," or vis mortua. Dead force is the force that strives to generate motion; it is potential vis viva. Living force, commonly called the "force of moving bodies," is the force of a body in motion, which communicates motion in collisions. Vis viva obeys a law of conservation, its most useful property. The measure of vis viva is the product of the mass of a body and the square of its velocity, which readers may recognize as our kinetic energy only lacking the factor $\frac{1}{2}$; in practice vis viva usually appeared with $\frac{1}{2}$.

In Newtonian mechanics the measure of moving bodies is momentum, the product of the mass of a body and its velocity, not the square of its velocity. Unlike vis viva momentum is a directional quantity; like vis viva it obeys a conservation law. There was a long-standing controversy between British supporters of Newton's momentum and Continental supporters of Leibniz's vis viva over the proper measure of the force of moving bodies. Beginning in the middle of the eighteenth century, some writers on mechanics decided that both parties were right, that the dispute was over words, the two parties meaning different things by the words "force of moving bodies." A paper found among Cavendish's manuscripts shows that he agreed with them.[91]

Not long before Cavendish entered Cambridge, the Scottish mathematician Colin Maclaurin published an account of Newton's calculus, *A Treatise of Fluxions*. Recognizing that many able foreign mathematicians followed Leibniz, Maclaurin showed that Newton's and Leibniz's forms of mechanics gave the same results, though he had a preference: any solutions to mechanical problems obtained with the use of vis viva could be obtained from

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[90] Schofield (1970, 186–187). Irvine (1805, 22)

[91] P.M. Heimann and J.E. McGuire (1970, 225–227).

Newton's universal principles, proven by the "most simple and uncontested experiments."[92] Cavendish knew Maclaurin's book, citing it in his plan of a treatise on mechanics. By founding Newton's theory of heat on vis viva, the invention of Newton's archrival Leibniz, Cavendish was not breaking faith with Newton but making good use of a common possession of different formulations of mechanics.

Vis viva cannot disappear without giving rise to a comparable effect, an equal quantity of potential motion. This property recommended vis viva for treating a range of mechanical problems, but it encountered difficulties in the case of collisions between bodies. From experience it was known that collisions are never perfectly elastic, implying that vis viva is lost, but it cannot really be lost. The missing vis viva was regarded as continuing on in hidden forms such as the compression of bodies or the motion of parts internal to bodies. Leibniz proposed the latter explanation, but he did not identify the hidden vis viva with heat, even though in the seventeenth century heat was commonly believed to be the internal motion of bodies. It would seem that the conceptual problems of treating heat as a quantity made this identification difficult.[93]

In an early unpublished paper, labeled by someone else "Remarks on the Theory of Motion," Cavendish discussed the usefulness of vis viva as a "way of computing the force of bodies in motion."[94] He said that vis viva was usually reserved for solving problems of machines used for "mechanical" purposes. The engineer John Smeaton wrote a paper about vis viva for fellow engineers, which he gave to Cavendish for comment.[95] For most questions arising in "philosophical inquiries," Cavendish wrote in "Remarks," the usual and most convenient way of computing the forces was Newton's momentum, but vis viva had a place. Instead of "vis viva," he spoke of the "mechanical momentum"[96] of bodies in motion; by this terminology, referring to both ways of computing the force of moving bodies as "momentum," he drew on his understanding that the use of one or the other was a practical choice, not one of fundamentals. What was fundamental is force, not the way it is measured. By assuming that forces are centrally acting, and that no force is lost by friction and inelastic collisions, Cavendish derived a general law of conservation of mechanical momentum and "additional momenta," or potential mechanical momenta. (Fig. 15.2). He extended the conservation law to encompass lost force by identifying heat with the mechanical momentum of the invisible vibrations of the particles of the bodies. He acknowledged that there were phenomena—the heats involved in fermentation, dissolution, and burning—that he did not know how to explain by his theory of heat. In "Remarks," he did not introduce the concepts of specific and latent heats, leading us to conclude that it was written before his experiments on latent and specific heats, placing it not later than the early 1760s. "Heat," which covers the same ground as "Remarks" but goes beyond, was written much later.

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[92] Colin Maclaurin (1742, 2:427, 433–434).

[93] Erwin N. Hiebert (1962, 80–93). P.M. Heimann (1977).

[94] Henry Cavendish (1921f); definition of "mechanical momentum" on 416.

[95] The paper Smeaton gave Cavendish to comment on was probably "New Fundamental Experiments upon the Collision of Bodies" (1782). J.G. Playfair (1822, 1:lxxxiii).

[96] Bernoulli first and then Smeaton called it "mechanic force." Newton treated the square of the velocity in the *Principia*, but he did not name it. W.H. Wollaston (1806, 16).

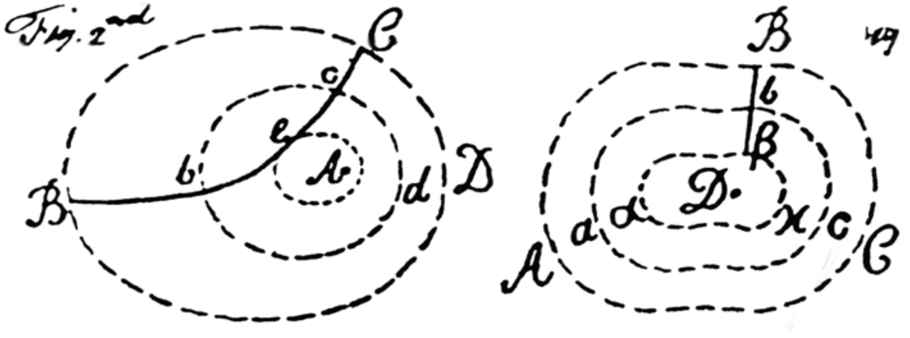

Figure 15.2: Forces. The dashed lines represent forces of attraction and repulsion of constant intensity centered on bodies or particles of matter, A and D. BC in the figure on the left and Bβ in the figure on the right are paths of a second attracting and repelling body or particle. With the aid of these diagrams and a proposition from Newton's *Principia*, Cavendish derived a general law of conservation of the sum of "real" and "additional" "mechanical momenta" (our kinetic and potential energies). It has been pointed out that Cavendish was struggling here with our concept of equipotential curves. "Remarks on the Theory of Motion," Cavendish Mss VI(b), 7: Plate 3; *Sci. Pap.* 2:430.

"Heat" carries no date. It was certainly written after 1783, for that year Cavendish rejected Black's term "latent heat" because it implied the material theory of heat, using instead expressions such as "heat is generated." In "Heat," he systematically used "latent heat," not because his opinion of the fluid theory of heat had changed, but because the expression had become standard, and he was writing to be read. In "Heat," he used another term he had avoided earlier, "vis viva" instead of "mechanical momentum," no doubt for the same reason. In the manuscript, he cited Priestley's history of optics, but that book appeared early, in 1772, and he cited the names, but not the publications, of Scheele and Horace Bénédict de Saussure for their work on radiant heat. Cavendish showed his familiarity with Scheele's only book, which appeared in English translation in 1780. His mention of Saussure no doubt referred to the second volume of his travels in the Alps, which came out in 1786.[97] The absence of citations to work done in the 1790s may be taken as indirect evidence for an upper limit for the dating of this manuscript.[98] Largely for these reasons, we place "Heat" in the late 1780s.

Cavendish begins "Heat" with a purely mechanical investigation, laying out propositions that parallel those in "Remarks," only developed more systematically and thoroughly. He defines vis viva as the mechanical effect of a body in motion, both "visible" and "in-

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[97] Cavendish, "Heat," 23. Experiments on "heat rays" and light using polished metal and glass, discussed in Carl Wilhelm Scheele (1780, 72–74, 92–98). Saussure's account of experiments that he and M.A. Pictet carried out on the reflection of "obscure heat" emitted by hot, but not red hot, bodies, discussed in Saussure (1786, 354–355).

[98] For example, Pierre Prevost's experiments on heat rays and Benjamin Thompson's on the mechanical production of heat in the 1790s would have been relevant to Cavendish's argument, as perhaps would Herschel's experiments on radiant heat from 1800.

visible.” The visible is the vis viva of the center of mass of a body undergoing progressive motion or of the body undergoing rotation or both; the invisible vis viva is that of the particles of the body moving among themselves; and the total vis viva of the body is the sum of both. He further divides the invisible vis viva into two parts: one is “active,” the other inactive, with the potential for becoming active. His symbol s, standing for the active, is the actual vis viva of all the particles constituting the body; his symbol S stands for one half the sum of the vis viva that each particle would acquire by the attraction or repulsion of every other particle in falling from infinity to its actual position within the body. Upon assuming that the attractions and repulsions between the particles are always the same at the same separations and different at different separations, he derives the law of conservation of vis viva, active and inactive; the quantity $s - S$ cannot change as a result of the motions of the particles among one another. Strictly speaking, the two quantities S and s individually change constantly because of the motion of the particles among one another, but because the number of particles is “inconceivably great” and because any increase in one quantity is matched by a decrease in the other, S and s do not sensibly change, unless there is an external cause.

Next Cavendish identifies the quantities occurring in the propositions about vis viva in mechanics with the quantities occurring in heat. According to his “hypothesis,” “heat consists in the internal motion of the particles of which bodies are composed,” which he regards as vibrations, the particles being bound close to their place by attracting and repelling forces. He identifies the “active heat” of the body with the actual vis viva, s, and the “latent heat” with the potential vis viva, $- S$, and consequently the “total heat” with $s -S$, the conserved quantity. “Sensible heat” is what Cavendish calls the heat of a body as given by a thermometer, and it is related to the active and latent heats through the constitution of the body. With these definitions, Cavendish has a technical vocabulary for the theory of heat.[99]

The first test of Cavendish’s theory is its ability to account for the phenomena of heat itself. When two isolated and unequally heated bodies are brought into contact, one gives up heat and the other acquires it until the sensible heat of each is the same, the condition of equilibrium. In the exchange the total heat given up must be the same as the total heat received, but just how this heat is divided between the active and latent heats in the two bodies depends on the weights of the bodies and on “some function either of the size of their particles or of any other quality in them,” for example, the frequency of vibration of the particles.[100] The distinctions, based on experimental knowledge, between sensible, total, active, and latent heats enable Cavendish to explain the phenomena of specific heat, the subject of his earliest experiments in heat.

Once he secures the vibrational theory of heat within its own field, heat, Cavendish applies it to other parts of physical science, first to optics. “There can be no doubt,” he says, that light is a body consisting of extremely small particles emitted from luminous bodies with extremely high velocity. The particles of light are bound to their natural places in a body by the forces of attraction and repulsion of the particles of the body, and when the particles of the body are set in brisk vibration, the particles of light are moved into positions where they experience violent repulsion, flying off from the body as free light. When these particles

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[99] Cavendish did not formally introduce a term for “specific heat”; he spoke of the “capacities for heat” of bodies. “Specific heat” first appeared in print in a publication by J.H. Magellan in 1780. Both terms are still used. Cavendish, “Heat”, 11–12, 24, 41.

[100] Ibid., 14–16.

are reflected from a body, they are not reflected by a single particle or by a few particles of that body but by a great quantity of its matter, so that according to mechanical principles no perceptible vis viva is communicated to the body. The same explanation applies to the case of refracted light. But where light is absorbed, its particles are reflected back and forth within the body until their velocity is no greater than that of the particles of the body, "so that their vis viva will be equally distributed between the body & them," and the absorbing body will thereby acquire sensible heat.[101] Light falling on a body that does not have a mirror surface heats the body.

A plate of glass is heated more than a plate of polished metal when it is exposed to a fire or the Sun, but since the metal absorbs more light than the glass, according to Cavendish's theory it ought to be heated more than the glass. To resolve this apparent conflict with the theory, Cavendish refers to recent experiments by Scheele and Saussure on the newly discovered "heat rays." Cavendish assumes that heat rays, like light rays with which they comingle in various proportions, are material particles emitted by hot bodies, and although their velocity is unknown, they too must communicate vis viva. What is important is the way heat rays differ from light rays. Not only do they not excite the sensation of vision, but they are absorbed by glass and are efficiently reflected by polished metals, exactly the reverse of the behavior of light. It is the heat rays, then, and not the accompanying light rays, that warm the glass preferentially. These new invisible rays enable Cavendish to reconcile the facts with his theory of heat; if the rays did not exist, the theory would fail.[102]

Heat can be produced mechanically, for example, by friction and hammering. Because a violent force is required to produce heat, the particles of the heated body must be displaced or even torn away at its surface, and that in turn alters the latent heat of the body, giving rise to sensible heat. The same displacement and tearing away of particles are responsible for the loss of elasticity in the collision of two bodies or in the bending of a body. Cavendish's analysis of the forces of particles is more problematic here than in some other applications of the theory, but on the basic point Cavendish is "certain": if any visible vis viva is lost by the rubbing, striking, or bending of bodies, these bodies must acquire an "augmentation of total heat equivalent thereto."[103]

Electricity is the field that Cavendish had developed with the greatest thoroughness, and although he had examined the effect of heat on conduction, he had not examined the converse, the heat generated by conduction.[104] Now, he says, he is going to "argue upon the principles laid down in my paper concerning the cause of electricity" to derive a formula for the vis viva of electric fluid discharged by a Leiden jar through a wire. Because of their extreme lightness, he doubts that the particles of the electric fluid could communicate sufficient vis viva to the particles of the wire to account for the violent heat of the wire. His explanation is that the electric discharge displaces the particles of the wire, greatly diminishing its latent heat. The heat caused by electric discharge is consistent with the theory, though, Cavendish says, "it is an effect which I should not have expected."[105]

As the final application of his theory, Cavendish discusses the expansion and change of state of bodies with heat. When a body is heated, the increase in the vibrations of its

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[101] Ibid., 18–20, 25–26.

[102] Ibid., 23–24.

[103] Ibid., 26–31, on 31.

[104] Cavendish (1879, 324).

[105] Cavendish, "Heat," 41.

particles alters their mutual attractions and repulsions, which in turn alter the size of the body. When the vibrations become great enough, the attractions and repulsions of the particles vary sufficiently for the body to change its form and properties entirely. This is what happens in evaporation and in melting: the increased vibrations of the particles diminish their adhesion, making bodies more fluid. By the same reasoning, Cavendish explains why chemical decomposition and combination are promoted by heat.[106]

Cavendish had a question he could not answer about the change in size of bodies. He said that bodies always expand with heat, but why this is so, and "why the size of the body is never diminished thereby, I do not pretend to explain; but there seems no reason why it may not be so."[107] He seems to have been unaware that below 4°C, water expands with cooling when he wrote this. However, a few years earlier, he was aware of it. In a proposal of experiments on the freezing of mercury for the Royal Society, he wrote: "Water takes up most room when coold almost to freezing than it does at 40° of heat & conseq. a thermometer filled with water stands higher at the former degree of heat then at the latter." He was concerned that the liquids including mercury used in the thermometers planned for the experiments might behave like water as they approach the degree at which they freeze, which would "make a puzzle in the exper.," but the remainder of the experiment would show whether or not this is the case.[108] Around this time, the singular property of water became well known. In 1797 Count Rumford spelled out the beneficial effects for all life on earth of this "miraculous" exception to the "general law of nature" that all bodies contract upon cooling.[109]

The "Conclusion" of "Heat" begins: "It has been shown therefore by as strict reasoning as can be expected in subjects not purely mathematical, that if heat consists in the vibrations of the particles of bodies, the effects will be strikingly analogous, & as far as our experiments yet go, in no case contradictory to the phenomena." That is, Cavendish shows that the hypothesis is *sufficient* to explain the phenomena, establishing one half of the argument. To establish its *necessity*, the other half, he calls implicitly on the principle of causality. "To put the matter in a stronger light," he says, it "seems certain that the action of such rays of light as are absorbed by a body must produce a motion & vibration of its particles; so that it seems certain that the particles of bodies must actually be in motion." With that, Cavendish lets rest the case for Newton's theory of heat. He has implicitly answered the main objections to it by showing that the hypothetical vibrations can account not only for the heat of friction, for example, for which the motion theory would seem to be well suited, but also for heats accompanying changes of state for which the material theory of heat is well-suited. In each application of the theory he has suggested possible motions and configurations of particles, so that unlike earlier versions of the motion theory his could not be faulted for lack of clear ideas about the mechanisms. Confirmed by its consequences, his hypothesis meets the test of a good hypothesis.

Cavendish reserves judgment on the opposing theory of heat to the end of the "Conclusion." Given the evidence for the existence of internal vibrations, he writes, there is no

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[106] Ibid., 38–39.

[107] Cavendish, "Heat," in McCormmach (2004, 101, 231).

[108] Henry Cavendish, "List of some Exper. …," Cavendish Mss III(a), 4(a).

[109] Benjamin Thompson, Count Rumford, "On the Propagation of Heat in Fluids" (1870–1875, 1:239–400, on 308–33). Tiberius Cavallo referred his readers to Rumford's discussion of this exceptional property of water (1803, 3:35–37).

reason to "have recourse to the hypothesis of a fluid, which nothing proves the existence of." He continues:

> The various hypotheses which have been formed for explaining the phenomena of heat by a fluid seem to show that none of them are very satisfactory; & though it does not seem impossible that a fluid might exist endued with such properties as to produce the effects of heat; yet any hypothesis of such kind must be of that unprecise nature, as not to admit of being reduced to strict reasoning, so as to suffer one to examine whether it will really explain the phenomena or whether it will not rather be attended with numberless inconsistencies & absurdities. So that though it might be natural for philosophers to adopt such an hypothesis when no better offerd itself; yet when a theory has been proposed by Sr I[saac] N[ewton] which, as may be shewn by strict reasoning, must produce effects strongly analogous to those observed to take place, & which seems no ways inconsistent with any, there can no longer be any reason for adhering to the former hypothesis.[110]

Cavendish does not criticize the material theory of heat for any specific failures; he criticizes it only for the kind of theory it is, prone to inconsistency, absurdity, and imprecision. To prefer it to Newton's theory is unreasonable. Three times in the "Conclusion" Cavendish uses the expression "strict reasoning." He used it before in his electrical theory: the method he proposed to follow there was first to lay down the hypothesis and then "to examine by strict mathematical reasoning, or at least, as strict reasoning as the nature of the subject will admit up, what consequences will flow from thence."[111] He uses the same method and the same wording in "Heat." Strict reasoning leads to correct conclusions.

We move on to experiments. To calculate the vis viva of light, Cavendish introduces Michell's experiment to "ascertain the momentum of light," widely regarded then as proof that light consists of streaming material particles.[112] Inside a box with a window for admitting direct sunlight, a thin sheet of copper was fastened to one end of a horizontal wire and balanced by a weight at the other end. Rays of the Sun were concentrated and directed by a concave mirror so that they struck the copper plate perpendicularly, resulting in a rotation of the wire.[113] From the observed speed of rotation and other details of the experiment, and from the assumption that the light was perfectly reflected from the copper, Cavendish calculates the vis viva of the sunlight falling each second on $1\frac{1}{2}$ square feet of surface. He translates this result into its mechanical effect: the rate of vis viva of sunlight falling on that surface exceeds the work done by two horses, that is, it exceeds two horsepower.[114] If the same quantity of light were absorbed by a fixed body instead of being reflected, the body would gain an equivalent quantity of heat and its temperature would rise. From this line of reasoning, Cavendish proposes two experiments, indicating that he intends to follow up his theory with an experiment to determine the mechanical equivalent of heat.

The first experiment is to "expose thermometers whose bulbs are coated with various dark & equally dark colord substances alternately to the ⊙ & shade & see whether they

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[110] Cavendish, "Heat," 42.

[111] Henry Cavendish, *PT*: 61 (1771):584.

[112] G.N. Cantor (1983, 57).

[113] Joseph Priestley (1767, 1:387–389). Cantor (1983, 57). S.G. Brush and C.W.F. Everett (1969).

[114] Cavendish, "Heat," 22.

receive the same increase of heat in the same time.” This experiment follows up what Cavendish calls “a necessary consequence of this theory.” He gives no details, but experiments at the time on the heat of sunlight suggest what he had in mind. The Cambridge professor of chemistry blackened the ball of a thermometer and exposed it to direct sunlight, finding that it registered a higher temperature than when the bulb was not blackened. He expressed the wish that others would repeat the experiment with different colored paints on the thermometer to determine the ability of colors to receive and retain heat.[115] The Royal Society’s Bakerian lecturer took up this suggestion, trying a variety of colors to see if the absorption of heat followed the progression of prismatic colors or some other law. He also exposed a blackened thermometer alternately to the Sun and shade, concluding that every degree of light was accompanied by a proportionate degree of heat.[116] In or around the year of Cavendish’s theory, 1787, George Fordyce described an experiment to see if sunlight falling on blackened surfaces of different substances heated them equally. He was interested in a general question, whether or not the same cause of heat always produces in the same body the same quantity of heat; for example, “whether a chemical attraction taking place between equal quantities of two substances shall always produce an equal quantity of heat.” He came to the question by observing reverberatory furnaces, wondering if by burning the same quantity of fuel the same quantity of heat would be produced. His experiment was indecisive, but he concluded that heat cannot be material, that it is a quality that might or might not be motion.[117] In the experiment Cavendish proposed, on the basis of his theory, the heating effect of rays from the Sun falling on equally dark surfaces presumably should be the same, since the rays would be completely absorbed, their vis viva registering as heat.

The second experiment is brought up in two places, the first in the preliminary sketch of the paper, where Cavendish speaks of a “calculation”: “Calculation of vis viva of ⊙s rays & Do required to commun. given quant. heat.” In the foul copy of the paper, the experiment is described: “Exper“. to determine the vis viva necessary to give a given increase [of] sensible heat to a given body by alternately exposing a thermometer to the ⊙ & shading it.”[118] The plan of the experiment seems to be this. Cavendish would calculate the vis viva per second of sunlight striking the surface of a blackened thermometer from the measurements in Michell’s experiment with the light-mill. He would read the change in temperature directly from the thermometer exposed to the Sun’s rays for a given time; this would be proportional to a quantity of heat equivalent to the vis viva of the light absorbed by the blackened thermometer. He would determine this quantity of heat from the change in temperature and the measurable heat capacity of the thermometer. The proportionality of the calculated vis viva and the measured quantity of heat would give a numerical value for the mechanical equivalent of heat. Because the light-mill was misunderstood, the value would have been wrong, but that is beside the point here. The expression “mechanical equivalent of heat” is our term, not Cavendish’s; we note the anachronism, but the meaning is the same, and as we have seen, elsewhere in “Heat” he uses the word “equivalent” in this connection.

In light of Cavendish’s reputation for anticipating results arrived at only much later by others, we might expect him to have calculated a value for the mechanical equivalent of heat. The value is implicit in his theory, and he apparently had it in mind in the experiment just

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[115] Richard Watson (1773).

[116] Tiberius Cavallo (1780, 591–594).

[117] George Fordyce (1787).

[118] Cavendish, preliminary sketch and foul copy, “Heat,” in McCormmach (2004, 153, 162).

described. Moreover he had in hand the concepts and units for expressing it: his measure of vis viva is mechanical work, the lifting of weights through a height, our foot-pound, and his measure of the quantity of heat is the same as ours, the heat required to raise the temperature of a unit weight of water 1° by the thermometer, our Brtish thermal unit. A determination of the mechanical equivalent would have made his hypothesis quantitatively complete. A parallel is the hollow-globe experiment, which by establishing the law of electric force made the hypothesis of his electrical theory quantitatively complete. This time Cavendish did not live up to our expectation, and we may ask why.

There is no record that Cavendish performed the experiment with the thermometer. He may have had reservations about Michell's experiment, which he depended on for calculating the vis viva of sunlight. When Michell performed the experiment, the concentrated rays of the Sun generated a great deal of heat, sufficient to melt the copper vane and disable the apparatus. This may have suggested to Cavendish that the heat of the air on the sunlit side of the vane, not the momentum of light, caused the arm to rotate. In any case that is the explanation of Michell's experiment, as explained by Abraham Bennett in 1792.[119] There were alternatives to Michell's experiment. In the next century James Prescott Joule gave the simplest and most persuasive demonstration of the mechanical equivalent of heat using a paddlewheel powered by descending weights.[120] Cavendish might have considered and rejected such an experiment for the reason Thomas Young gave in his text on natural philosophy: fluids cannot acquire any sensible increase in heat from internal friction, one of the standard arguments against the motion theory, mentioned above by Milner.[121] Like the heat of the emission of light, the heat of internal friction was thought to be too small to measure. Cavendish proposed an experiment on the production of heat by friction, "exper. whether friction is as much diminished by oil & grease as the heat is," but he said too little to know what he planned.

A value for the mechanical equivalent of heat would have joined a small number of useful physical constants such as the velocity of light and the acceleration of gravity, though as an equivalence it was a different kind of constant. It probably would not have had the importance to Cavendish as it does to us, a possible reason he did not pursue it further. Because he did not express physical relations as equations between terms with physical units, conversion factors and other physical constants did not come up as a matter of course as they do in modern physics; we discuss this point later in connection with the universal gravitational constant G. That he did not place particular importance on the mechanical equivalent of heat in its own right is further suggested by the full description of the experiment: he proposed to determine the vis viva needed to increase the temperature of a body a given amount "& thereby to give a guess at the velocity with which the particles of a body vibrate supposing that the total heat of a body heated to 1000° is double its heat at 0°."[122] From his statement of the problem, we deduce that the average velocity of the particles of the body in meters per second is $\sqrt{4000J}$, where J is the mechanical equivalent of heat using the Fahrenheit scale for temperature. Inserting today's value for J, and converting meters to inches, Cavendish's unit, the average velocity of the particles of the body at 1000° is about 6800 inches per second. Cavendish would have found J from Michell's experiment and his own experiment

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[119] Abraham Bennett (1792, 87–88).

[120] Heintz Otto Sibum (1995, 73–74, 104–105).

[121] Young (1807, 1:655).

[122] Cavendish, "Heat"; in McCormmach (2004, 162).

exposing a blackened thermometer to the Sun, discussed above. Cavendish's estimate of the velocity of particles points to his interest in the physical reality described by Newton's theory of heat.

As to the immediate stimulus for writing the paper, Cavendish said nothing. In 1783 he received Lavoisier and Laplace's paper on calorimetry, which he read with critical interest.[123] Unlike the usual statements of the motion theory of heat, which did little more than assert the identity of heat and motion, Laplace's presentation was mechanically precise: "Heat is the vis viva resulting from the imperceptible motions of the constituent particles of a body." He pointed out that just as in the material theory of heat, in which the quantity of fluid is conserved, in the motion theory there is also a conserved quantity, vis viva. By an appeal to the law of conservation of vis viva, he explained how heat is communicated from one body to another: when two bodies of unequal temperatures are brought into contact, the vis viva of the warmer body diminishes while that of the cooler body increases until the temperatures are equalized, at which time the vis viva exchanged in each direction is identical. This is the same insight as Cavendish recorded in his early "Remarks on the Theory of Motion." In 1783 Cavendish and Laplace were both thinking about the motion theory of heat. In May of that year Cavendish's paper on the freezing point of mercury with its assertion of Newton's theory of heat was read to the Royal Society, and next month Lavoisier and Laplace's paper was read to the Royal Academy of Sciences. In Laplace's statement of the motion theory of heat, Cavendish read a reflection of his own reasoning, a possible stimulus for him later to return to the theory and improve it.

In 1785 Fordyce published an experimental paper in the *Philosophical Transactions* demonstrating a loss of weight by ice upon melting. Because the ice apparently lost weight as it gained heat, Fordyce speculated that heat might be a body possessing absolute levity, though he was inclined to believe that heat is a completely general quality like attraction, only its opposite.[124] If Fordyce's experiments were proven right, Blagden told Laplace, they would bring about an "extraordinary revolution in our ideas."[125] That was recognized by Benjamin Thompson, who in 1787 repeated Fordyce's experiments, convincing himself that heat could not be a material substance.[126] Cavendish had earlier witnessed experiments like Fordyce's, and he was kept informed on pertinent researches in Paris.[127] When Fordyce's experiment on ice was announced, Cavendish had just published his experiments on air, which included experiments disproving Warltire's contention that heat has weight. We doubt that Fordyce's paper on heat or any other theoretical or experimental paper on heat around 1787 was the occasion for Cavendish to write "Heat"; if it had been, he would have mentioned it. Nor, we believe, was the occasion any new work of his own. The central idea of "Heat", the identification of heat with vis viva, had occurred to him long before, at the time he wrote "Remarks," and he had performed the relevant experiments on specific and latent heats in the 1760s.

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[123] Antoine Laurent Lavoisier and Pierre Simon Laplace (1982, 4–6). Henry Guerlac (1976, 244–248). Charles Blagden to A.L. Lavoisier, draft, 15 Sep. 1783, Blagden Letterbook, Yale.

[124] George Fordyce (1785, 364); Coleby (1954, 245).

[125] Charles Blagden to Pierre Simon Laplace, 5 Apr. 1785, draft, Blagden Letterbook, Yale; Charles Blagden to Lorenz Crell, 28 Apr. 1785, draft, ibid.

[126] Sanborn C. Brown (1979, 219–220).

[127] John Roebuck (1776). These experiments, witnessed by Cavendish among others, showed an increase of weight in iron and silver upon cooling, a result in agreement with Fordyce's later experiment. Charles Blagden to Henry Cavendish, n.d., [1785]; in Jungnickel and McCormmach (1999, 608–609).

There are, however, several circumstances that may have affected his decision. The first is a widening interest in heat: several books published around 1780 called attention to the problem of the cause of heat, and an unusual number of papers on heat appeared in the *Philosophical Transactions* in 1787–88. The second circumstance is Cavendish's involvement in experimental heat in 1783–88. He carried out experiments of his own—on freezing mercury, freezing mixtures, and cold produced by expanding air[128]—and he devoted a good deal of attention to experiments on heat carried out by others at Hudson's Bay under his direction. These varied experiments might have been a stimulus, since they were about change of state and the thermal effect of mixing acids with water, phenomena which he did not address in his first discussion of Newton's theory of heat in "Remarks." The third circumstance is his work in chemistry. He distinguished his explanation of the production of water from Watt's by their different ideas on the nature of heat. In 1786, in a book on the latest advances in heat, light, and pneumatic chemistry, the Irish physician and chemist Bryan Higgins said that he did not need to justify his preference for the material view of heat because Cavendish, Black, and other distinguished natural philosophers "have accepted it."[129] Higgins admired Cavendish for his "precision in conducting experiments," to whom "moden Philosophy [...] owed more [...] than to any other man now living, except Dr. Franklin, deeming him truly worthy of [...] the immortal Newton,"[130] but he was almost certainly mistaken about Cavendish's view of heat. If Cavendish read his book, he would have realized how incompletely he had informed the scientific world, a conceivable motivation for working up a paper with the intention of publishing it. The fourth circumstance is a widespread skepticism about the motion theory of heat. We mentioned numerous arguments against it. From around 1780, the fluid theory of heat came to be increasingly adopted, as notable supporters of the motion theory of heat abandoned it: Magellan in 1780, Cavallo in 1781, Macquer in 1784, and Fourcoy in 1786.[131] Other supporters of the motion theory of heat were seen to waver. An example is William Nicholson, who in his treatise on natural philosophy in 1782 wrote that the view of heat as the vibration of particles was "scarcely hypothetical," and that to postulate a fluid of heat was tantamount to multiplying causes in violation of the rules of scientific reasoning; moreover, such a fluid demanded scarcely credible, "amazing" properties. Eight years later, in his treatise on chemistry, Nicholson left undecided the nature of heat, calling it a "great question" deserving the attention of natural philosophers.[132] By the time of Cavendish's theory, the material theory of heat had acquired a large following, and by the end of the century the material theory was all but universally accepted in Britain. The arguments about heat were usually carried on among followers of the material theory themselves rather than between them and upholders of the motion theory.[133] In 1804 John Leslie, a former student of Black's, said that there were still some adherents of the motion theory of heat, but they were badly misguided. There were "insurmountable objections" to that theory; in addition to its being "vague and undefined," a "shapeless hypothesis," "merely nugatory,"

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[128] 27 Feb. 1783, Charles Blagden Diary, Royal Society. Charles Blagden to Erasmus Darwin , 14 Sept. 1786, draft, Blagden Letters, Royal Society 7:34. Henry Cavendish (1921b).

[129] Bryan Higgins (1786, 301–302).

[130] Joseph Priestley (1775, 16).

[131] Fox (1971, 23, 28).

[132] William Nicholson (1781, 1:134; 1790, 6).

[133] Fox (1971, 19–20, 23, 104–105).

it “explains nothing.”[134] Cavendish responded to the trend in thinking about heat with an intended publication of a fully up-to-date version of the Newonian theory of heat.

The fifth circumstance is the state of natural philosophy at the time. The understanding of the physical world that had guided Cavendish’s researches for twenty years was under attack or ignored. The elements of the new chemistry listed caloric, and pneumatic chemistry was acquiring a caloric theory, according to which particles of gases are surrounded by a repellent fiery matter. The ether and the imponderable fluids were widely thought to have provided a foundation for natural philosophy. Cavendish never mentioned the ether; as we have seen, he denied that heat is material; he believed that light has weight; and he never referred to magnetic fluids. He accepted that electricity is a fluid distinct from ordinary matter, but his electrical theory was ignored by his British colleagues and was all but unknown abroad.[135] He never referred to phlogiston as imponderable or as having negative weight or as incapable of being isolated and studied in its own right. With “Heat,” Cavendish demonstrated that a principal direction of Newton’s natural philosophy was capable of accommodating recent experimental advances.

The final circumstance is the abundant practical applications of heat at the time. In 1785–87, as we will see, Cavendish and Blagden made journeys to various parts of Britain, visiting industrial works wherever they went, making close observations of blast furnaces and steam engines. “Heat” contains no discussions of such applications, yet by repeated exposure to examples of the conversion of heat into mechanical work and of chemical reactions generating heat in industrial furnaces, Cavendish’s thoughts would have been directed to the subject of “Heat.”

If Cavendish had carried out his original intention, he would have submitted his paper to the Royal Society and a slightly abbreviated version of it would have been read at a meeting. It would have been read in its entirety by the papers committee. Very few purely theoretical papers were published in the *Philosophical Transactions*, but Cavendish’s electrical theory was, and his theory of heat would have been too. It was mathematical, and there were few mathematical natural philosophers in Britain, but a few were enough. Whatever their opinion on the nature of heat, knowledgeable readers would have recognized “Heat” as a well-constructed argument directed to a worthy question, the cause of heat. They would have acknowledged that it conformed to widely held objectives in natural philosophy. It was exact, potentially quantitative, and accessible to the instruments of experimental physics; it proceeded from the laws of nature and experimental facts, and it announced a new law of nature, the conservation of energy, which applied to every physical process, establishing connections within and between the parts of natural philosophy. The hypothesis laid down a cause of heat phenomena, vis viva, the force of moving bodies, and the theory developed its consequences throughout natural philosophy; it embraced most of the important facts of heat, and it was in an acceptable meaning of the term Newtonian. With “Heat,” the theory of heat looked to join a select company of theories, gravitational astronomy and mechanics. So why did Cavendish not publish it?

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[134] John Leslie (1804, 140–141).

[135] Blagden, upon delivering to Cavendish a gift of René-Just Haüy’s new treatise on electricity and magnetism, which contained an electrical hypothesis similar to Cavendish’s, observed that the author seemed unaware of Cavendish’s paper of 1771: Charles Blagden to Claude Louis Berthollet, 11 Sep. 1787, draft, Blagden Letters, Royal Society 7:69.

That question is asked about Cavendish's other work too, but in the case of "Heat," the question is unavoidable, for unlike many of his researches, he intended this one for publication from the start. Perhaps Cavendish did not want to enter into rivalry, usually the first guess. This can be ruled out, since in his lifetime no similar work was published. Experiments on the mechanical equivalent of heat began to appear only in the 1840s, and publications on the mechanical theory of heat only in the 1850s. After a brief discussion by Daniel Bernoulli in the early eighteenth century, the next publication on the kinetic theory of gases came out after Cavendish had been dead for six years, and it did not identify heat with vis viva but with momentum. Because Newton's theory of heat was out of favor, Cavendish might have wanted to avoid the criticism that was certain to follow. However, he had allies. His colleague Thomas Young said in 1807 that the "most sober reasoners of the present" subscribe to the vibration theory of heat.[136] Perhaps Cavendish had unanswered questions. Where he discussed the heat of electrical discharge and the latent heat of the wire, he noted, "This must be examined."[137] But he had questions of the same sort when he began, as we know from preliminary notes he made for his paper. Nothing suggests that he found any disagreement with experiment. The mathematical development of his theory of heat fell short of that of his electrical theory, and explanations of phenomena were largely qualitative, but he knew that at the beginning too, and in his published paper on electricity he applied his theory to phenomena only qualitatively. The theory of heat did not obviously point to a new class of phenomena in the way the theory of electricity did, but it laid the foundation for the next stage of the science of heat. With reference to Cavendish's unpublished papers, Blagden said that "it is to be supposed that he afterwards discovered some weakness or imperfection in them."[138] General as it is, it is the best explanation we are likely to get.

The Natural Philosopher

Cavendish had mastered Newton's science, but he needed more than Newton gave him to make "Newton's theory of heat," and important as Leibniz's vis viva was, that did not give it to him either. Rather Cavendish drew on these sources and on his and others' experiments on heat, and by strict reasoning he brought them together to make the theory he presented in "Heat."

The persuasiveness of "Heat" derives from its coherence, comprehensiveness, and exactness, which includes mathematical reasoning where it applies. In one after another branch of natural philosophy, Cavendish demonstrates that Newton's theory does "really explain" the phenomena of heat. "Heat" is a continuous argument for the hypothesis that heat consists of the invisible vibrations of bodies; it is a study in *understanding*.[139]

There are various ways of showing why Cavendish is seen as a natural philosopher, and "Heat" is one of them. "Philosophy," the natural philosopher and geologist James Hutton wrote, is the aim of science. Although natural philosophy cannot advance one step without experiment, unless experiment is guided by philosophy it can produce only endless collec-

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[136] Young (1807, 1:656). In addition to Young, they included Humphry Davy, Benjamin Thompson, and Cavendish. Schofield (1970, 290–295).

[137] Cavendish, "Heat"; in McCormmach (2004, 190).

[138] Blagden, in the family obituary of Henry Cavendish.

[139] "Heat" disproves Yukitoshi Matsuo's assertion of Cavendish's "failure to unify a variety of heat phenomena in terms of dynamics and his subsequent abandonment of a systematic consideration of them" (1975, 93–94).

tions of facts, and that, Hutton said, is not philosophy. "The disposing of one fact, that is, the putting it into its proper place in science or the general order of one's knowledge, is doing more for natural philosophy, than a thousand experiments made without that order of connection or relation which is to inform the understanding." William Enfield, an instructor at Warrington Academy, identified natural philosophy with the ordering of scientific facts within general truths. Honor is bestowed on those who enhance the public store of experimental facts, and "one who proceeds thus far, is an experimentalist; but he alone, who, by examining the nature and absorbing the relations of facts, arrives at general truths, is a philosopher." The natural philosopher Hugh Hamilton wrote, "It is the business of natural Philosophy to reduce as many Phaenomena as may be to some general well-known Cause."[140] In "Heat," Cavendish reduces the phenomena of heat to a cause, vibrating particles together with their vis viva and the law of conservation of energy, and he works out the theory of heat within a general order of understanding, mechanics. He shows connections between phenomena belonging to all parts of natural philosophy, arriving at "general truths." Earlier we saw that in electricity, he made connections between the principal phenomena of that field. He met the criteria of a natural philosopher.

In "Heat" Cavendish tells how he thinks the physical universe is constituted. The totality of the material world and its activity arise from attractive and repulsive forces, the all-embracing general truth. Cavendish brings the perspective of the natural philosopher to bear on his discussion of the heat of friction and hammering:

> According to father Boscovich & Mr Michell matter does not consist of solid impenetrable particles as commonly supposed, but only of certain degrees of attraction & repulsion directed toward central points. They also suppose that the action of 2 of the central points on each other alternately varies from repulsion to attraction numberless times as the distance increases. There is the utmost reason to think that both of these suppositions are true; & they serve to account for many phenomena of nature which would otherwise be inexplicable. But even if it is otherwise, & if it must be admitted that there are solid impenetrable particles, still there seems sufficient reason to think that those particles do not touch each other, but are kept from ever coming in contact by their repulsive force.[141]

Matter likely is nothing other than centers of force. Cavendish thinks that Boscovich and Michell are probably right about matter, but it would change nothing in the argument if Newton, who believed in solid impenetrable particles, is right, for in either case the force of repulsion keeps particles from touching and losing vis viva. The idea of alternating attractive and repulsive forces entered explicitly in several researches of Cavendish's,[142] but only in "Heat" did he say where it came from.

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[140] Hutton (1794, 36). Hugh Hamilton (1766, 36).

[141] Cavendish, "Heat," in McCormmach (2004, 187–188).

[142] He introduced it in his theory of boiling. Cavendish, "Theory of Boiling," 361. He used it to reconcile his theory of electricity with experiments. Maxwell in Cavendish (1879, 174–175). He analyzed the error of a magnetic dipping needle by assuming that the axis of the needle and the plane on which it rolls are prevented from actually touching by a repulsive force. Henry Cavendish, "On the Different Construction of Dipping Needles," Cavendish Mss IX, 40:12–14. In "Heat," he used the idea to resolve difficulties with the heat of friction in his theory of heat, and he used it to derive the general law of conservation of energy, which applies to forces and heat wherever they appear in natural philosophy.

Boscovich published his idea of forces in his *Theoria philosophiae naturalis* in 1758.[143] Michell arrived independently at a similar idea, which his friend Priestley published in his history of optics in 1772.[144] There was a British tradition paralleling Boscovich's idea,[145] which may have been more important, though Boscovich developed the concept of central points interacting through central forces in greatest detail. In Boscovich's theory of natural philosophy, at close separations central points experience infinite repulsion; at large separations they experience gravitational attraction; and in between they experience attractions and repulsions responsible for cohesion, vaporization, and a variety of other chemical and physical phenomena. He represents his universal "law of forces" by a continuous curve: above the axis the force is repulsive, below the axis it is attractive, and places where it passes between repulsion and attraction mark the limits of cohesion. When disturbed, central points vibrate around these places, and the vibrations continue indefinitely until the central points are again disturbed. The area between the curve and the axis is proportional to vis viva, the measure of the action of the force across a distance. In a general way, Boscovich's law, by accounting for combustion, dissolution, and fermentation, and by implying perpetual vibrations of particles and the conservation of vis viva, supports Cavendish's theory of heat.[146]

Bodies act on bodies across a distance. Blagden recorded in his diary that Cavendish "argued that one had no right to say that matter could not act where it was not: one knew nothing about it but from experience, & experience rather led to believe that it might."[147] The explanation of this is forces acting at a distance. The physical universe is constituted of such forces, the basic, irreducible concept of natural philosophy. The entire human experience of nature testifies to the existence and ubiquity of forces, making a case for Newton's view that the main task of natural philosophy is to determine the forces of nature. As we saw in the chapter on chemistry, the exact description of the forces responsible for the phenomena of heat was beyond the capability of natural philosophy in Cavendish's time. They act over short distances only, and no "universal synthesis of short-range forces" had been established.[148] This did not imply, however, that nothing could be learned from such forces. Newton had shown in his derivation of the sine law of refraction for individual rays of light that it is possible to determine rigorously some results of importance without knowing "what kind of Force" is acting, assuming only very general properties of the force.[149] In his derivation of the conservation law, Cavendish did exactly this, assuming only that the force with which particles attract and repel each other "is every where the same at the same distance however different at different distances."[150] When the conservation law was derived again in the next century from the same general idea of forces, it expressed the prevailing belief

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[143] Cavendish would have read about Boscovich's force in his *Theoria*. Cavendish and Michell met Boscovich on his tour of England, both dining with him at the Royal Society Club on 5 June 1760, and Cavendish with him again on 26 June 1760: Minute Book of the Royal Society Club, Royal Society, 4.

[144] Priestley (1772c, 1:309–311, 392–393, 786–791).

[145] Cantor (1983, 71–72); Schofield (1970, 237–238); P.M. Heimann and J.E. McGuire (1971, 233–306).

[146] It makes no difference here that Boscovich believed in the matter of fire; Roger Joseph Boscovich (1966, 22–23, 43, 73, 76–96). Boscovich did not have a conservation law and he generally regarded vis viva as having little significance. Thomas L. Hankins (1965, 294), and on Boscovich, 291–297; on Michell and Boscovich, Schofield (1970, 36–49).

[147] 22 Nov. 1804, Charles Blagden Diary, Royal Society 4:284.

[148] Cantor (1983, 87).

[149] Newton (1952, 82).

[150] Cavendish, "Heat," in McCormmach (2004, 179).

that forces are not destroyed, only converted to other forces, inspiring investigations that would redraw the map of the physical sciences. Cavendish was onto a good idea.

In the introduction to this book, we give our reasons for preferring to speak of Cavendish as a "natural" philosopher rather than as a "Newtonian" philosopher, but that is not to deny that he was also a Newtonian of a certain persuasion. We return to this point even as we call attention to his differences with Newton and to sources other than Newton for his understanding of forces. To characterize fully Cavendish's Newtonianism would be to repeat much of what we have said about him from his education at Cambridge onwards. His researches in heat contain his most telling statement on the subject. "When a theory has been proposed by Sr I[saac] N[ewton]," Cavendish wrote, and it is in agreement with experience, there is no reason to accept an alternative theory. To no other authority did he give an endorsement like this. In "Heat" he did not weigh the evidence for the competing theories of heat, Newton's and the material, but developed only the former and rejected the latter. If there is the suggestion of a doctrinaire element in Cavendish's thinking, it remains that the theory of heat as motion would be vindicated in the next century.

Another aspect of Cavendish's Newtonianism placed him not in the vanguard of but in opposition to the next development in science. In America, Francis Hopkinson observed that when he viewed a lamp through a silk handkerchief and moved the handkerchief before his eyes, he saw dark bars which did not move. Hopkinson took his "optical Problem" to the astronomer David Rittenhouse, who performed experiments with a square of parallel hairs, observing that the lines seen through it varied in strength and color. Doubts about Rittenhouse's experiments were expressed at a meeting of the Royal Society. "Lord Cavendish," Hopkinson wrote to Thomas Jefferson, performed the experiment and "declared it was truly stated." Cavendish had a good opinion of Rittenhouse, being the first to sign the certificate recommending him for membership in the Royal Society. What Rittenhouse had constructed was a diffraction grating, which would be used to measure the wavelength of light. No doubt Cavendish had an explanation for Rittenhouse's experiment, probably agreeing with Rittenhouse's own, which was that it was an instance of the inflection of light by bodies, as described by Newton and explained by the particle theory of light. With hindsight, it would seem that Rittenhouse's experiment was a missed opportunity for Cavendish, but then other people missed it too, and there was another explanation for it.[151] Cavendish continued to hold to the particle theory of light after Thomas Young introduced the wave theory of light in 1800: in or after 1804 Cavendish calculated the gravitational bending of light passing near the surface of a body such as the limb of a star or the edge of a hair.[152]

As Young understood him, it was "Newton's opinion, that heat consists in a minute vibratory motion of the particles of bodies, and that this motion is communicated through an apparent vacuum by the undulations of an elastic medium, which is also concerned in the phenomena of light."[153] Young's understanding pointed to the physics of the ether, the origin of unified views of nature in the nineteenth century. Cavendish's understanding of Newton's opinion did not include an ether, or if it did he never mentioned it. So far as we know, he held to the view that the phenomena of nature have a uniform cause in attractive and repulsive,

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[151] Francis Hopkinson to Thomas Jefferson, 14 Apr. 1787; in Boyd (1955, 288–290). Brooke Hindle (1964, 276–277). John C. Greene (1984, 158–160). 6 Nov. 1794, Certificates, Royal Society 5.

[152] *Sci. Pap.* 2:437. The calculation is undated, but an inspection of the watermark on the paper shows that it could not have been earlier than 1804.

[153] Thomas Young (1802, 149).

centrally acting forces. This view, together with mechanical theorems about the measure of the force of moving bodies, vis viva, permitted him to display a connectedness between the several major domains of phenomena constituting the broad field of natural philosophy.

Workplace

Cavendish was able to develop a comprehensive theory of heat because of his exhaustive experimental study of heat, as described earlier in this chapter. To judge from the laboratory record of his experiments on heat, and that of his experiments in other fields, he spent as much time in the laboratory as he did in his study. For a few laboratories of the time, there exist drawings. We do not have one of Cavendish's, but we have the next best, sketches he made of various apparatus, which give the reader an idea of what he would have seen if he had entered his workplace. Or what would have greeted her: John Davy recalled that a lady of rank—he thought she was the duchess of Gordon—upon visiting Cavendish at Clapham expressed surprise at seeing a long row of utensils, which turned out to be objects used in the crystallization of saline solutions.[154] For most of Cavendish's experiments, his laboratory was inside his house. Since Cavendish's laboratory had to be versatile, we include his drawings of apparatus for several fields.

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[154] Wilson (1851, 178–179).

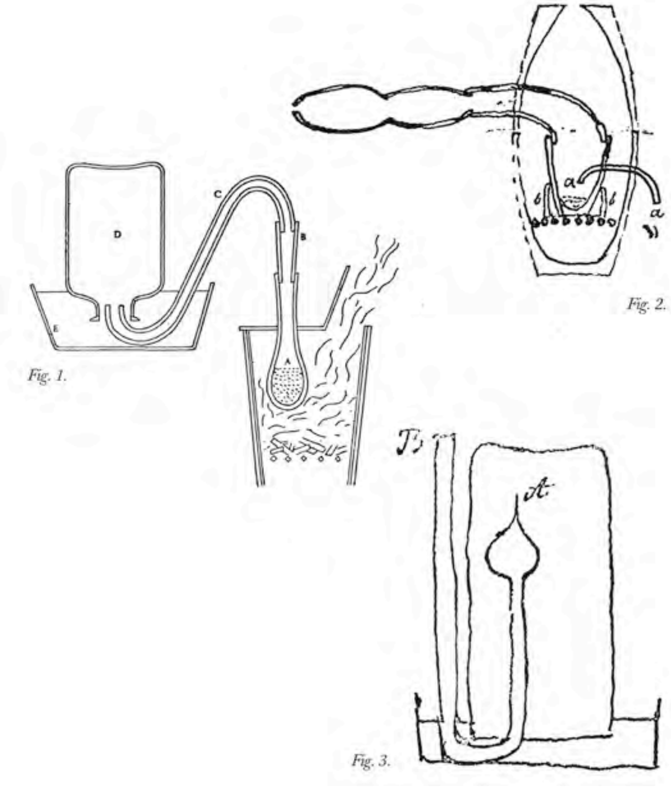

Fig. 1.

Fig. 2.

Fig. 3.

Figure 15.3: Laboratory Apparatus. Figure 1. Apparatus for distilling vegetable and animal substances. A bottle for collecting air D is filled with water and then is inverted into vessel E filled with water. *The Scientific Papers of the Honourable Henry Cavendish*, ed. E. Thorpe, 2 vols. (Cambridge: Cambridge University Press, 1921) 2:308; hereafter in the captions *Sci. Pap.* Figure 2. Apparatus for subliming arsenic in a crucible, with a set of aludels attached, placed within a reverberatory furnace. Cavendish Mss II, 1(b): 21. Figure 3. Apparatus for measuring the expansion of air with heat; the bent tube contains mercury and air. Cavendish, *Sci. Pap.* 2:374. Reproductions by permission of the Chatsworth Settlement Trustees.

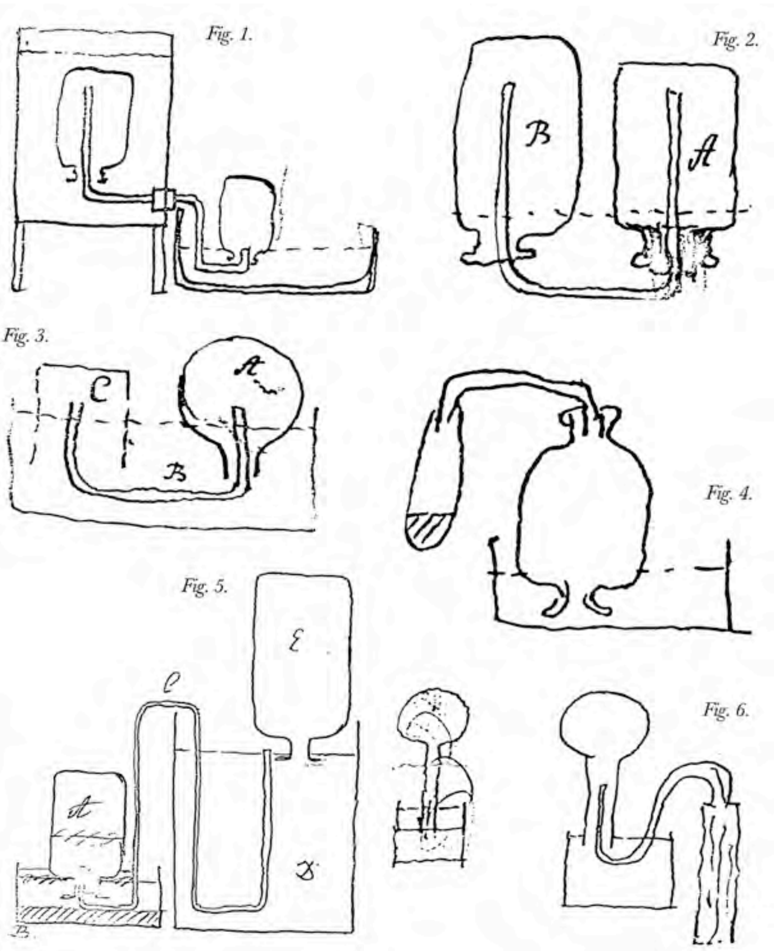

Figure 15.4: Laboratory Apparatus. Figure 1. Apparatus for an experiment to decide if heavier airs in a mixture of airs settle to the bottom. The mixture is contained in the bottle on the left, and as water is gradually let into it, different samples are caught in bottles on the right. Cavendish Mss II, 5:102. Figure 2. Apparatus for eudiometer experiments. Bottle B is filled with nitrous air, bottle with common air. Ibid. 5:42. Figure 3. Sulfur is burned in the glass globe A, and the air that is forced out by the heat is caught in jar C and examined, as part of Cavendish's eudiometer tests. Ibid. 5:61. Figure 4. Apparatus for capturing air upon boiling burnt charcoal with spirit of niter. Ibid. 5:345. Figure 5. Apparatus to determine if fixed air is produced by mixing common or dephlogisticated air in bottle A with nitrous air in bottle E. Ibid. 5:5. Figure 6. Apparatus to determine the effect on the volume of dry air by saturating it with moisture. Cavendish Mss Misc. Reproductions by permission of the Chatsworth Settlement Trustees.

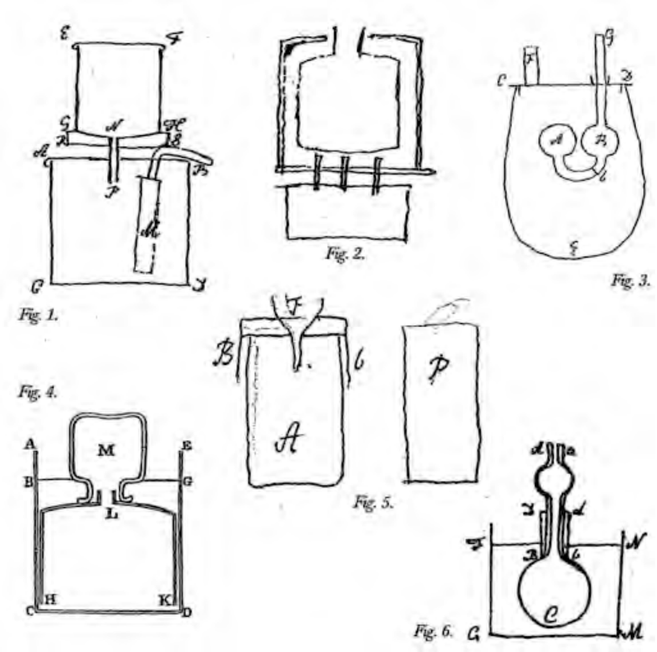

Figure 15.5: Laboratory Apparatus. Figure 1. Apparatus for experiments on the heats of mixtures. Through the cylindrical funnel on top, hot water is added to cold water in the pan below; M is a stirrer. Untitled paper on experiments on specific and latent heats, Cavendish Mss. Figure 2. Apparatus for determining the time of evaporation of boiling water. The water is contained in a tin bottle surrounded by an insulated tin frame and placed over a spirit lamp. Cavendish Mss III(a), 9:42. Figure 3. Apparatus to decide if the heat at which water becomes steam is the same as the heat of the steam. The ball A, which contains a little water and otherwise is filled with mercury to b, is exposed to steam and to the boiling water. Ibid. 1:1. Figure 4. Apparatus for collecting air discharged from pump water when it is boiled; the water is in ACDE, the air in M. Cavendish, *Sci. Pap.* 2:105. Figure 5. Apparatus to find the weight of fixed air in calcareous earth. Acid is poured through the funnel onto a sample of the earth contained in cylindrical glass A; after effervescence, the plug P is drawn in and out of the empty part of A to drive out any residual fixed air. Cavendish Mss II, 5:379. Figure 6. Apparatus to determine if the electrical charge of coated glass is the same whether hot or cold. The glass bowl C is filled with mercury as is the surrounding vessel, making it a Leiden jar, the charge of which is tested while a thermometer is dipped into the mercury at different heats. Cavendish, *Electrical Researches*, opposite p. 180. Reproductions by permission of the Chatsworth Settlement Trustees.

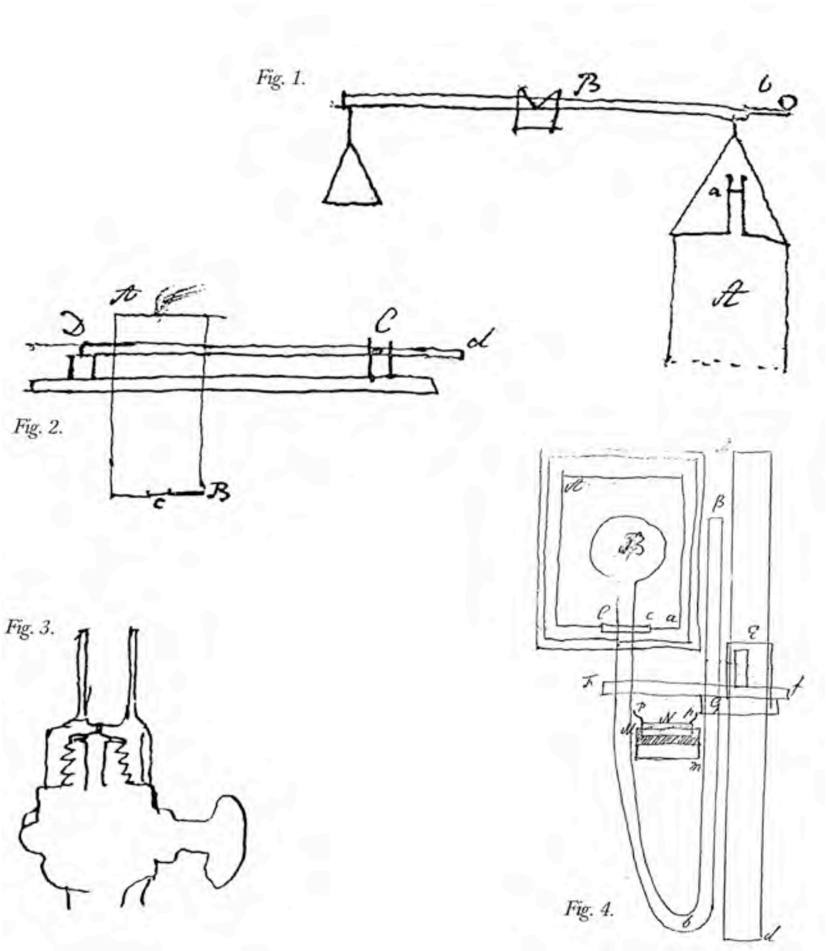

Figure 15.6: Laboratory Apparatus. Figure 1. Apparatus to test if the vis inertiae of phlogisticated air is the same in proportion to its weight as that of common air. The method requires finding the time in which a given quantity of air contained in A passes through a small hole at the top under a given pressure. Cavendish, *Sci. Pap.* 2:321. Figure 2. Apparatus for measuring the strength of the detonation of inflammable air with other airs. Air is admitted into the brass cylinder AB and electrically fired, lifting the pivoted board Dd to which it is fixed. Cavendish Mss II, 5:130. Figure 3. Apparatus for measuring the cold produced by the rarefaction of air. The brass cock is screwed over the cock of the condensing glass of an air pump. The ball of a thermometer is fitted into the cylinder of the cap, a small hole at the bottom of which allows the escaping condensed air to blow on the ball. Cavendish Mss III(a), 8:11. Figure 4. Apparatus for finding the "force of steam," or tension of aqueous vapor, at heats below 212°. A small amount of water stands above the mercury in Bb. The tin pot Aa contains heated water. Cavendish, Mss III(a), 1:40. Reproductions by permission of the Chatsworth Settlement.

Chapter 16
Earth

Philosophical Tours in Britain

Active in planning voyages of discovery, Cavendish never went on one himself. He did, however, make a number of journeys by carriage within Britain to expand his knowledge. On the first journey we know anything about, he passed through Oxford to Birmingham and back by way of Towcester, making trials of Edward Nairne's Earth-magnetic dipping needle at each stop, usually in a garden. Those trials may have been the whole point, for it was 1778, soon after Cavendish's report on the meteorological and magnetic instruments of the Royal Society, and he was still very much involved.[1] Beginning in 1785 Cavendish became a regular and more rounded scientific tourist. This fiftyish man of fixed, secluded habits had recently taken on an associate, Charles Blagden, who encouraged his adventurous turn. For three successive summers, Cavendish and Blagden made journeys to several parts of Britain, always in the summer when roads were at their best. A person who helped with arrangements for one of their journeys called it their "philosophical tour,"[2] which it was, though Cavendish called it simply a "journey."

An inveterate traveler, Blagden recorded his journeys in notes and letters, beginning with a journey he took to Scotland to study at age seventeen.[3] We have his report of a visit to Wales when he was twenty-three, an impressionable if unfocused tourist. An admirer of Rousseau, the "most eloquent & feeling of men,"[4] he was drawn to abbeys and vistas but he was also interested in mines, ironworks, and "philosophical curiosities." Having a strong desire to know the larger world, he was struck by the "extreme stupidity" of people who were entirely satisfied with their "little world." Wherever he traveled he was frustrated because people could not answer his simple questions about what lay a mile around them—places, routes, departures.[5] When after serving several years as a surgeon to the British Army in North America he returned to England, he toured Devonshire where he found the coves and rocks "beautiful" and "romantic," and where he also observed mileages, weather, slate, and

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[1] Henry Cavendish, "Trials of Nairne's Needle in Different Parts of England," Cavendish Mss IX, 11:45–54. Dates in the second half of August 1778 are scattered through this record of observations.

[2] George Hunt to Mr. Hext, 23 Jan. 1787, Blagden Papers, Yale, box 1, folder 4.

[3] Charles Blagden to Sarah Nelmes, 1 Nov. 1765, Blagden Letters, Royal Society, B. 159. In other letters from 1767 Blagden gave Nelmes accounts of shorter journeys in Scotland. Nelmes, who lived in Bristol, was related to Blagden. "Accounts, Bills, Insurance, and Copy of Will of S. Nelmes," Blagden Mss, Royal Society.

[4] Blagden recommended reading Rousseau to Thomas Curtis, 26 July 1771, Blagden Letters, Royal Society, B. 162.

[5] Charles Blagden, "Memorandum of a Tour Taken for Four Days Beginning August 18 1771," Blagden Papers, Yale, box 1, folder 3.

clay.[6] His most consequential journey was from Plymouth to London in 1781, where he made a life for himself in science.

It was Blagden who suggested the journey that he and Cavendish made in 1785. Early that year he proposed that they visit John Michell in Yorkshire to see the progress he had made with his "great telescope." Blagden was unsuccessful at first, and by the time Michell extended a formal invitation, he and Cavendish had set out in a different direction.[7] The journey they did make that year was Blagden's idea too, as he explained: he "proposed the scheme one day" of visiting the ironworks near Cardiff, and when he described them, Cavendish became "very curious" and agreed to make the trip. Blagden wrote about their plans to his brother-in-law William Lewis, who was ironmaster at Pentyrch near Cardiff. Lewis offered them his house, but if the "Hammers should be too noisy" he would put them up at another house at a remove from the pounding.[8]

There was nothing odd about Cavendish's curiosity about ironworks. The English aristocracy was generally forward-looking, ready to promote and invest in industry and sometimes to participate directly. They often took a lively interest in engineering and industrial development. When they got together, they might inspect a new canal lock or the draining of a fen, and on journeys they might visit industries on the way. From early on, they had a correct appreciation of the importance of transportation, especially if they were fortunate enough to own land containing minerals. The duke of Bridgewater built a canal running from coal mines on his estate to Manchester, the beginning of a network of water connections. Other peers followed the example.[9]

Cavendish and Blagden kept an account of their tours, written in part by Blagden, and in part by Cavendish.[10] Their first stop in 1785 was Alderley in Gloucestershire, where they stayed with Blagden's older brother John Blagden Hale, and from where they made a side trip to a dye works, the first of their many industrial visits. From Alderley they went to Pentyrch in Wales, where they stayed with William Lewis, who showed them the ironworks. They explored the nearby hills and coal pits, observing strata and testing stones with acid. The dominant feature of the land there is Garth Mountain, which they climbed carrying a barometer (Figs. 16.1–16.2). One of the objectives of Cavendish and Blagden's journeys was to measure heights by the barometer, a method used by surveyors and improved by scientists, in which there was considerable interest at the time.

Ever since Pascal sent his brother-in-law up a mountain with a barometer in 1648, the prospect of measuring the heights of scalable mountains with a barometer was seen as an alternative to the trigonometric method. The barometer measures the difference in height of a mercury column in air and in a vacuum. To translate that difference into the pressure of the atmosphere corrections need to be made for capillarity and temperature (and later for gravity and errors of the scale and the zero of the scale). In his report on the Royal Society's instruments, Cavendish gave corrections for capillarity, using a table prepared by his father

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[6] Charles Blagden, "Tour of the South Hams of Devonshire," 1780, Charles Blagden Diary, Yale, Osborn Shelves f c 16.

[7] Charles Blagden to John Michell, 25 Apr. 1785 and 13 Sep. 1785, drafts; in Russell McCormmach (2012, 399).

[8] Charles Blagden to William Lewis, 20 June 1785, draft, Blagden Letterbook, Yale. William Lewis to Charles Blagden, 25 June 1785, Blagden Letters, Royal Society, L.46.

[9] Montagu of Beaulieu (1970, 150).

[10] The journal is in a wrapper labeled in Cavendish's hand, "Computations & Observations in Journey 1785," Cavendish Mss X(a) 4:8. The journal was written by Blagden, but the copy at Chatsworth is in a copyist's hand. The original is in Blagden's papers at Yale.

of depressions in inches of mercury for bores varying from 0.1 to 0.6 inch diameters.[11] His colleagues Roy, Deluc, and George Shuckburgh, an expert on instruments, gave rules for temperature corrections, Roy's being the best.

Writing in 1777 Shuckburgh said that the method of measuring heights with a barometer had been "capable of but little precision till within these few years." He and Deluc published observations of elevations they had taken on Mont Blanc, Europe's highest peak. Although Shuckburgh used Deluc's rules for correcting the barometer for temperature, his measurements on the mountain differed from Deluc's. Using Deluc's and Shuckburgh's readings, Cavendish calculated the height of Mont Blanc, obtaining a result that was lower than Shuckburgh's by 700 feet. Cavendish also compared rules for taking heights by the barometer by Deluc, Maskelyne, and Pierre Bouguer, referring to his father's experiments on the specific gravity of air at different temperatures and pressures, and he assisted Roy in experiments on the expansion of mercury, again drawing on his father's work. Cavendish did a good deal of work on the barometric method of finding heights before applying it on his journeys in the 1780s.[12]

For carrying up mountains, Cavendish had a portable barometer made by Ramsden. Because it had to be vertical, the barometer came with a tripod, which folded up as a carrying case, with legs hollowed out at the bottom to accommodate the cistern. This barometer was very accurate: the height of the mercury column was read to one-five hundredths part of an inch by means of a nonius moved by rack work. Roy had two instruments identical to Cavendish's, finding them to agree within a few thousandths of an inch.[13]

Heights of Mountains

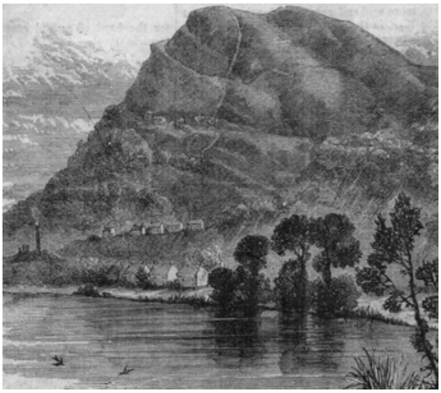

Figure 16.1: Garth Mountain. Near Cardiff. On the lower left, we see a furnace. Courtesy of Cardiff Central Library.

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[11] Middleton (1964, 172, 179, 189).

[12] Gavin de Beer (1956, 3–4). George Shuckburgh (1777, 1–2, 12–13). William Roy (1777, 673). Henry Cavendish, "Rule for Taking Heights of Barometers," Cavendish Mss VIII, 12; "Observations of Thermom. on Mont Blanc," Cavendish Mss, Misc. Charles Blagden to Joseph Banks, 5 Oct. 1786, BL Add Mss 33272, pp. 19–20.

[13] Middleton (1964, 132–133, 161).

Figure 16.2: Portable Barometer. Photograph by the authors at Chatsworth. This is probably the barometer that Cavendish carried to the top of Garth Mountain to measure its height. When folded into its mahogany case, the barometer measures 43½ inches. The instrument is suspended in gimbals. At the bottom, near the wooden cistern, there is a thermometer with a corrections scale. William Roy, with whom Cavendish collaborated on experiments with barometers, used a portable barometer almost identical to this one for taking heights of mountains. Although the Chatsworth barometer is unsigned, we know from Roy that this kind of barometer was made by Jesse Ramsden. Roy (1777, facing p. 658). The photograph is reproduced by permission of the Chatsworth Settlement Trustees.

Lewis showed Cavendish and Blagden the ironworks at Merthyr, where he was a part owner. Between 1759 and 1784, four independent ironworks were built near one another on the outskirts of Wales's first industrial town, Merthyr (Fig. 16.3). The works were still modest in size when Cavendish and Blagden saw them, but in the nineteenth century they would be the center of the British iron trade, and for a time two of the ironworks were the largest in the world.

The operations that Cavendish and Blagden witnessed centered on iron smelting, the first stage of which was carried out in blast furnaces. Built into hills, blast furnaces were usually made of stone blocks, narrowing toward the top and reaching to considerable heights. The furnace at Pentyrch was not especially tall, measuring twenty-six feet with a funnel that rose a bit higher, but the furnace at Merthyr was sixty feet tall. At the ground level of a blast furnace, there was a hearth with access in the front for tapping molten iron and slag. Ore, fuel, and limestone, a flux, were alternately introduced from the top.[14] Once going the red-hot charge might continue burning for weeks or months. A blast of air entered the furnace through one or two side openings near the bottom, increasing the flow of oxygen and raising the temperature of the furnace high enough to melt the materials. Traditionally the blast was produced by leather bellows operated by cams from a waterwheel, but by the time of Cavendish's visit most of the bellows had been replaced by cast-iron cylinders and pistons six feet in diameter, which had greater force. These too were operated by waterwheels, sometimes augmented by steam engines, usually the older Newcomen type, which returned water from the downstream to the upstream side of the wheel.[15] The iron produced by blast furnaces, called pig iron, could be used for making cast iron, but the most common kind of iron in the eighteenth century was wrought iron, which had to be refined. This was done by reheating the pig iron in smaller furnaces, or hearths, called forges.

Ironworks

Figure 16.3: Working Iron at Merthyr Tydfil. Watercolor by J.C. Ibbetson in 1792. A mass of hot iron is being struck by a trip hammer to remove slag. Courtesy of Cyfarthfa Castle Museum.

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[14] Laurence Ince (1993, 9).

[15] Ibid., 9–11.

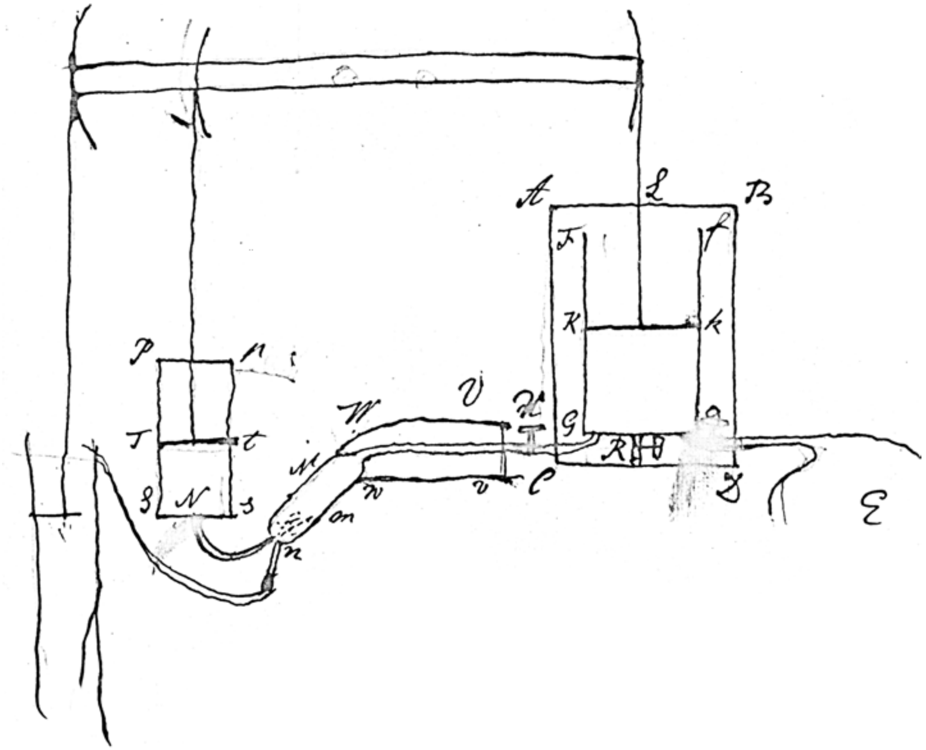

Figure 16.4: Cavendish’s Drawing of a Steam Engine. In this diagram, Mm is the condensation chamber, Pp is the air pump, and is Ff is the working cylinder. Cavendish gives the dimensions and the strokes per minute of the engine, and he notes its advantage: “In common [Newcomen] engine as much steam condensed on sides as is used to fill the cylinder.” Cavendish Mss, Misc. Reproduced by permission of the Chatsworth Settlement Trustees.

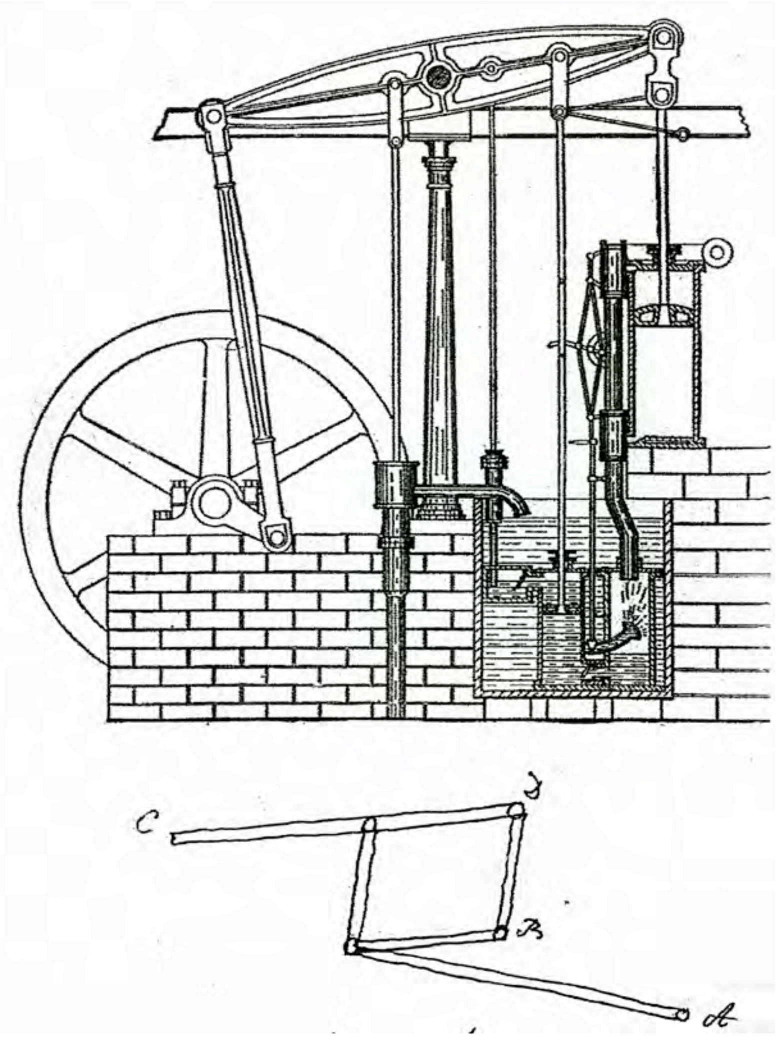

Figure 16.5: Parallel Motion. In the early Watt engines, the piston was connected to the beam by a chain. By replacing the chain with a rod, it was possible to develop power on the upward as well as the downward stroke, to push as well as pull, doubling the action of the engine. There was a problem, however. The piston rod moved vertically, while the beam moved circularly. Watt solved the problem with a four bar linkage between the rod and the beam in the form of a familiar pantograph, which produces parallel lines; in this case, parallel motion. A piston moving vertically up and down transmitted force in both directions to a circularly moving beam. Watt took out a patent on his “parallel motion” in 1784. Cavendish drew a picture of the linkage in his 1785 journal; it is shown at the bottom of this illustration.

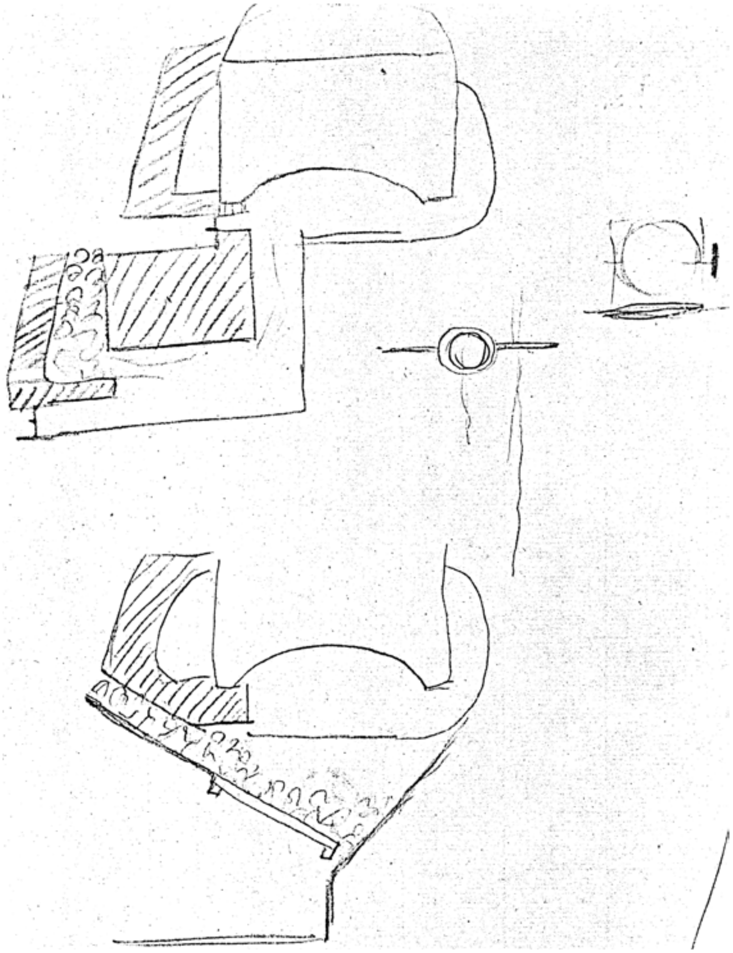

Figure 16.6: Cavendish’s Drawing of Watts Furnace for Burning Smoke. In 1785, Watt patented a smoke-consuming furnace. It had two sources of heat. On a grate, there was a regular fire, the first source. Where the fire was drawn into a flue or chimney, there was a second grate containing red-hot coals that had ceased to smoke, the second source; there the smoke of the first fire was consumed. Cavendish Mss, Misc. Reproduced by permission of the Chatsworth Settlement Trustees.

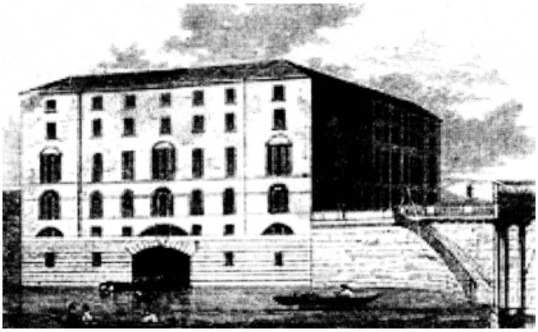

Figure 16.7: Albion Mills. Cavendish may have observed Watt's smoke-consuming furnace in Birmingham on his journey in the summer of 1785, or he may have observed it at Albion Mills, located on the Surrey side of Blackfriars Bridge. Built-in 1783–86, Albion Mills was the largest and technologically most up-to-date flour mill of the time. In the fall of 1785, Watt came to Albion Mills where his steam engine was to be installed. It was his advanced double-acting, rotative engine, proper for turning mills, and it was to be worked by his newly invented smoke-consuming furnace. In 1789, a second engine was installed. In 1791, Albion Mills burned down. It bears on Cavendish's interest that later that year, he together with Blagden, Banks, and the engineer John Smeaton were invited to inspect drawings of a steam engine and a waterwheel at Falcon Stairs, near Blackfriar's Bridge and the former Albion Mills. Charles Blagden to Joseph Banks, 23 Oct. 1785, Banks Correspondence, Kew, 1:212. John Maitland to Joseph Banks, 19 Dec. 1791, Manuscript Department, British Museum, Add Mss 33979, p. 118. Wikimedia.

Iron production was attended by intense heat, fiery chemical reactions, copious emission of gases, and heavy mechanical violence. Cavendish and Blagden's journals recount the scenes they witnessed. Under the hammer, fiery balls of iron "strike off sparks, some of which fly to a great distance, and a few have the brilliant appearance of steel dust in fireworks. There comes besides a white flame from different parts of the mass, and at times a different flame from certain spots, of a light bluish colour, like that from burning Sulphur."[16] Coalfields in the vicinity of the ironworks added to the effect. They passed a pit that had been burning many years, which they described: "from some places close by the road, a strong flame was now issuing, and the earth seen through the crevices and apertures in many places was red, or even white hot. All about the places actually burning, lay the cinders of old conflagrations."[17] Yet but for a difference of scale, there was a resemblance between ironworks and Cavendish's laboratory at home. In extracting pure metal from raw earth, workers used the same chemicals he did; they similarly combined their materials by proportionate weights and contended with impurities; and they had similarly used hearths and bellows.

Midway through their journey Blagden sent Banks an encouraging report. They had seen cloth and iron manufactures in "great perfection," and they had been "perfectly successful" in measuring the highest mountains in four counties and had plans to measure the Malvern Hills on the way to Birmingham.[18] In Birmingham they visited James Watt and

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[16] Blagden, Journal of 1785, p. 53.

[17] Ibid., p. 57.

[18] Charles Blagden to Joseph Banks, 31 July 1785, Banks Correspondence, Royal Botanic Gardens, Kew 1.199.

his partner Matthew Boulton at the latter's Soho Manufactory. Cavendish's papers contain a drawing he made of a steam engine of Watt's construction. In the 1780s Watt patented three major improvements of his steam engine, which itself was an improvement over the Newcomen engine. The first translated the reciprocal motion of the steam engine into a rotary motion, useful in manufacturing. The second doubled the amount of power the engine could deliver. The third was an application of the pantograph principle giving the piston the motion it needed for a double-acting engine, the invention Watt considered his masterwork (16.4). Watt told Cavendish about a scientific experiment he had performed with the steam engine on the condensation of steam, and Cavendish no doubt told him about his own experiments on the subject. Cavendish learned that Watt had invented a furnace to burn smoke, which he intended to apply to the steam engine. Later that year, Watt came to Albion Mills, near Blackfriar's Bridge in London, where his advanced double-acting, rotative steam engines worked by his new smoke-consuming furnaces were installed. Cavendish's papers contain a sketch he drew of Watt's furnace, probably on a visit to Albion Mills (Figs. 16.6–16.7).[19]

New Willey Ironworks near Broseley in Shropshire was their next stop. Its ironmaster was John Wilkinson, whose innovative boring mill for making cannon was exactly what Watt needed to make accurate cylinders for his engines, improving their efficiency by correcting for leakage of steam. They visited a second, new ironworks of Wilkinson's at Bradley, near Birmingham. This ironworks differed from others they had seen in the use of reverberatory furnaces instead of the traditional hearth forges, in which iron lies directly on the fuel, which contains impurities. The advantage of reverberatory furnaces is that the iron is separated from the fuel, heated by hot gases flowing over it and by radiant heat reflected from the roof of the furnace.

They visited the ironworks at Colebrookdale, a large plant a quarter mile in length, near the historic Ironbridge. Abraham Darby III, the third-generation head of the company, had made castings for the bridge, the first major structural use of cast iron. The still-standing 100-foot, semi-circular bridge spanning the River Severn linked ironworks at Coalbrookdale with sites across the river.[20] The blast for the two furnaces at Coalbrookdale was delivered by two cylinders powered by water raised by a steam engine of Watt's design. The year of Cavendish and Blagden's visit another steam engine was installed to blow air at two forges located outside the building.[21]

Steam engines came with the setting of their journeys, which was the Industrial Revolution. A new landscape was taking shape, into which Cavendish ventured with the same curiosity he brought to his studies in mechanics, chemistry, and heat. On their first journey, in addition to iron-making, he and Blagden saw a range of industrial operations: quarrying, coal-mining, coke-making, brass-drawing, tin-plating, and more. They saw slitting mills, flattening mills, cannon mills, trip-hammers, cranes and other equipment for moving hot heavy masses. They saw the finished products, iron and steel made into buttons, needles, nails, and ship bolts.

Wherever they went, they talked to owners, engineers, and workman, who gave them information no one else could. In their journals they also recorded observations of strata, rocks, and pebbles surfacing the roads, and on separate sheets they kept a record of barometer

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[19] Initially there were problems with the piston rod and the sun-and-planet gear of Watt's engine, but by early 1786 the repairs had been made. In 1789, a second engine was installed.

[20] S.B. Hamilton (1958, 455–456).

[21] Richard Hayman (2003, 71).

and thermometer readings, from which Cavendish calculated elevations. Blagden wrote to Banks that Cavendish “bears the journey remarkably well.”[22] The journey lasted about three weeks.

The following year, 1786, Cavendish and Blagden set out again on a three-week journey, this one longer than the first, to the north of England.[23] They traveled directly to John Michell’s parsonage at Thornhill, near Wakefield in Yorkshire; after a short visit, they left Michell and then returned to stay several more days.[24] By then Michell had worked for many years on a telescope, which when Cavendish and Blagden saw it had the biggest mirror of any telescope in the world. Blagden wrote in his diary the only account of what it was like to look through it. “At Mr Michell’s took some altitudes & looked over his fossils [. . .] At night looked thro’ his telescope: tho’ much false light & confused images yet observed ♄ with it well: could see the belt plainly; & observed an emersion of the 3 sat. much better than it appeared thro’ the 2 feet reflector.”[25] On Saturday, Blagden went to Michell’s sermon, which he had heard or read before; he said nothing about Cavendish attending the sermon. Cavendish discussed geology with Michell, and he came away with a copy of Michell’s table of strata going down 221 feet, measured to the inch.[26]

Cavendish took advantage of the journey to follow up his chemical interests. He accepted Lord Mulgrave’s invitation to visit his alum works, “having formerly made experiments himself on the crystalization of alum.”[27] After the journey, alum liquor and related substances from the alum works were sent to Cavendish in London.[28] The connection of the journey with Cavendish’s scientific work can be seen in the interest he took in plumbago, a graphite substance formed in furnaces during the extraction of iron from its ore. He and Blagden made a special trip to Rotheram to enquire about plumbago, and in Chesterfield Cavendish succeeded in acquiring a specimen of kish iron “for examination,” kish being the workmen’s name for plumbago. Plumbago had come up in connection with Kirwan’s criticism of Cavendish’s 1783 paper “Experiments on Air” for failing to take into account the production of fixed air. In his answer, Cavendish said that Kirwan’s belief that a mixture of iron filings and red precipitate produced fixed air would be a strong argument if it were not that iron contains plumbago, and plumbago was known to consist mainly of fixed air. Cavendish performed an experiment to show that Kirwan’s fixed air had come from the plumbago in his iron filings rather than from the iron itself, as Kirwan believed.[29] Before Cavendish and Blagden began their first journey, no doubt at Cavendish’s request, Blagden wrote to the chemist Peter Woulfe in Paris asking him to apply to a French chemist there for

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[22] Charles Blagden to Joseph Banks, 31 July 1785, Banks Correspondence, Royal Botanic Gardens, Kew, 1.199.

[23] “Computations & Observations in Journey 1786,” Cavendish Mss X(a), 5. The wrapper is labeled in Cavendish’s hand; the narrative is written in the copyist’s.

[24] Charles Blagden to C.J. Phipps, Lord Mulgrave, 2 Aug., 1786, draft, Blagden Letters, Royal Society 7:17. Charles Blagden to John Blagden Hale, 14 Sep. 1786, draft, ibid. 7:33. Charles Blagden to John Michell, 5 Aug. 1786, draft; in McCormmach (2012, 407–408).

[25] 2 Sep. 1786, Charles Blagden Diary, Yale, Osborn Shelves f c 16

[26] Henry Cavendish, “Strata Which Michell Dug Through for Coal,” in Cavendish’s journal of the 1786 trip, Cavendish Mss X(a), 3:13–14.

[27] Charles Blagden to C.J. Phipps, Lord Mulgrave, 2 Aug. 1786, draft, Royal Society 7:17.

[28] “Examination of Substances Sent from Lord Mulgrave’s,” in “White Book,” Cavendish Mss Misc., pp. 7–13.

[29] Kirwan thought that the phlogistication of air generates fixed air. Cavendish knew that it does not. Henry Cavendish (1784b, 184). In 1779 Scheele performed experiments on plumbago, a substance which had been used in pencils, showing that it consists mainly of carbon with some iron. Thomson (1830–1831, 2:71).

an abstract of his memoir on plumbago.[30] Cavendish brought his interest in plumbago with him on his journey to the ironworks.

In Sheffield they observed file-making and other manufactures "pretty much in detail." They stayed at a place recommended by Michell, the Fortune Inn, which proved to be "the vilest house," Blagden complained to Michell, at which he "ever had the misfortune to put up."[31] Michell said that he knew it only by reputation and would not recommend it again. In Chesterfield they went down a mine, which Blagden found "fatiguing," his legs too short for the turns in the ladder; he said nothing of Cavendish's discomfort, if he experienced any.[32] "Tempestuous" wind and rain frustrated their plans to climb mountains in the Lake District, forcing them to leave sooner than they had planned, but not before Blagden had caught a glimpse of the "magnificent & beautiful" scene.[33] What Cavendish thought of the natural beauty of the lakes he did not say, but it would seem that he was indifferent to it. The closest Blagden came to criticizing Cavendish in writing was in a letter fifteen years later, where he wrote, "When I went to the lakes it was in company with Mr. Cavendish, who had no curiosity for several things which it would have given me great pleasure to have seen. Winander More struck me as the *prettiest* piece of water I had ever beheld."[34] What Cavendish took away from the scene is suggested in a letter Blagden wrote to Banks a month after their return: Cavendish was "making experiments upon the stones we brought home," and on specimens from the industrial works, "which will find him some employment if he critically examines them all."[35]

For the third straight year, in 1787 Cavendish and Blagden set off on a journey, this time to the southwestern corner of England, Cornwall. They brought with them letters of introduction written by Watt and Boulton among others.[36] Cavendish and Blagden went down a tin mine 800 fathoms deep, Blagden again finding the descent troublesome and little of interest at the bottom except for the manner of working, which had to be seen to be understood. On the rest of the trip he and Cavendish contented themselves with seeing what was above ground.[37] They visited Josiah Wedgwood's clay pits for porcelain manufacture; the previous winter Wedgwood had sent Blagden specimens of feldspar, with the request that he show them to Cavendish.[38] They visited smelters with their strong smell of arsenic and their workman covered with red dust. They saw big stampers driven by waterwheels, crushing ore, and steam engines emptying mine shafts of water and hauling up ore.[39] They saw pumping machinery improved by Watt, to whom, Blagden thought, the Cornish were

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[30] Peter Woulfe to Charles Blagden, 26 June [?] 1785, Blagden Letters, Royal Society W30.

[31] Charles Blagden to John Michell, 19 Sep. 1786, draft; in McCormmach (2012, 409–412).

[32] Charles Blagden to Joseph Banks, 17 Sep. 1786, BL Add Mss 33272, pp. 9–10.

[33] Charles Blagden to Joseph Banks, 4 Sep. 1786, ibid., pp. 7–8.

[34] Charles Blagden to Henry Temple, Lord Palmerston, 25 Nov. 1800, Blagden Papers, Yale, box 63/43.

[35] Charles Blagden to Joseph Banks, 8[?] Oct. 1786, BL Add Mss, 33272, pp. 15–16.

[36] Charles Blagden to James Watt, 23 Aug. 1787, draft, Blagden Letters, Royal Society 7:349. Two letters of introduction from George Hunt, 23 Jan. 1787, who was asked to write them by his nephew R. Wilbraham, "The bearers of this are Mr.Cavendish …." Blagden Papers, Yale, box 1, folder 4. Along the way Blagden solicited letters: James Reynolds to Rev. Burlington, 18 Aug. 1787, "The bearer, Dr Blagden, is my particular friend …." Blagden Letters, Royal Society, R.5.

[37] Charles Blagden to Mrs. Grey, 14 June 1787, draft, Blagden Letters, Royal Society 7:324. Charles Blagden to William Watson, 22 Aug. 1787, draft, ibid. 7:347.

[38] Josiah Wedgwood to Charles Blagden, 30 Dec. 1786, Gloucestershire Record Office, D 1086, F 158.

[39] Thirty-page journal of the 1787 journey, by Blagden, in a copyist's hand, and with many insertions in Cavendish's hand. Cavendish Mss X(a), 6.

indebted to be able to "work their copper mines at all."[40] Cavendish collected specimens to subject to "chemical analysis" which Blagden expected would "shew some more light" on how they were formed.[41]

On their route to Cornwall, they followed the seacoast "on account of particular experiments to be done there."[42] On Dartmoor in southwest Devonshire, they carried out an elaborate series of observations with barometers, thermometers, and rain gauges having to do with a problem in the barometric measurements of heights. Blagden, who had lived in nearby Plymouth, made the local arrangements, which involved the assistance of three other men and the construction of a small meteorological observatory on the boulder-strewn hills of Dartmoor, rising to 2000 feet.[43] The scientific expedition into the wet and windy moors was planned and funded by Cavendish.

On their journey, between industrial sites they observed strata as usual,[44] and this time fair weather permitted them to climb mountains with their barometer.[45] On their return through north Devon, Blagden, who had been there before, took "great pleasure in shewing to Mr.Cavendish" the "grand beauties of that remarkable coast." Blagden reported to Banks that Cavendish looked "the better for his journey."[46]

Cavendish and Blagden made no more journeys together. In the summer of 1788, Blagden went to France, sending back scientific news to Cavendish.[47] So familiar had they become as a traveling pair that the following year Blagden had to correct Deluc, explaining that he was planning a tour of Italy not with Cavendish but with Lord Palmerston.[48] Cavendish made one more journey we know of, in 1793. Blagden was then living in Europe,[49] and this time it was Banks who planned it. He wanted Cavendish to witness trials of a new steam engine working the Gregory lead mine in Derbyshire, in which Banks had an interest. Banks urged Watt and Boulton to meet with Cavendish at the mine,[50] and in the notes Cavendish kept of the journey, he mentioned an experiment of Watt's to determine the specific gravity of steam.

Such were Cavendish's journeys in his middle years. Setting out from London in different directions, he explored different corners of the kingdom. Wherever he went, he examined industrial processes, materials, and products, determined the heights of mountains, observed the "order of the strata," and collected stones, noting their physical characteristics and investigating them chemically. From his observations and other sources, he wrote a paper on the strata of the island.[51] He was a tourist with an active curiosity and definite tastes: what interested him he pursued tirelessly, and what did not he silently ignored.

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[40] Charles Blagden to Mrs. Grey, 28 Aug. 1787, draft, Blagden Letters, Royal Society 7:351.

[41] Charles Blagden to John Michell, 1 Sep. 1787, draft; in McCormmach (2012, 434–436).

[42] Charles Blagden to William Lewis, 11 July 1787, draft, Blagden Letters, Royal Society 7:338.

[43] Brian Le Messurier, ed. (1967, 15). Charles Blagden to William Farr, 12 June 1787, draft, Blagden Letters, Royal Society 7:67; and other correspondence with Farr around this time.

[44] Henry Cavendish's journal of the 1787 trip, Cavendish Mss X(a), 7.

[45] There are several large sheets of observations taken with the barometer on the 1787 trip, in Cavendish Mss Misc.

[46] Charles Blagden to Joseph Banks, 14 Aug. 1787, Add Mss 33272.

[47] Charles Blagden to Joseph Banks, 13 July 1788, ibid.

[48] Charles Blagden to Jean André Deluc, 5 Sep. 1789, draft, Blagden Letters, Royal Society 7:301.

[49] Charles Blagden to Joseph Banks, 11 May 1793, BL Add Mss 33272, pp. 119–20. Henry Cavendish to Joseph Banks, 23 Sep. 1793; in Jungnickel and McCormmach (1999, 696).

[50] Joseph Banks to Matthew Boulton, 6 and 18 July, 10 Aug. 1793, Birmingham Assay Office.

[51] This twenty-one page paper on strata in Cavendish's hand does not have a group number, but it is kept with the travel journals in the Cavendish Mss.

A great reader of travel books, as we know from his library, Cavendish was prepared to be enticed out of his study by Blagden and to become himself, for a time, a traveler. His journals differ from the usual types of travel journals by their exclusive focus, though they have much in common with the geological and industrial observations of William Lewis's and Charles Hatchett's, and with the geological observations of Deluc's and Saussure's.[52]

The journeys marked a change in the direction of Cavendish's work. His course of experiments in pneumatic chemistry came to an end with his paper on phlogisticated air in 1785, the year he made his first journey with Blagden. In 1786 he began keeping a new record of chemical experiments, an indexed, bound book, which he labeled "White Book No. 1." It contains transcriptions from his laboratory notes, some of which are inserted loosely, not yet transcribed, bearing telltale chemical stains.[53] The experiments it records span twenty years, to 1806; their subject could be called geological and industrial chemistry, but the simpler description of mineralogical chemistry would not be misleading, given the eighteenth century practice of using of "mineralogy" to stand for both ores and stones.[54] The *Philosophical Transactions* at the turn of the century contained substantial papers in this field, the challenge of which one of the authors Richard Chenevix described: to establish qualitatively the presence of different substances in the specimens required "delicate research," and to determine quantitatively their proportions was the "most difficult operation of analytic chemistry."[55]

The "White Book" came to light relatively recently. The variety of substances Cavendish examined can be suggested by a few entries: whitish sparkling ore from Hudson's Bay, native iron from Mexico, earth from Isle of Man, lava from Mount Vesuvius, limestone, chalk, clay, and mica. Making no distinction between the natural and the manmade, the book also records experiments on specimens from mines and wastes from industrial processes such as kish from iron furnaces, slag from the purification of copper, finery cinder, and dust from lead smelting furnaces. The engineer James Cockshutt supplied Cavendish with specimens of coal and iron, and Cavendish wrote a paper on the making of iron with recommendations for the engineer,[56] an exchange we might view as an early meeting of two revolutions, the scientific and the industrial.

Cavendish's journals are the first indication of his active interest in geology. In Britain in the late eighteenth century, the main spur to geology came from what he was doing, crossing large tracts of country making observations of strata.[57] When Blagden toured the Continent, he reported to Cavendish on the soils there, extending his observations on the other side of the Channel.[58] Cavendish acquired considerable knowledge of geology, but

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[52] Horace Bénédict de Saussure (1786). Jean André Deluc (1810). Charles Hatchett (1967). F.W. Gibbs (1952, 211).

[53] This book has 138 numbered pages; 90 loose sheets are laid between the bound ones. Large blank spaces are left in the book for cross referencing and later additions. It is a copy book for preserving results of experiments. "White Book No. 1," Cavendish Mss, Misc. On p. 59 Cavendish refers to "2d book," which suggests that there was once a "White Book No. 2."

[54] V.A. Eyles (1969, 175).

[55] Richard Chenevix (1801, 209).

[56] Henry Cavendish, "Paper Given to Cockshutt," inserted loosely in "White Book No. 1," Cavendish Mss Misc.

[57] Roy Porter (1977, 119).

[58] The guiding thought appeared in John Michell's paper on earthquakes, where he noted that level countries show great expanses of the same strata: "we have an instance of this in the chalky and flinty counties of England and France, which (excepting the interruption of the Channel, and the clays, sands, of a few counties) compose a tract of about 300 miles each way." John Michell (1760, 587).

nothing suggests that he had any thought of publication. In one place he acknowledged that he was scratching the surface of the Earth, and that only superficial knowledge could come of it.[59] He mentioned an experiment of Watt's to determine the specific gravity of steam. The last candidate Cavendish recommended for fellowship in the Royal Society was a geologist, James Hall, in 1806.[60] Known as the "father" of experimental geology, Hall is remembered especially for his experiments in answer to criticisms of James Hutton's *Theory of the Earth*. A principal criticism came from an early result of pneumatic chemistry. Against Hutton's explanation of the formation of limestone by subterranean heat, his critics argued that heat would have calcined the limestone, driving off its fixed air (carbon dioxide) and converting it to quicklime, as Black had shown. Using Wedgwood pyrometers to measure temperatures upwards of 1000°, and using Benjamin Thompson's method of measuring the force of gunpowder to determine very high pressures, Hall proved that Hutton was right. In other experiments, to which he was led in part by observations in a glass factory, Hall proved that fused basalt becomes stony masses when it cools, not just glass as Hutton's critics maintained.[61] We do not know what Cavendish thought of Hutton's theory, but we suspect that he liked it better than he did the theories of Hutton's critics such as Deluc and Kirwan, who upheld the account in Genesis of the origin of the world.[62] John Playfair, the foremost exponent of Hutton's theory, said that geology used to explain everything by the "first origins of things," the reason it was so long in becoming a science; geology as a science was properly concerned to "discover the laws" of the great "revolutions" of the Earth.[63] Hall, he said, agreed that geology as a science properly sought "laws." That Cavendish would have approved of Hall's direction in the science is supported by his experiment of weighing the world, discussed next.

Weighing the World

The first indication of Cavendish's interest in the experiment appears in a letter to John Michell in 1783. Michell was having difficulty completing his large telescope, and Cavendish wrote to him with a suggestion: "if your health does not allow you to go on with that I hope it may at least permit the easier and less laborious employment of weighing the world." Tactfully, Cavendish expressed his preference: "for my own part I do not know whether I had not rather hear that you had given the exper. of weighing the world a fair trial than that you had finished the great telescope."[64] Michell died ten years later, in 1793, without having tried the experiment (or finished the telescope). Most of his instruments and apparatus were left to his former college in Cambridge, Queens'.[65] What happened next is explained at the beginning of Cavendish's paper in the *Philosophical Transactions* for 1798, "Experiments to Determine the Density of the Earth." "Many years ago, the late Rev. John Michell, of this Society, contrived a method of determining the density of the Earth, by ren-

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[59] Archibald Geikie, "Note on Cavendish as a Geologist," in Cavendish, *Sci. Pap.* 2:432.

[60] 20 Feb. 1806, Certificates, Royal Society, 6.

[61] V.A. Eyles (1972, 54).

[62] Jean André Deluc (1809, vi, 24, 63–64). Deluc argued against Hall's experimental conclusions, pp. 359–361. Kirwan said that geological facts are historical, relying on testimony, and that recourse cannot be made to experiment. Richard Kirwan (1799, 4–6, 482).

[63] Playfair quoted in Deluc (1809, 11–14).

[64] Henry Cavendish to John Michell, 27 May 1783, draft; in Jungnickel and McCormmach (1999, 567–569).

[65] "Michell, John," *DNB*, 1st ed. 13:333–334, on 334.

dering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me."[66] Wollaston belonged to a family of men of science and the Church, all of whom had studied at Cambridge; Cavendish knew them all.[67]

Michell's apparatus came to be known as a "torsion balance." In a footnote to his paper in 1798, Cavendish referred to Coulomb's use of an apparatus of the same kind for measuring small electric and magnetic attractions in the mid 1780s: "Mr. Michell informed me of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments." As to when Michell came to his idea of measuring the density of the Earth with a torsion balance, and when Cavendish learned about it, we are not told. We know that it was no later than 1783, for Cavendish referred to it that year. We can set a lower limit on the time by a paper that Cavendish gave to Maskelyne in or around 1773 in which he said that he knew of only two practical ways of finding the average density of the Earth, by a pendulum beating seconds and by the attraction of a mountain;[68] he said nothing about Michell's third way, by a torsion balance.

Cavendish was nearly sixty-seven when he "weighed the world," the name he and Michell used for the experiment. He began the experiment, which was in reality seventeen "experiments," each consisting of many trials, on 5 August 1797, completing the first eight of these by the last week in September. The remaining nine he carried out in April and May of the following year. The paper reporting them was read to the Royal Society on 21 June 1798, just three weeks after the last experiment.

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[66] Henry Cavendish (1798, 249)

[67] Wollaston's father, Francis, born the same year as Cavendish and a classmate of Cavendish's at Cambridge, took his degree in law but entered the Church instead. Skilled in astronomy, he had his own observatory and first-class instruments. With at least that much in common, on 8 Dec. 1768 Cavendish brought Francis Wollaston as a guest to a meeting of the Royal Society. The certificate proposing Wollaston's membership is signed by Cavendish along with Maskelyne and several other prominent members. 3 Jan. 1769, Certificates, Royal Society 3:65. "Wollaston, Francis," *DNB*, 1st ed. 21:778–779. One of Francis Wollaston's sons, William Hyde Wollaston, was an eminent chemist, whom Cavendish proposed as he had his father for membership in the Royal Society. 9 May 1793, Certificates, Royal Society 5; "Wollaston, William Hyde," *DNB*, 1st ed. 21:782–787, on 782. Another of Francis's sons George Hyde Wollaston was one of Cavendish's neighbors on Clapham Common, where Cavendish performed his experiments on the density of the Earth. George Hyde Wollaston's house along with Cavendish's are on the map of Clapham Common (Fig. 11.12). Another of Francis's sons was Francis John Hyde Wollaston, Jacksonian Professor of Chemistry, from whom Cavendish received Michell's apparatus. "Wollaston, Francis John Hyde," *DNB*, 1st ed. 21:779–780. Michell's association with the Wollastons went back as far as Cavendish's. As a recently elected fellow of the Royal Society, Michell's first recommendation for a new member, in 1762, was for Francis's youngest brother, George Wollaston, then fellow and mathematical lecturer in Sidney-Sussex College, Cambridge. "Wollaston, Francis," 779.

[68] Henry Cavendish, "Paper Given to Maskelyne Relating to Attraction & Form of Earth," VI(b), 1:19.

Apparatus for Weighing the World

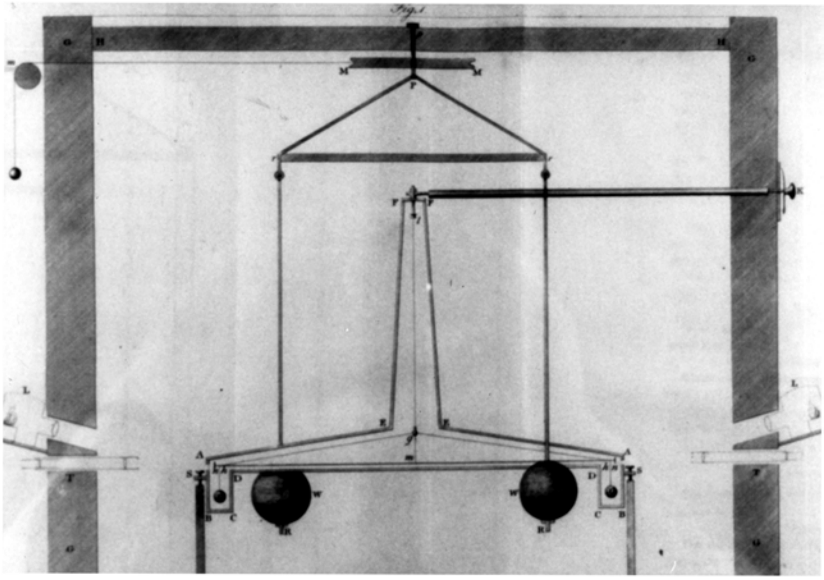

Figure 16.8: Apparatus for Weighing the World. Cavendish's modified and rebuilt version of John Michell's apparatus. The large metal spheres R are weights that attract small metal spheres suspended from the ends of the arm, which in turn is suspended by the fine wire gl. The room in which the apparatus is housed and protected is also shown as are the arrangements for viewing it from outside the room. "Experiments to Determine the Density of the Earth," *PT* 88 (1798):526.

Cavendish began his account with words that should encourage readers, "The apparatus is very simple" (Fig. 16.8). Its principal moving part was a six-foot wooden rod suspended horizontally by a slender wire attached to its center, and suspended from each end of the rod was a lead ball two inches across. The whole was enclosed in a narrow wooden case to protect it from air currents. Toward the ends of the case and on opposite sides of it, were suspended two massive lead balls, or "weights," each weighing about 350 pounds. Cavendish rebuilt Michell's apparatus.

The force that turns the rod aside is the gravitational attraction between the weights and the balls. From the angle of twist of the rod and the period of vibration of the rod moving freely as a horizontal pendulum, the density of the Earth is deduced. It is not obvious how the Earth enters the experiment, becoming "obvious" only when the formulas for the forces acting in the experiment are written out and the resulting equations are combined. Cavendish did not use equations but worked with proportions, and as a result his reasoning is unfamiliar to a modern reader. The experiment essentially compares the gravitational attraction of the lead weights on the balls with the gravitational attraction of the Earth on the same, the source of the Earth in the experiment.[69]

Earlier in the century it had been an open question whether or not a mass the size of the mountain is sufficient to cause a measurable effect. Twenty-five years before Cavendish's experiment, the Royal Society had carried out a successful experiment on a mountain, and as we have seen, Cavendish had helped prepare for it. In the experiment Michell invented, Cavendish achieved a measurable effect with masses small enough to fit into an apparatus. Newton had been discouraging, having calculated that if two one-foot spheres of Earth-matter were placed only one-quarter inch apart, they would not "come together by the force of their mutual attraction in less than a month's time." Newton was right about the minuteness of the force: in Cavendish's experiment the gravitational attraction of the weights on the balls was of the order of one part in 10^8 (one hundred million) of the gravitational attraction of the Earth on them, that is, of their weight.[70]

Because the smallest disturbance could destroy the accuracy of the "weighing," Cavendish placed the apparatus in a small, closed "room" about ten feet high and as many feet across. From outside the room, Cavendish worked pulleys to swing the weights close to the case to set the rod in motion, the deflection and vibration of which he observed by means of telescopes installed at each end of the room. Veniers at the end of the rod enabled him to read its position to within one hundredth of an inch. The only light admitted into the room was provided by a lamp near each telescope. Once an experiment was underway, it was not interrupted until the end; depending on the stiffness of the suspension wire, it might take as long as two and one-half hours.

Given that the apparatus was simple and the procedure straightforward, it might seem that Cavendish's report of the experiment would be brief. It was not, taking up fifty-seven pages in the *Philosophical Transactions*, in length second only to his paper on the theory of electricity. The reason it was long was Cavendish's concern with accuracy. Near the beginning of his paper, where he estimated the minuteness of the gravitational force, he began a discussion of errors and corrections, which he continued to the end. The following account gives an idea of Cavendish's meticulous way of experimening.

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[69] In more detail, his reasoning is as follows. He deduces the density of the Earth in two steps. The first step assumes the laws of pendulum motion. The second step assumes the inverse square law of gravitation. Step 1. Cavendish draws on two relations: the period of vibration of a pendulum is proportional to the square root of the length of the pendulum and inversely proportional to the square root of the restoring force on the pendulum. With the aid of an analogy between the horizontal torsion pendulum and an imagined vertical simple pendulum beating seconds, the length of which is known, Cavendish expresses the force required to move the small balls at the ends of the torsion arm, with its observed period of vibration, through any observed angle of deflection of the arm in terms of the weight of a ball. Step 2. Cavendish invokes Newton's law of gravitation twice, once to express the attraction between a small ball and the nearby larger ball, or "weight," and once to express the attraction between the small ball and the Earth. The latter attraction is written so as to include the to-be-determined average density of the Earth. Forming a ratio of the two attractions, he expresses the attraction of the "weight" on the ball in terms of the attraction of the Earth on the same ball. Finally, he combines Steps 1 and 2. The force of the twisted wire from Step 1 is equal to the force of attraction between the small balls and the "weights" from Step 2. By dividing one force by the other, Cavendish arrives at the desired result: the density of the Earth, expressed in terms of the density of water, is equal to a numerical factor times the square of the period of vibration of the torsion arm divided by the deflection of the arm. By means of this reasoning, Cavendish brings the world into his laboratory.

[70] Isaac Newton (1962, 2:569–570). Cavendish stated the proportion as one part in fifty million, which applied to the 8-inch weights Michell intended to use. For the 12-inch weights Cavendish used, the proportion is roughly 3 times larger, but the order of magnitude of the minuteness remains the same.

Looking ahead to the conclusion, that unequal heating of the air was the disturbing force that was hardest to avoid, Cavendish explained how he located and designed the apparatus to minimize this main "source of error." Other sources of error he considered first. He found "some inaccuracy" in the vibration of the arm caused by the resistance of the air, but the "error" caused by the motion of the point of rest he found to be inconsiderable. He determined the time of vibration of the apparatus for each experiment separately to minimize the effect of "accidental attraction, such as electricity," arising from the plates of glass through which he observed the moving arm, causing an "error in the result." To determine the incidental attraction on the arm by the iron rods from which the heavy lead weights were suspended, he removed the weights. When he did, he found a disparity between his observations and his theoretical calculations of the attraction of the rods, which he first attributed to magnetism, but then upon replacing the iron rods by copper ones and still finding the same excess attraction, he concluded that it was due to an "accidental cause." Being unable to "correct" the "error," he calculated that its effect on the final result was no more than one thirtieth of the whole. With this measure of reassurance he continued with the main experiment. Next, observing that the attraction of the weights on the balls seemed slowly to increase with time, he suspected a "want of elasticity" in the wire or in something the wire was attached to, but by drawing on his knowledge of the limits of elasticity, and doing experiments on the wire he was using, he decided that this was an unlikely cause; he replaced the wire with a stiffer one nonetheless. His description of elastic after-working in the wire, it has been noted, was original, its discovery usually being assigned to the late nineteenth century. Finding that the attraction of the weights continued to vary, he suspected magnetism again; to check it out, he performed experiments to see if the weights and balls acquired the polarity of the Earth, arranging the weights so that they could turn on a vertical axis and rotating them daily, and then replacing the two-inch lead balls with ten-inch magnets and reversing them. The latter replacement is an example of what has been called one of the "grand principles of experimental physics": if a disturbing effect is suspected, it is made bigger to see how serious it is; Cavendish used this principle in his chemical work too, pointed out earlier. He decided once again that magnetism was not the source of the error. He next supposed that the cause of the variable attraction was "a difference in temperature between the weights and the case," producing a current of air. Even though he thought that this cause was "improbable," he took the apparatus apart and did new experiments, this time placing lamps beneath the weights and a thermometer next to the case. The effect was large after all, and so he did more experiments, burying thermometers in the weights and viewing them through the telescope by light reflected from a convex mirror, convincing himself that he had found a major source of this error: overnight the weights did not cool as much as the case, giving rise to convection currents, which pushed the balls toward the sides of the case. He then carried out the remaining experiments to determine the density of the Earth.[71]

Cavendish was not finished with errors. In calculating the density of the Earth from his data, he made several idealizations: the arm and the copper rods holding the weights have no weight, the weights attract only the nearest ball, and the attraction of the case is ignorable. In light of these, he made *six* "corrections," five of which were not of "much signification," but were "not entirely to be neglected" either. The important correction was the effect of the position of the arm on the attraction between the weights and the balls, which

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[71] Cavendish (1798, 250, 252, 254–255, 259, 263–267). C.W.F. Everett (1977, 548).

influenced the time of vibration. One of the corrections, that of the effect of the mahogany case on the arm inside it, required an extensive analysis, which Cavendish included in the paper as a mathematical appendix, even though the "whole force is so small as not to be worth regarding." In the conclusion of the paper, Cavendish gave a table of results of the seventeen experiments. They agreed closely with one another, but still the differences were too large to be explained fully by the "error of observation" or by air currents owing to temperature differences. He expressed the final outcome of the experiments as a mean of the results for each of the two wires, finding the two means to be the same. Noting that the extreme results differed from the mean by no more than one fourteenth of the whole, he concluded that the mean density of the Earth was determined "to great exactness" as 5.48 that of the density of water.[72]

Cavendish thought that his readers might object that because the outcome was influenced by currents of heated air, it could be influenced by yet another source, "some other cause, the laws of which we are not well acquainted with," leading to "a considerable error in the result." To put to rest this objection, he reminded his readers that he had made the experiments in various weathers and temperatures. He anticipated another objection; "namely, that it is uncertain whether, in these small distances, the force of gravity follows exactly the same law as in greater distances." His reply was that there was no evidence that the law differs "until bodies come within the actions of what is called the attraction of cohesion, and which seems to extend only to very minute distances." Nevertheless he carried out a number of experiments with the balls placed as close to the case as possible, finding no difference. In these ways, Cavendish concluded his paper with second and third thoughts about possible factors affecting the accuracy of the outcome.[73]

The experiment of weighing the world consisted of observations of matter moving in response to two of the best-known forces, gravity and the restoring force of twisted wire, but as we have seen, to achieve accuracy, Cavendish had to consider nearly all of the forces known to natural philosophy: in addition to gravity and elasticity, they were forces associated with magnetism, electricity, deformation, heat, and cohesion. Cavendish's mastery of the art of experiment rested on his mastery of natural philosophy.

Despite and in part because of his last experiment Cavendish had not freed himself from the claims of the earlier method of determining the density of the Earth, the attraction of mountains. His paper brought a prompt response from Charles Hutton, who had received copies of Cavendish's manuscript from both Maskelyne and the Royal Society. From the Royal Military Academy in Woolwich where he worked, he wrote to Cavendish about his "ingenious" paper, which made the density of the Earth 5.48 that of water. What led him to write the letter was the last paragraph of the paper, which called attention to the earlier, lower value of $4\frac{1}{2}$, in the "calculation of which" he, Hutton, had borne "so great a share." Anyone who has looked at Hutton's laborious calculations can sympathize. Hutton thought that Cavendish's wording hinted at inaccuracies in his calculations and seemed to disparage the Royal Society's experiment on the mountain in Scotland. That experiment, Hutton reminded Cavendish, had determined not the density of the Earth but only the ratio of that density to the density of the mountain, 9 to 5. Hutton had supposed that the density of the mountain is the density of ordinary stone, $2\frac{1}{2}$ times that of water, but the actual density of the mountain was unknown, as Hutton had pointed out at the time. All that was known was that Schehallien

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[72] Cavendish (1798, 277, 280, 283–284).

[73] Ibid., 284.

was a "massive stone," and Hutton now believed that its density was higher, 3 or even $3\frac{1}{2}$, which would make the density of the Earth "between 5 and 6," where Cavendish had put it, and "probably nearer the latter number." The Royal Society had not finished its experiment because it had not determined the density of stone, Hutton said. Even now, he hoped that the Society would do it, so that "an accurate conclusion, as to the density of the earth, may be thence obtained."[74]

Cavendish, as we have seen, repeated his experiment many times, in different seasons, and with attention to a range of possible errors and corrections, and he had taken mean values, considered the spread of the extreme values, and in general estimated the confidence that could be placed in 5.48. At the bottom of Hutton's letter to him, Cavendish drafted a brief response, which is identical to the last paragraph of his published paper.[75] In that paragraph, Cavendish did not commit himself as to which density, his or the Royal Society's, was more to be "depended on," since the Society's was "affected by irregularities whose quantity I cannot measure."[76]

In 1811, the year after Cavendish's death, John Playfair investigated the structure of the rocks of Schehallien, finding three kinds, with densities 2.4, 2.7 to 2.8, and 2.75 to 3. On the basis of these figures, Hutton calculated a new mean density of the mountain, about 2.75, which gave a value for the mean density of the Earth of "almost 5." As for the Royal Society's experiment on the attraction of mountains, Hutton said, "we may rest satisfied" with this result.[77] Playfair's values for the density of the mountain raised the density of the Earth, though it was still under Cavendish's 5.48, which was closer to, within 1 percent of, the accepted value today. After Cavendish's death, it was noticed that in averaging over the results of his experiments, he had made an arithmetic error; the corrected mean density of the Earth is 5.45, which is not as close, but still it is within 1.3 percent of today's value.

In the next century, the astronomer Francis Baily thought that Cavendish wrote his paper "more for the purpose of exhibiting a specimen of what he considered to be an excellent method, than of deducing a result which should lay claim to the full confidence of the scientific world."[78] In light of what Cavendish said at the end of his paper, we are inclined to think that he had both ends in view, but Baily was right to call attention to the method. It is that, not Cavendish's measurement, which has secured the experiment a lasting place among the methods of experimental physics.

Weighing the world had a precedent in William Gilbert's experiments on magnetism 200 years before. In his *De Magnete*, a classic work in early experimental physics, he wrote that he had formed "a little lodestone into the shape of the earth," and that he had "found the properties of the whole earth, in that little body," on which he could experiment at will.[79] Gilbert called his little Earth-shaped magnet a "terrella," a little Earth. We wonder if there was an association of ideas; at Chatsworth there is a terrella in a silver mount said to have belonged to Henry Cavendish.[80]

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[74] Charles Hutton to Henry Cavendish, 17 Nov. 1798; in Jungnickel and McCormmach (1999, 710–711).

[75] Cavendish's manuscript in the Royal Society does not show an interpolation on the last page. Perhaps Cavendish rewrote the last page, or perhaps he made no change in his wording in response to Hutton's letter. Henry Cavendish to Charles Hutton, draft, n.d. [after 17 Nov. 1798]; ibid., 712.

[76] Cavendish (1798, 284).

[77] Charles Hutton (1814, 2:64).

[78] Baily quoted in P.F. Titchmarsh (1966, 330).

[79] Kenelm Digby, 1645, quoted in "Biographical Memoir," in William Gilbert (1958, xviii).

[80] Mary Holbrook (1992, 113).

Cavendish had assisted the Royal Society in preparing the experiment on the mountain, but he did not take part in the experiment. His own experiment with metal spheres, his gravitational terrellas, corresponded to his normal way of life. To weigh the world, he did not need to go out into it; he could do it, and do it more precisely, in his laboratory, using an apparatus and reasoning from universal principles. He stayed at home and looked inside of the room and through a slit in a case, inside of which was the world on his terms.

At his home on Clapham Common, he worked largely in seclusion, though he used assistance when he needed it; in the last two parts of his experiment on the density of the Earth, he had George Gilpin, the clerk of the Royal Society, replace him at the telescope. Just as he was a private man and yet a constant companion of men of science, the work he carried out in seclusion entered the public world of established scientific problems, instrumental possibilities and qualified parties. If his experiment on the density of the Earth is looked at for what it tells us about Cavendish, as if it were a diary, which he did not keep, or a formal portrait, which he did not allow, it is a revealing experiment.

Weighing the world has been called a beautiful physics experiment, but it would be incorrect to call Cavendish a physicist, as we understand the word. He was a natural philosopher of the eighteenth century. One of the differences between the two is the conditions of work. In 1878 John Henry Poynting gave an account of experiments he undertook "to test the possibility of using the Common Balance in place of the Torsion Balance in the Cavendish Experiment," and in 1891 he reported on his continuing experiments with the common balance. For his repetition of the Cavendish experiment, he received a grant from the Royal Society, and he was given a place to work in the laboratory at Cambridge named after the Cavendish family. James Clark Maxwell, the first director of the Cavendish Laboratory, gave Poynting permission to do the experiment.[81] His experiment belongs to the time of physics, with its principal home in places of higher learning, with laboratories, directors, and grants. By contrast, Cavendish did his experiment by himself at his expense on Clapham Common.

When physics emerged in the nineteenth century, the worldview of physical science had changed from from Cavendish's day. An example is the role of time. Herschel, Kant, Buffon, and others from the middle of the eighteenth century envisioned the Earth and the heavens as evolving over eons in accordance with mechanical principles, but it would be scientists who came later who would work intensively within a worldview strongly imprinted by history.[82] Not eons but short durations, capable of exact measure, were the frame of reference of Cavendish's work; his instruments at the time of their auction contained "a very curious machine for measuring small portions of time."[83] Time for him was a measure of events, not a generator of events. He kept a number of clocks going, comparing them, timing the cooling of mixtures with them, and by the standard portrait of him, subjecting himself to their rule; they marked the regularity and sameness of nature and of his life. His interest in time is suggested by his study of the Hindu civil year, which is based on astronomical periodicities, portending nothing new in the world. In his work on heat, he arrived at the first law of thermodynamics, but he did not foresee a second law of thermodynamics, which implies

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[81] John Henry Poynting (1892, 565–566).

[82] Stephen Toulmin and June Goodfield (1965, 125, 266).

[83] Item 20 in *A Catalogue of Sundry Very Curious and Valuable Mathematical, Philosophical, and Optical Instruments [...] Of a Gentleman Deceased. ... On Saturday the Fifteenth of June 1816, at Twelve O'clock,* (London, 1816), Devon. Coll.

the physical directionality of time. His geological observations in the field led him to the chemistry of minerals not to ideas about the Earth evolving in time. His last important published experiment, the subject of this chapter, replaced the chemical balance, an instrument of precision, with a torsion balance, also an instrument of precision, both balances being instruments of equilibrium. The secular changes in his readings of the torsion balance were an error in the experiment. In the vanguard of the emerging physical science of precision, the Cavendish experiment was a complement in the laboratory of the periodic motions of the solar system, and as such it belonged to the classical Newtonian worldview.

The Cavendish Experiment

John Playfair wrote that skeptics would have predicted that after the systems of Aristotle and Descartes, Newton's too would pass: "This is, however, a conclusion that hardly anyone will now be bold enough to maintain, after a hundred years of the most scrupulous examination have done nothing but add to the evidence of the *Newtonian system*."[84] In his lectures on natural philosophy, Thomas Young said that Cavendish's result for the mean density of the Earth lay halfway between the limits guessed by Newton, between 5 and 6, a "new proof" of the "accuracy and penetration of that illustrious philosopher."[85] Conceived as a continuation of Newton's work, Cavendish's weighing of the world bestowed new honor on Newton, discoverer of imperishable truth.

Writing to Banks in 1802, Blagden reported a conversation with Laplace, which he thought Banks might want to pass along to Cavendish. Laplace said that many people suspected that the attraction Cavendish measured may involve electricity as well as gravity. For his part, Laplace wished that "Mr. Cav. would repeat it [the experiment] with another body of greater specific gravity than lead," such as a glass globe filled with mercury or a gold ingot.[86] In his paper Cavendish wrote that he planned to correct a defect in his method "in some future experiments," but so far as we know he did no more experiments, nor did he need to, for others would do them. In the following century, the density of the Earth was measured at least six times using Cavendish's method, twice using the Royal Society's method of the attraction of mountains, and several more times using a different method of the attraction of mountains; it was also done using the seconds pendulum and, as mentioned, the common balance.[87]

In time the Cavendish experiment ceased to be regarded as a way to determine the density of the Earth, even as it continued to be performed. It became instead the experiment to determine "big *G*," the gravitational constant appearing in the law of gravitational force, defined as the strength of attraction between two one-kilogram masses one meter apart. As C.V. Boys put it in 1892, "Owing to the universal character of the constant *G*, it seems to me to be descending from the sublime to the ridiculous to describe the object of this [Cavendish's and now Boys's] experiment as finding the mass of the earth or the mean density of the earth, or less accurately the weight of the earth."[88]

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[84] Playfair, quoted in Deluc (1809, 14–16).

[85] Thomas Young (1807, 2:575).

[86] Charles Blagden to Joseph Banks, 1 Apr. 1802, BL Add Mss 33272, pp. 172–173.

[87] B.E. Clotfelter (1987, 211). Notable repetitions include F. Reich (1838); Francis Baily (1843); C.V. Boys (1895).

[88] Boys is quoted by Clotfelter (1987, 211). Boys recommended using a room with a more uniform temperature then Oxford's; his accuracy was great, despite his room.

The Cavendish experiment today is often called the experiment to determine *G*, which is correct given that the experiment is the common possession of physics. It is often said that Cavendish's object was to determine *G*, which as a historical statement is incorrect but understandable given that the constant is more significant than the density of the Earth. In Cavendish's time, there was no independent unit of force, such as our dyne and Newton. The strength of any force was expressed in terms of an equivalent gravitational attraction, and weight was the measure of mass. The universal gravitational constant did not come up, though we can easily calculate it from Cavendish's data.[89] We find implicit in his work two of the three principal universal constants, the velocity of light *c* and *G* (Planck's constant *h* is the third), but Cavendish did not think of *c* as necessarily having a constant value, and it was the better part of a century after Cavendish's experiment before *G* entered physics.

Today, 300 years after Newton and 200 years after Cavendish, gravity is still at the center of physical research. To quote from a publication by researchers in the field: the "most important advance in experiments on gravitation and other delicate measurements was the introduction of the torsion balance by Michell and its use by Cavendish .... It has been the basis of all the most significant experiments on gravitation ever since."[90]

By its method and example, Cavendish's experiment has had a far-reaching influence on physics. In "Cavendish's skillful hands," the torsion balance has "revolutionized the science of precision measurement"; not only have nearly all of the determinations of *G* been done with that instrument, but it has been used in "countless other applications, such as seismological measurements and electrical calibration—wherever precise control over very small forces is called for."[91] A contributor to a symposium on general relativity traces the "noble tradition of precise measurement to which we are heirs" to Cavendish's experiment, which he calls the "first modern physics experiment."[92]

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[89] Cavendish did not write an equation for the force of universal gravitation, as we do: $F = \frac{GMm}{R^2}$. He could have calculated *G* without having a unit of force, but he had no need for it, and it would not have occurred to him. Clotfelter (1987, 213).

[90] A.H. Cook (1987, 52). Appropriately, Cook talks of the Cavendish experiment only in connection with *G* and not with the density of the Earth. Only recently, he says, has the accuracy of *G* been improved upon over what can be obtained from Cavendish's own experiment, and although in the study of materials we can achieve an accuracy of one part in 10^{12}, we still know *G* only to about 1 part in 10^3.

[91] Christian von Baeyer (1996, 98–99).

[92] Everett (1977, 546).

Chapter 17
Last Years

Clapham Common

Historians of chemistry may remember Clapham Common in the eighteenth century as the home of a distinguished chemist. Other historians remember it as the home of the "Clapham Sect," a group of prosperous, well-educated Anglican reformers known as evangelicals, who worshiped in the local church, Holy Trinity. Active at the time Cavendish weighed the world, members of the sect were fervently pious, believers in original sin and hellfire, living by the word of God, and working to save themselves, their countrymen, and heathen everywhere. Their goal was to breathe life into the Church of England, which they believed had capitulated to shallow eighteenth-century rationalism, with its external morality and calculus of happiness. They kept spiritual diaries, in which they recorded their sins at the end of each day. Their causes were social as well as religious: corruption in Parliament, barbarity of the criminal code, dueling, bull baiting, cockfighting, and, their most heartfelt, slavery, against which they fought for sixty years. One of their number, William Wilberforce, brought the first bill to outlaw the slave trade in 1789; it and subsequent bills failed until 1807, when persistence was rewarded, the abolition of slavery itself having to wait considerably longer. The meeting place of the Sect was an oval library in a roomy house on the Common belonging to Henry Thornton, a banker, Member of Parliament, president of the Sunday School Society, and chairman of the Sierra Leone Company. Thornton's cousin Wilberforce moved to Clapham Common to share his house. Thornton's somewhat less ardent brothers Samuel and Robert lived across the Common. The rector of Holy Trinity John Venn, founder of the Church Missionary Society, lived in another hospitable house on the Common.[1]

John Venn followed the path of his evangelical father, Henry, who had held the curacy at Clapham for some years. John did well in mathematics at Cambridge, and he was interested in astronomy and natural philosophy, able to explain the principles of the thermometer and compass; he owned a Dolland telescope and other good scientific instruments, and he read the *Philosophical Transactions*. While ministering to souls, he made a scientific contribution to Clapham by giving lectures on science to his own and his neighbors' children, and by introducing Jenner's smallpox vaccination to the whole parish. At Clapham, he often saw Isaac Milner, the capable Jacksonian Professor of Natural Philosophy, who implanted the evangelical movement in Cambridge, and who won over Wilberforce to evangelicalism.[2] As a student at Cambridge, Venn had been a close friend of Francis John Hyde Wollaston, who succeeded Milner as Jacksonian Professor and from whom Cavendish obtained Michell's apparatus for weighing the world. But like all members of the Clapham Sect, Venn preferred

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[1] John Pollock (1977, 117–118); Standish Meacham (1964, 27–28); E.M. Forster (1956, 4–9, 26–63); R. de M. Rudolf (1927, 89–90); Michael Hennell (1958, 104–168).

[2] John Gascoigne (1989, 254).

Heaven over Earth, and in a letter of comfort to Wollaston, who had suffered a personal loss, he cautioned him not to immerse himself in science to the detriment of his duty to Christ: "Alas! How little honour it is to be the best chemist in Europe in comparison with being a useful minister of Christ. What comparison can there be between saving a soul and analyzing a salt.... Science and amusement and company are useful in their proper places; you know me too well to think that I would declaim against them in general. It is the abuse of them that prevails at Cambridge—an abuse which renders us careless and insensible upon the verge of eternity."[3] Caring only for his work, Cavendish might well have appeared to Venn guilty of the "abuse" of science, but in their work, Cavendish and Venn were not so far apart, each offering the self in the name of truth. Shy men both, Venn's force of personality derived from his otherworldliness, his faith in eternity; Cavendish's derived from his this-worldliness, which was not without its own form of the eternal, faith in the laws of nature.

Like nature, society was subjected to "experiment" in the eighteenth century. In all good faith, the evangelicals could support a poorly conceived experiment on society: children from central Africa were brought to Clapham, where they were taught to be civilized in the English way. For a time the children roamed the Common, invited into the neighboring houses by their curious owners; unaccustomed to the "rigors of the English climate," most of these children died.[4] The recent French Revolution, Thornton believed, was an "experiment made upon human nature by men insensible of our natural corruption, an experiment by which they expected to show the advantage of a general deliverance from restraint—the superiority of Reason over Revelation. When men are thus left to follow Nature, and are released from their subjection to the laws both of God and of civil society, iniquity will not fail to predominate."[5] The logical outcome of the French Revolution was Napoleon, who was expected to arrive momentarily at Clapham. Evangelicals were not pacifists; Venn published *Reflections in This Season of Danger*, in which he declared that "religion not only permits but enjoins us to defend our property, liberty, and lives against the attacks of violence." The parish was defended by the Clapham militia, commanded by Samuel Thornton.[6] As it turned out, the disturbances of the peace in 1797–98, when Cavendish made his measurements of gravitating matter at his home on the Common, were of the usual kind. The patrol for watching and lighting for the village of Clapham reported that two men were stopped early one Sunday morning in possession of "a bag of cabbages, a pewter pot, and some greenhouse plants."[7]

Benevolence and charity, beloved by the evangelicals, may have meant nothing or little to Cavendish, who reduced them, Wilson said, to a "singular numerical rule." If a person approached Cavendish with a request, he looked over the list for the largest gift, then wrote a check for that amount, no more no less.[8] We have located the lists of Easter offerings from the rector's account book for the years from 1791. In the first year, Cavendish matched the maximum gift on the list, one pound one shilling, but when a neighbor whose gift he had matched raised his gift to five pounds five shillings, Cavendish stayed with his original one pound one shilling; the neighbor's health may have prompted his generosity, for two years

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[3] Hennell (1958, 42, 52–53, 143). Forster (1956, 35–36, 53).

[4] Pollock (1977, 183–184). Hennell (1958, 241–242).

[5] Meacham (1964, 65).

[6] Ibid., 80. Hennell (1958, 215).

[7] "Watching and Lighting Notes, Clapham," 138.

[8] George Wilson (1851, 180–182).

later he was dead.[9] If Cavendish had a rule, he did not apply it foolishly. The charities to which he contributed from January 1806 to January 1807 were the African Association, a cause supported by the evangelicals; magdalen, or reformed prostitutes; asylum; poor people; St. George's Hospital; and St. Giles Charity School. In the last seven months of his life, at his Bedford Square address he gave to forty-eight "poor people," whose names are listed in his porter's account book. If Cavendish's giving was not heartfelt, it was not grudging either.[10]

Banks, Blagden, and Cavendish

In 1768 the Council of the Royal Society accepted the request of the youthful Joseph Banks to accompany Captain Cook on his voyage to the South Seas to observe the transit of Venus the next year. Described in the minutes of the Council as a "gentleman of large fortune, who is well-versed in natural history," Banks came from a family of landed gentry in Lincolnshire with a tradition of public service. On Cook's voyage, he brought with him a company of seven persons, paid for by himself, who included Linnaeus's pupil Daniel Solander.[11]

Banks's assertive presence on Cook's voyage foreshadowed his activity as president of the Royal Society. As patron and administrator, he exerted a remarkable personal force on English science over several decades. Georges Cuvier said of him: "The works which this man leaves behind him occupy a few pages only; their importance is not greatly superior to their extent." Meager as his scientific accomplishment was, Cuvier said, Banks had performed "good service to the cause of Science" in other ways, such as using his influence with men of power.[12] No single activity can summarize Banks's way of serving, but he may have shown himself to best advantage as host of a Sunday salon at his house. Cavendish went faithfully to the tea-drinking-only socials held in the civilized setting of Banks's library. Called by Banks "conversaziones," an elegant word for an English at-home, his salons were distinguished for their regularity, intimacy,[13] and diversity; there London men of science mixed with visitors from out of town and abroad, colleagues, world travelers, and men of fortune and rank. Cavendish as an aristocrat and man of science was welcome on both counts.

Cavendish publicly gave his approval of Banks's presidency during the dissensions in the Society, as we have seen, and implicitly he gave it over the thirty-two years he served in the Royal Society under Banks. Long accustomed to working together in the Society, and to meeting socially at the Sunday conversaziones and elsewhere, Cavendish and Banks were friendly, but not close.

By contrast, Cavendish and Blagden were "intimate,"to use Blagden's word. Someone said of their connection that in the end it did not "suit,"[14] but the break, if that is what it was, appears to have begun as a break between Blagden and Banks, with Cavendish the affected third party. In early 1788 Blagden wrote to Banks that he intended to resign as secretary of

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[9] Untitled Clapham rector's account book, 1791–1842, Lambeth Archives, P/C/26, p. 152.

[10] "Bedford Square. James Fuller's Account with the Exec. of Hen: Cavendish … Settled 30 August 1810," Devon. Coll.

[11] 9 June 1768, Minutes of Council, Royal Society 5:314. George A. Foote (1970, 434).

[12] David Philip Miller (1981, 9, 14–16, 19, 43–47). Hector Charles Cameron (1952, 209).

[13] Timothy Holmes (1898, 46, 68).

[14] Wilson (1851, 129).

the Royal Society, and on the same day he sent a copy of the letter to Cavendish, explaining that he was taking this step to prevent him and Banks from becoming a "violent mixture."[15] Three days later Blagden wrote to Watson, who may have intervened to make peace, that he bore no "ill will" toward Banks and would continue to serve him but would stop "short of an absolute sacrifice" of himself.[16] In early 1789, Blagden told Banks that his secretaryship of the Royal Society was the "great misfortune" of his life, and he referred to his "reflections" on his "connexion" with Banks, which he said he would send later.[17] Banks replied that he had no idea what Blagden was talking about, whether Blagden's complaints were leveled at him or at the world in general. He had thought they were friends but now he feared they were enemies.[18]

Blagden's unhappiness was multiplied by a task Banks had assigned him, to find a method of determining the correct excise duty on alcoholic beverages. For a time the Swiss chemist Johann Caspar Dollfuss had worked on it, but then Dollfuss left, and his experiments were repeated by George Gilpin, clerk of the Royal Society, who then proposed further experiments for Blagden. In this work Blagden was assisted by Cavendish,[19] but it nevertheless cost him much time and effort.

Blagden complained that he should have been paid for this task. Banks replied that he had performed many tasks for the government and never thought of reward, but he would look into it if Blagden would tell him what he expected. Blagden's resentment of Banks had been building. From the time he returned from America, Blagden said, Banks had taken him for granted and deceived him. When he accepted the job of secretary to the Royal Society during the dissensions, Banks made him a "tool of his ambition." Blagden believed that Banks would advance him in society and improve his fortune, but instead he discouraged Blagden from pursuing his profession, medicine, and even from marrying, his only purpose being to keep Blagden dependent on him. Banks defended his character and conduct.[20] Blagden's rancor at Banks continued, as did their correspondence until it became tiresome.[21]

The draft of a letter by Blagden, which was probably written in 1790, gives a clear idea of the extent of his disappointment. The recipient of the letter is Blagden's benefactor, who could be Cavendish though more likely he was Heberden or someone else. To make Banks's "ungenerous, (if not treacherous) conduct the more evident," the letter read, "let me contrast it with your own. You, to whom I had not had any opportunity of being serviceable, seeing how unwisely I neglected my profession, had the goodness not only to advise me to resume it, but likewise to offer that you would bear all the pecuniary risk attending the

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[15] Charles Blagden to Joseph Banks, 2 Feb. 1788. Charles Blagden to Henry Cavendish, 2 Feb. 1788; in Jungnickel and McCormmach (1999, 648–649).

[16] Charles Blagden to William Watson, Jr., 5 Feb. 1788, draft, Blagden Letters, Royal Society 7:115.

[17] Charles Blagden to Joseph Banks, 27 Mar. 1789, BL Add Mss 33272, pp. 56–57.

[18] Joseph Banks to Charles Blagden, n.d. [after 28 Mar. 1789], draft, BL Add Mss 33272, p. 58.

[19] "Remarks by Mr.Cavendish," Blagden Collection, Royal Society, Misc. Notes, no. 65. Charles Blagden to Henry Cavendish, 12 and 26 Mar. 1790; in Jungnickel and McCormmach (1999, 675, 677). Among other assistance, Cavendish made available his father's table of the expansion of water with heat. "From the Experiments of Lord Charles Cavendish, Communicated by Mr. Henry Cavendish. March 1790," Blagden Collection, Royal Society, Misc. Notes, no. 99.

[20] Charles Blagden to Joseph Banks, 28 Mar. 1789, BL Add Mss 33272, p. 59. Joseph Banks to Charles Blagden, 15 July 1789, Blagden Letters, Royal Society, B.39. Charles Blagden to Joseph Banks, 25 July 1789, draft, Blagden Collection, Royal Society, Misc. Matter – Unclassified. Joseph Banks to Charles Blagden, 31 July 1789, Blagden Letters, Royal Society, B.40.

[21] Charles Blagden to Joseph Banks, 27, 28, 29 Mar. and 8 Apr. 1790; Joseph Banks to Charles Blagden, n.d., BL Add Mss 33272, pp. 73–74, 80.

pursuit, so that my private fortune should at all events remain unimpaired. I am sensible how imprudently I acted in not following your advice; but at that time I had still the weakness to believe Sir J[oseph] B[ank]'s professions sincere."[22]

When Blagden considered marriage, Cavendish entered into his plans. In 1789 the potential wife was picked out, Ann Osborne, and in November of that year Blagden asked his brother to inform him about her. Would she enjoy Blagden's kind of company and "particularly would so far enter into the pursuits of my friend Mr. C. as not to think some portion of time spent in his company tedious? This would be a matter of the utmost consequence to us both. You will easily suppose I do not mean that she should enter into our studies, but simply that she should not find it disagreeable to be present when such matters were the subject of conversation, or when any experiment which had nothing offensive in it, was going on."[23] Blagden contemplated the three of them together, Blagden, his wife, and Cavendish. He was concerned about her reaction, not about Cavendish's, calling into question Cavendish's severe misogyny, as described by Wilson.[24] In one of his letters of reproach to Banks, referring to his desire to marry, Blagden said that he "had great reason to believe Mr. Cavendish would assist me in making such a settlement as the family could not properly object to."[25] Banks too had taken into account Blagden's expectations; to justify his use of Blagden services on the problem of excise duties, he told Blagden in the stilted third person way they had adopted in their communications with one another that "as the Dr [Blagden] told me on a former occasion, that if he married Miss Bentinck [another prospect], Cavendish would make ample settlement on him, equal to the wishes of her family, I little suspected that his time and trouble were to be valued by the hour."[26] From the letters of 1789 and 1790, we see that Cavendish was a friend and support to Blagden.

Cavendish is said to have accepted Blagden as his associate on the condition that he give up medicine and devote himself to science.[27] The contrary would seem to have been the case. Blagden reminded Banks that in 1784, he told him that "Mr.Cavendish wished me to prosecute seriously the profession of physic."[28] The year 1784 was two years into Blagden's association with Cavendish. Around this time Blagden wrote plaintively to people about "being now quite out of the practice of physic" and unable to advise on remedies,[29] about being as little familiar with inoculation and other topics of medicine "as if I had never been of the profession."[30] Blagden blamed Banks for encouraging him to abandon his profession and then not advancing and compensating him.

It seems clear that in 1789 Blagden was on good terms with Cavendish and not with Banks. That summer Blagden contemplated going abroad with Henry Temple, second Viscount Palmerston, and his wife, Lady Mary, and possibly staying away the coming winter. His only reservation about that plan was Cavendish's desires: if by being away he would

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[22] Draft of a letter in the Blagden Collection, Royal Society, Misc. Notes, no. 224. Because of the similarity of content and wording to a letter from Blagden to Banks on 8 Apr. 1790, it is probably from around that time. Blagden's comment that he "had not had any opportunity of being serviceable" might seem to rule out Cavendish.

[23] Charles Blagden to John Blagden Hale, 13 Nov. 1789, draft, Blagden Papers, Yale, box 5, folder 49.

[24] Wilson (1851, 169–170).

[25] Blagden to Banks, 8 Apr. 1790.

[26] Banks to Blagden, 27 Mar. 1790.

[27] Henry Brougham (1845, 258).

[28] Blagden to Banks, 8 Apr. 1790.

[29] Charles Blagden to William Farr, 14 Nov. 1785, draft, Blagden Letterbook, Yale.

[30] Charles Blagden to Françoise Delarouche, 1 Dec. 1786, draft, ibid.

hold up Cavendish in any of his pursuits, he would stay. Cavendish raised one objection, which did not have to do with his desires but with Blagden's: being abroad would interfere with what Blagden had "much more at heart than any object in life,"[31] his return to medicine. Blagden thought his chances of practicing medicine at the resorts were as good as in London, and with Cavendish's blessing, he left with the Palmerstons. Before he did, he sold his house and its furnishings on Gower Street, with the thought that he would never again have a permanent address in England. Persons with messages for him were to be directed to Cavendish's house on Bedford Square. His bureau containing private papers was left in Cavendish's bedroom, and Cavendish was given the key and instructed to open the bureau and keep or burn the papers in it if Blagden should suffer an accident.[32] Blagden had recently turned forty and his life seemed without direction, as he set out on yet another Continental journey, evidently with gloomy premonitions.

As it turned out Palmerston did not go on to Italy to spend the winter of 1789–90 as planned, and in the late fall Blagden returned to London to resume his job as secretary of the Royal Society. Out of the turmoil, nothing much changed in Blagden's life, and a surface calm was restored. Banks and Blagden settled for a modus vivendi, but there was an edge to it. Blagden confided in his diary that "Sir JB came at length, & behaved with his usual cunning & falseness, for éclat."[33] He found the "perverseness & jobbing of Sir JB's manner worse than ever."[34] Banks's moral sentiments were "debased," his character "odious."[35] People who meet daily over a long time can irritate one another, but Blagden's censures of Banks are severe and persistent. On his side, Banks could be wounding, as he was when Blagden considered stepping down as secretary of the Royal Society. Blagden had been elected to that post for fourteen successive years, during which time he had ruined his eyes and could no longer read papers at the meetings, but he wanted to leave open the possibility of resuming the job later. Banks told him to forget it because Blagden's "enemies" would bring up his absences on his travels and accuse him of "not cultivating science with the same ardor as you have formally done, owing to the habits you have lately adopted of mixing much in the gay circles of the more elevated ranks of society."[36] Blagden replied with indignation that he had "never performed the office so well" as he had last winter.[37] Blagden resigned for good in the winter of 1797.[38]

From what he could learn, Wilson concluded that Cavendish and Blagden's break "did not occur till at least 1789." We agree; as we note above, as late as November 1789 Blagden was concerned about how his potential wife would react to his work with Cavendish. A year later, Blagden excused himself from a trip he had planned with Palmerston because of "some experiments at Clapham." Cavendish and Blagden continued to be much together,

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[31] Charles Blagden to Henry Cavendish, Aug. 1789, draft, Blagden Letters, Royal Society 7:794.

[32] Charles Blagden to John Blagden Hale, 17 Sep. 1789; "An Inventory of Furniture. Taken September 3. 1789 at Dr Blagden's House in Gower Street Appraised & Sold to Hill Esq.," Gloucestershire Record Office, D 1086, F 155, 157. Charles Blagden to William Lewis, 15 Sep. 1789, draft, Blagden Letters, Royal Society 7:306. Charles Blagden to John Blagden Hale, 16 Sep. 1789, draft, ibid. 7:309. Charles Blagden to Henry Cavendish, 16 Sep. 1789; in Jungnickel and McCormmach (1999, 668–669).

[33] 25 May 1807, Charles Blagden Diary, Royal Society 5:73.

[34] 20 Nov. 1806, ibid. 5:12.

[35] 2 Feb. 1805, 12 Mar. 1807, ibid. 4:307 and 5:46.

[36] Joseph Banks to Charles Blagden, 27 Apr. 1797, Blagden Letters, Royal Society, B.44.

[37] Charles Blagden to Joseph Banks, 27 Apr. 1797, BL Add Mss 33272, 158–159.

[38] He resigned on 30 Nov. 1797. The draft letter of resignation, undated, with no address, begins: "The inflammation of my eyes …." Blagden Collection, Royal Society, Misc. Matter – Unclassified.

but their relationship was less close than it had been. We can safely assume that Cavendish did not want to quarrel with Banks, but this was unlikely the main reason. Thomson said that Blagden "left him."[39] Blagden wanted to make changes in his life, which first of all had to do with his obligations to Banks, and a reduction in his obligations to Cavendish may have been included. In late 1789, while he still fully intended to continue his association with Cavendish, he described his financial situation to his brother to pass along to his prospective wife: his stocks were worth between 9 and 10,000 pounds, and his income was 250 pounds a year from his half pay in the military and his secretaryship of the Royal Society; he was "not without other expectations, but of these nothing can be said."[40] Cavendish did not contribute to Blagden's income, but Blagden's "expectations" probably had to do with him in the event that he married. As it turned out, Blagden did not marry, and Cavendish consequently would not have entered his subsequent plans. Another consideration was that Cavendish was not as busy as he had been and his having less need for Blagden's help, their separation may have been mutually desired. About the personal side of their association we know little; their natures being very different in some ways, it is possible that their collaboration was trying. If his relationship with Cavendish eventually did not suit, Blagden's regard for Cavendish did not change. Writing to Banks from Paris in 1802, Blagden compared Cavendish with "Laplace, who is as much superior among them here as Mr. Cavendish is with us."[41]

The Duchess and the Philosopher

Through the Devonshire and the Kent dukedoms, Cavendish had an enduring connection with the world independently of Blagden, Banks, and his other scientific colleagues. For most of his adult life, the head of the Cavendish family was William, fifth duke of Devonshire. From Chatsworth, Thomas Knowlton wrote to the naturalist John Ellis, "I wish that our Duke [the fifth duke was twenty-two] would, like his father, who every day improved in knowledge, take a turn that way."[42] The young fifth duke would continue not to be like his father, who had been a self-improving man with a highly developed sense of service, one of the most respected British statesmen of the eighteenth century. The fifth duke was the first of the dukes of Devonshire resolutely to turn his back on politics. He had that much in common with Henry Cavendish, in whom the absence of political desire was clearly an asset in his chosen life. The fifth duke had other traits in common with Henry: he was intelligent, reclusive, awkward, and indifferent to religion, but here the resemblance ends. Since little individual exertion was required of the duke, he made little, preferring to lie in bed until the middle of the afternoon and then to get up only to go to his club. He was dissolute, unfaithful, and, in his dedicated passivity, fascinating. He disapproved of Henry Cavendish, as we have noted, because "*he works*."[43] When Henry Cavendish died, the duke took a passing interest in the inheritance. The duke lived only one year beyond his working relative.

The fifth duke and his (first) wife, Lady Georgiana Spencer, had this in common: like their great friend Charles James Fox, they were both prodigal gamblers.[44] Otherwise the

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[39] Thomas Thomson (1830–1831, 1:338). Wilson (1851, 129). Charles Blagden to Henry Temple, Lord Palmerston, 8 Oct. 1790, draft, Blagden Papers, Yale, box 63/43.

[40] Charles Blagden to John Blagden Hale, 13 Nov. 1789.

[41] Charles Blagden to Joseph Banks, 1 Apr. 1802, BL Add Mss 33272, 172–173.

[42] Thomas Knowlton to John Ellis, Oct. 1770, in James Edward Smith (1821, 2:79).

[43] John Pearson (1983, 122–123). Francis Bickley (1911, 202).

[44] Hugh Stokes (1917, 283–288).

duchess was the duke's temperamental opposite, vivacious, enthusiastic, charming, "her animal spirits were excessive," whereas the duke, by contrast, was said to be a simile for winter.[45] Like the Cavendishes, the Spencers had sided with the victorious party in the Revolution of 1688–89 and with greater interest in politics than her husband, the duchess actively supported Fox and his followers. Known as the queen of London fashion, she also had an avid if unfocused interest in music, literature, history, and science. With Giardini, she studied music;[46] with a "Philosopher," she studied the globes, buying two for herself from the instrument maker George Adams;[47] with a "German," she studied chemistry and mineralogy;[48] with Blagden she exchanged scientific news; and she took a keen interest in her cousin-in-law Henry Cavendish. When writing to the duchess, Blagden referred to "our friend Mr. Cavendish."[49]

Figure 17.1: Georgiana (Spencer), Duchess of Devonshire. By Joshusa Reynolds. Reproduced by permission of the Chatsworth Settlement Trustees.

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[45] Mary Robinson (n.d. 301).

[46] Bickley (1911, 241).

[47] Georgiana Cavendish, duchess of Devonshire to Countess Spencer, 11 Jan. 1783, Devon. Coll.

[48] Charles Blagden to Henry Temple, Lord Palmerston, 21 Feb. 1794, draft, Osborn Collection, Yale, box 63/43.

[49] Charles Blagden to Georgiana Cavendish, duchess of Devonshire, 4 Jan. and 6 Mar. 1794, Devon. Coll.

From abroad, the duchess asked Blagden to tell her about "any chemical, mineralogical, or philosophical novelty" and to give her compliments to Cavendish,[50] and when she and Blagden happened to meet abroad, they spent an evening with "much talk about chemistry & mineralogy." Blagden noted in his diary: "Dss of Devonshire said she was quite wild with studies of that nature: asked much about Mr. Cavendish & his pursuits"; "much talk with the Dss about Sir Jos. Banks's meetings, Mr. Cavendish."[51] The duchess called on Cavendish at his house,[52] and Cavendish called on her, often, it is said. Once when Blagden came to see her at Devonshire House, he found Cavendish there engaged in scientific talk.[53] In wanting to be informed about scientific advances and about Henry Cavendish's activities, the duchess overcame his shyness and his alleged misogyny. To have his company she had only to keep to his subject, science, her lively curiosity no doubt doing the rest.

Unpublished Work

In his later years, Cavendish worked on nearly all of the subjects he had in his early years, though the proportions changed. Astronomy was now prominent. Part observational and larger part mathematical, his astronomical papers make up a large share of his scientific manuscripts. The papers sometimes begin as carefully drafted studies with a clear objective but then trail off into calculations of unclear significance, and other times they have a finished quality, meant to be shown to someone. Cavendish did not make systematic observations of the sky as Maskelyne and Herschel did—he did not have that kind of observatory and he did not spend his time that way—but he made observations from time to time to test techniques such as taking transits, and he looked at things that other astronomers looked at, a planet, a comet, a variable star, and volcanoes on the moon.[54] As in other areas of science, in astronomy he took a sustained interest in instruments, methods, and errors of observation. In this section, we look at three examples of unpublished work from, or bearing on, astronomy: an astronomical instrument, orbits of comets, and refraction and dispersion of light.

Around London there was a series of observatories roughly following the course of the River Thames. Cavendish's Observatory at Clapham Common was directly south of London, and on a line with it to the east were Aubert's observatory at Loam Pit and just beyond that Maskelyne's Royal Observatory at Greenwich. Considerably to the west of this

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[50] Georgiana Cavendish, duchess of Devonshire to Charles Blagden, 4 Mar. 1794, Blagden Letters, Royal Society, D.61.

[51] G. De Beer (1950, 76, 80, 83).

[52] Once when she called on Cavendish, his servant told her he was unwell, and she asked Blagden to find out how he was. Charles Blagden to Joseph Banks, 11 Aug. 1795, BL Add Mss 33272, 143. It was not an excuse: Blagden called on Cavendish later that month and found him "decaying: his forehead healing not kindly." 27 Aug. 1795, Charles Blagden Diary, Royal Society 3:67.

[53] 1 Sep. 1794, Charles Blagden Diary, Royal Society 3:14. Cavendish may have acted as a tutor to the duchess: when Blagden arrived at her house, he found "Mr. Cav. there; saw none had notes." The duchess proposed that Cavendish "shew extracts from Js de Physique." On 27 Nov. 1794, Blagden again came across Cavendish at the duchess's: "Met Mr. Cav. there: pleasant talk." Ibid., 33(back).

[54] Herschel observed what he regarded as a volvanic eruption on the moon, shining with a fiery light, and he observed two "extinct" volcanoes as well; he came to his conclusion about what he saw "by analogy, or with the eye of reason." With a telescope, Cavendish and Blagden observed the unusual light in the dark area of the moon where Herschel thought he had located a big volcano. William Herschel (1787). Charles Blagden to Mrs. Grey, 14 June 1787, draft, Blagden Letterbook, Royal Society 7:324.

group was Herschel's observatory. Cavendish knew these observers well, as he did another astronomer Michell, who did not live in London. We should get to know them.

In 1781 William Herschel discovered a new major planet, the first to have done so since antiquity, naming it after George III (it was renamed Uranus), who rewarded him with a royal pension, freeing him from his original profession, music; the same year he was elected to the Royal Society. (Fig. 12.6). He settled near Windsor Castle, where he made observations at night and telescopes by day, which he sold to supplement his pension or used himself. The biggest of his telescopes was a reflector of (for that time) unprecedented proportions, four feet across and forty feet in length—Blagden walked through the iron tube of this telescope hardly having to stoop[55]–its dimensions being a proper measure of his ambition, which was to see to the ends of the universe and to survey its contents. From his systematic sweeps of the sky, he identified over 800 double stars and 2500 nebulas of all kinds. He published sixty-nine papers in the *Philosophical Transactions*, laying the foundations of stellar astronomy. His achievement was the result of patient application, excellent instruments, masterly observation, and imaginative theorizing, a rare combination in any science.[56] Seven years younger than Cavendish, he interacted with Cavendish, though probably not often.

At the Royal Society Club, John Playfair found that members paid little attention to guests, of whom he was one. The exception was Alexander Aubert, whom Playfair found "a very polite man, and a great consolation to a stranger."[57] (Fig. 12.3). This detail captures a truth about Aubert: he was observant and helpful. He seemed to have had no personal ambition in astronomy, only a passion for it and a standard of excellence. Equipping his observatories with instruments by the leading instrument makers, Jesse Ramsden, Peter Dolland, John Bird, and James Short, he had "the best set of astronomical instruments that belongs, perhaps, to any private man."[58] Because of the quality of his instruments, Herschel asked him to confirm his own observations so that they would be taken seriously by other astronomers.[59] He was a director and from 1787 governor of the London Assurance Company, administrative experience which he brought to his learned side pursuits. A fellow of the Royal Society since 1772, he was elected to the Council and appointed to committees for astronomy and meteorology, on which he served regularly with, and almost as often as, Cavendish. In 1778, the Council considered two members to replace the outgoing president, Aubert and Banks, and after long deliberation they made their fateful choice of Banks.[60] Afterwards it was asked "what Mr. Aubert had done."[61] He published very little.[62] He and

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[55] Charles Blagden to John Michell, 31 Oct. 1786, draft; in McCormmach (2012, 413).

[56] Michael A. Hoskin (1963, 17–18, 62–64). "Herschel, Sir William (1738–1822)," *DNB*, 1st ed. 9:719–725.

[57] Playfair quoted in Geikie (1917, 160).

[58] Playfair, quoted ibid., 159. In the 1780s Aubert's astronomical establishment was "except that of Count Brűhl […] the only well-equipped private establishment of the kind in England." "Aubert, Alexander (1730–1805)," *DNB*, 1st ed. 1:715. "Brűhl, John Maurice, Count of (1736–1809)," ibid. 3:141.

[59] William Herschel to Alexander Aubert, 9 Jan. 1782, copy, Royal Astronomical Society, Herschel W1/1, 21–24; published in Constance A. Lubbock (1933, 102–103).

[60] Henry Lyons (1944, 197).

[61] Ibid. Edward Smith (1911, 56–57).

[62] Over the course of his long activity in astronomy, he published three papers on the transit of Venus in 1769, a new method of finding time by equal altitudes in 1776, and meteors in 1783, all brief and all appearing in the *Philosophical Transactions.*

Cavendish were the same age and saw each other regularly at their clubs. Cavendish brought Aubert into his financial affairs as a trustee of his property at Clapham Common.[63]

Cavendish saw Nevil Maskelyne often and in the same places he saw Aubert, at the Royal Society and at their clubs (Fig. 12.5). Maskelyne offered Cavendish what Herschel and Aubert could not; not only was he a fine observer and skilled with instruments, he was highly competent in mathematics, evident from memoranda that passed between him and Cavendish. He probably met Cavendish while a student in Cambridge. After graduation, he was ordained to a curacy, and about the same time he was elected fellow of his college, Trinity. Two years before Cavendish, he was elected to the Royal Society, where most of their collaboration took place. Early on he assisted the astronomer royal James Bradley in computations, and with Bradley's help he was sent abroad by the Royal Society to observe the transit of Venus in 1761. In 1765 he became the fifth astronomer royal, replacing Nathaniel Bliss, who died after only two years. Under the first three astronomers royal, John Flamsteed, Edmond Halley, and Bradley, the Royal Observatory at Greenwich held a leading position among European observatories. That no longer could be said, but Maskelyne oversaw an important change in the way the Observatory was used: whereas past astronomers royal kept their observations more or less to themselves, beginning with Maskelyne, observations made at the Observatory became public property. From the Royal Society he received a fund to publish his observations, which appeared in four volumes between 1776 and 1811. In 1766 he brought out the first number of the *Nautical Almanac* (for 1767), which he continued for forty-four years until his death, thought to be his most important work. He championed the lunar method of determining longitude at sea, which used tables and a sextant for measuring the distances of certain stars from the moon. He published frequently in the *Philosophical Transactions*, always on subjects related to astronomy. Playfair said that Maskelyne was "slow in apprehending new truths, but his mind takes a very firm hold of them at last." According to a French visitor at the Observatory, Maskelyne had a "politeness and a *complaissance* that scholars of his rank don't always have *pour des Passans*." His methodic exactness and his devotion to astronomy suited Cavendish, and their two temperaments were compatible.[64]

If Cavendish did not meet Michell in Cambridge, where Michell was a fellow of Queens' College when he was a student at Peterhouse, he met him in London no later than 1760, the year both of them were elected to the Royal Society. That same year, at Cavendish's first dinner as a member of the Royal Society Club, Michell was present as a guest,[65] and in later years Cavendish brought Michell as his own guest. Like Cavendish, Michell was a natural philosopher, though his main publications were in geology and astronomy. In theoretical inventiveness, he was Herschel's equal, and he had mathematical skills comparable to Cavendish's and Maskelyne's. In mid-life he resigned his fellowship in Cambridge to become a country pastor. To keep up contact with men of science, he regularly made the long journey from his parish in Yorkshire to London. His one known

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[63] In a bundle of papers dealing with Cavendish's Clapham Common property are extracts from Aubert's and Aubert's heirs' wills. They were assembled to transfer the property to the duke of Devonshire. Devon. Coll., L/38/78.

[64] Maskelyne's obituary, *Gentleman's Magazine* 81:1 (1811): 197, 672. Playfair (1822, 1:lxxix; Appendix, No. 1, "Journal"). "Maskelyne, Nevil (1732–1811)," *DNB*, 1st ed. 12:1299–1301.

[65] 14 Aug. 1760, Minute Book of the Royal Society Club, Royal Society, 4.

sustained correspondence was with Cavendish, a continuation of a conversation from his last visit to London.[66]

Hardly had Cavendish settled into his new house on Clapham Common than he took the first step toward erecting a telescope of 123-foot focal length made by Constantine Huygens, brother of Christiaan, who also made telescopes. Constantine, who was then secretary to King William III, presented the telescope to the Royal Society in 1691.[67] Besides this telescope, the Royal Society later acquired two more object-glasses made by Constantine of even greater focal length, 170 feet and 210 feet. Evidently borrowing all three, Cavendish definitely tried the 123-foot telescope and probably the 210-foot one.[68] The incentive to build telescopes of such long focal lengths was to reduce aberrations and to achieve high magnification.[69] Christiaan Huygens is usually given credit for introducing the so-called "aerial" telescopes, which dispensed with unwieldy rigid tubes for mounting the object-and eye-glasses, making possible telescopes with much longer focal lengths. Not until John Hadley built a Newtonian reflecting telescope with a parabolic mirror in 1721 did astronomers know of any practical way to minimize aberrations other than by lengthening their telescopes, ultimately a dead end.[70]

Christiaan Huygens's account of an aerial telescope was published in the *Philosophical Transactions* in 1684. To dispense with the "heaviness and disproportion" of the telescope tube, Huygens cut out "almost the whole tube, saving only a small part of it near the objective glass, and somewhat towards the Eye glass, ordering these two extremities in such a manner, that they may do the same service, as if the whole tube of one piece should be employed." He described a fifty-foot mast for erecting an aerial telescope of seventy-foot focal length, a stand for steadying the observer's arms, a lantern for illuminating the object-glass so that it could be found at night, and a cord for aligning the eye-glass and the object-glass.[71] Never

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[66] Material on Michell's life, in McCormmach (2012).

[67] The focal length has been stated variously as 120, 122, 123, and 126 feet, as has its aperture, $6, 7\frac{1}{2}, 7\frac{7}{8}$ inches. R.A. Sampson and A.E. Conrady (1928–1929, 289, 291).

[68] The Journal Book of the Royal Society said that Christiaan Huygens made the telescopes: 7 Jan. 1742, JB 13:4334. Sampson and Conrady give the reason for attributing them instead to the brother, Constatine. "Three Huygens Lenses" (Sampson and Conrady 1928–1929, 292). When Cavendish returned the telescopes he included his apparatus. Ibid., 289.

[69] Any increase in magnification comes at a high price, for the length of a telescope increases faster than the magnification: to double the magnification, the length has to be quadrupled; to triple it, the length has to be increased ninefold. The 123-foot Huygens telescope has a magnification of 218. William Kitchener (1825, 22). The very slight curvature of the long focal length lens greatly reduces spherical aberration, and chromatic aberration is practically eliminated for the following reason. The telescope consists of two lenses, neither of which is achromatic, but if the two lenses are made of glass of the same dispersion and the telescope is focused at infinitely distant objects such as stars, the angular magnification for any given color depends only on the curvature of the lenses and not on the refractive index. The workmanship on the Huygens lenses was of high quality, but not the glass, which compares poorly with the cheapest bottle or window glass. The tangle of fine veins in the glass made the refraction irregular. The glass available to Huygens resulted in a poor definition of images, as Cavendish no doubt determined. Sampson and Conrady (1928–1929, 298–299).

[70] Newton's other early reflecting telescopes had spherical mirrors, which were subject to spherical aberration. Astronomers knew that to achieve sharp images, the mirrors needed to be parabolic, but they were hard to make. Hadley's first telescope with a mirror of 6 inches diameter and a length of 6 feet worked almost as well as Huygens's 123-foot aerial telescope.

[71] Huygens explained the working of the aerial telescope. The observer stood resting his arms on a light frame or hurdle and holding the eyepiece (concentric, adjustable metal tubes containing the eye-glass) by the handle. A cord connected it to a short board on which the object glass was mounted at one end and a counterpoise at the other. By tension on the cord the observer could bring the two lenses into parallel. Christiaan Huygens (1684). Smith (1952, 354). Sampson and Conrady (1928–1929, 298).

very popular in Britain, the aerial telescope was hard to manage, and on dark nights the object-glass was difficult to see without artificial light, alllowing stray light to enter the eye-glass. The alternative, a telescope of long focal length that came with sliding tubes, was also hard to use, affected by wind and vibration.[72] The Royal Society considered fixing the Huygens telescope to a tall, solid building, but they could not settle on any tall or solid enough. Halley was ordered to consider the scaffolding of St. Paul's Cathedral. James Pound mounted the telescope on a maypole removed from the Strand and relocated in Wanstead Park, where he and Bradley made successful use of it. Pound made improvements on its "furniture and Apparatus," the most important of which was a micrometer, which gave the Huygens telescope its one advantage over the Newtonian: the longer the telescope, the larger the image, and the micrometer measures a large image more accurately than a small one. The telescope was borrowed again by William Derham, who returned it in 1741, having made no observations: "The chief inconvenience is the want of a long pole of 100 or more feet, to raise my long glass to such a height as to see the heavenly bodies above the thick vapours," and he was told that this would cost him eighty or ninety pounds, which were beyond his means. In 1748 Charles Cavendish together with Jones, Folkes, and Graham brought the Huygens lenses from the Royal Society to Macclesfield's Shirburn Castle to try it.[73] The telescope worked fine: a visitor who went to Shirburn Castle to look at Jupiter through it saw "that bright planet in perfection."[74] In 1778 Maskelyne borrowed the longer 210-foot Huygens telescope.[75]

At this juncture, Henry Cavendish enters the history of Huygens's telescopes. In November 1785 the Council of the Royal Society gave him permission to borrow the 123-foot telescope and also the other Huygens object-glasses, which he kept for three years. Among Cavendish's manuscripts is a study by him of a ship's mast, which we take to be the mount for the Huygens telescope. It begins with fundamentals: "According to Newton the resistance of wind to a globe is equal to […] and therefore if wind is 60 miles per hour…." To judge from his calculations—he determined the pressure of wind on two cylinders of unequal diameters each 40 feet in length—the Huygens telescope was erected on a wooden mast 80 feet high, supported by 20-foot struts planted 11 feet from the base. A horizontal piece was fixed to the mast.[76] Well secured, the mast remained in place long after Cavendish died, identified in a description of his property this way: "In a padlock at the back of the house is a mast of a ship, erected for the purpose of making philosophical experiments."[77] The mast towered above Cavendish's house as if it were the home of a nostalgic man of the sea.

In March 1786 Aubert told Herschel that after half a year, Cavendish still had not tried the Huygens lenses on objects on land, but he was busy preparing the apparatus for trying them on celestial objects.[78] In June Blagden told Berthollet that Cavendish was ready to "make a trial of the old aerial telescopes," and that Herschel looked forward to the trial for

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[72] A.J. Meadows (1970, 307).

[73] Smith (1738, 2:354, 440). R.S. Rigaud (1832, ix, lx, lxxxiv). 20 June 1728, JB, Royal Society 13:237. 10 and 29 Aug. 1748, Minutes of Council, Royal Society 4:5–8. King (1955, 63). Charles Yorke to Philip Yorke, 23 Aug. 1748, BL Add Mss 35360, f. 185. Thomas Birch to Philip Yorke, 18 Aug. 1748, BL Add Mss 35, 397.

[74] Catherine Talbot to Elizabeth Carter, 10 Oct. 1748, in Carter (1809, 1:293–294).

[75] 10 Dec. 1778, Minutes of Council, Royal Society 5:369.

[76] The computations for the mast are in Cavendish Mss, Misc. Robert Smith (1738, 2:355).

[77] Burgess (1929, 57).

[78] Alexander Aubert to William Herschel, 23 Mar. 1786, Royal Astronomical Society, Mss Herschel W 1/13, A23.

"comparing the effect with that of his large reflectors."[79] Blagden thought that the 200-plus-foot telescope would probably be found inferior to Herschel's big reflectors, but still it was "desirable to form a just estimate of the tools with which our ancestors worked."[80] Herschel came to Clapham Common to participate in the trial, as did the instrument maker Peter Dolland, whose father, John, had shown how to eliminate one of the major aberrations (chromatic) of telescopes. They found that the "Dwarf," a forty-six-inch triple-lens achromatic refractor (either Dolland's or Cavendish's), was "fairly a match for the [123-foot] Giant."[81] Cavendish evidently was the last person to mount Huygens's telescopes for making observations, though the lenses continued to draw interest.[82]

From the 1780s Cavendish devoted a large body of work to the orbits of comets, beginning with the "comet" discovered by Herschel in 1781. Cavendish made computations from observations by Maskelyne and the Oxford astronomer Thomas Hornsby, who resisted calling it a "planet" (it was, in fact, Uranus).[83] Cavendish's study of comets proper began with observations by Caroline Herschel, who assisted her brother William at the observatory. When he was away she made sweeps of the sky herself, in the course of which she became a proficient discoverer of comets, eight in all. Blagden at the Royal Society was informed directly by her and indirectly by Aubert of her first comet, in 1786. Blagden used the occasion of an inspection of the Greenwich Observatory to announce her discovery to the assembled astronomers. Banks with some friends planned to visit Caroline Herschel and see the comet for themselves.[84] When she discovered her next comet, Cavendish made observations of it.[85] His interest in comets was directed to two problems, which were connected: one was methods of computing their paths, the other was computing deviations of their paths from perfect conic sections, analogous to computing errors, a regular activity of Cavendish's.

Newton showed that a comet moves on a parabolic path, which in the case of a returning comet coincides with a highly eccentric ellipse. In principle three observations determine the elements of the path, but in practice it was a difficult problem for astronomers. A forty-year-old method by Boscovich had recently been rejected by Laplace, leading to an acrimonious dispute, and capturing the attention of calculators. As a test of their methods, and of their skill, astronomers looked forward to the return in late 1788 or early 1789 of the great comet observed in 1532 and 1661.[86] The mathematical problem was to find the distortion of the

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[79] Charles Blagden to C.L. Berthollet, draft, 5 June 1786, Blagden Letterbook, Royal Society 7:2.

[80] Charles Blagden to Benjamin Thompson, draft, 7 July 1786, Blagden Letterbook, Royal Society 7.

[81] This is what Dolland told Kitchener (1825, 22).

[82] Out of historical curiosity, the astronomer W.H. Smyth considered setting up the telescope again, around 1835: "I was so puzzled to know how they contrived to get the eye and object-glasses of these unwieldy machines *married*, or brought parallel to each other for perfect vision, and so desirous of comparing the performance of one of them, that I was about to ask the Royal Society's permission to erect the aerial 123-foot telescope in their possession. The trouble, however, promised to be so much greater than the object appeared to justify, that I laid the project aside." Quoted in Weld (1848, 1:331). In 1929 Sampson and Conrady examined the two Huygens lenses of longer focal lengths. They used an interferometer to determine the focal lengths and again to determine the radii of curvature, since the extreme shallowness of curvature of the long-focal-length lenses precluded the use of a spherometer. Sampson and Conrady (1928–1929, 294–297).

[83] Supported by Cavendish's computations, Hornsby thought that Herschel's observations were in error. Herschel thought otherwise. Thomas Hornsby to William Herschel, 26 Feb. 1782; William Herschel, "Memorandum for Mr.Cavendish," in Lubbock (1933, 106–107).

[84] Charles Blagden to Claude Louis Berthollet and to Benjamin Thompson, 4 Aug. 1786, draft, and to Caroline Herschel, 5 Aug. 1786, draft, Blagden Letters, Royal Society 7:18–20. Caroline Herschel (1786).

[85] Henry Cavendish, "Miss Herschels Comet," Cavendish Mss VIII, 37. This was the 1788 comet.

[86] Charles C. Gillispie (1978, 309–310).

path of the comet when it passed the large planets Jupiter and Saturn on its way out of the solar system, affecting the timing of its return. The Royal Academy of Sciences at Paris announced a prize for the best solution. Maskelyne published a paper "to assist astronomers in looking out for this comet." Cavendish corresponded with Maskelyne about it, made computations on the comet of 1532, and wrote a paper on how to compute the return of a comet whose path is altered by the attraction of planets.[87]

In December 1788, while looking for the expected return of the great comet (it failed to return), Caroline Herschel discovered a faint comet, her second. Evidently with this in mind, Cavendish wrote a substantial paper laying out his method for computing the orbits of comets, both parabolic and an elliptical, from three observations. His method made use of a globe covered in white paper on which the ecliptic and various circles and points were drawn. He gave his study of comets' orbits to Maskelyne, who suggested a planisphere made by Adams in place of Cavendish's globe. Along with this and other comments on Cavendish's paper, Maskelyne sent him the observations he had requested, those for Caroline Herschel's recent comet, the orbit of which he wanted to compute using his method.[88] (Fig. 17.2). In due course Cavendish wrote to Maskelyne that he had been "so much taken up about this & other matters" that he had not been able to study his comments on his method. He said that up to this point the method caused "rather more trouble than I imagined it would be before I tried it but on the whole seems as if it would prove an useful method especially if proper tables were made which if I knew of any one that I could employ to compute them I would get done." He wrote a paper on the disturbance of a comet's orbit in passing planets,[89] a variation of the problem of the alteration of the orbit of a planet by another planet, which he also worked on.[90]

Years later, Cavendish returned to comets to make lengthy studies of methods of computing their orbits[91] and to compute the path of the first of two comets discovered by the French astronomer Pierre Méchain in 1799.[92] After pointing out a small error in a logarithm, Cavendish told Maskelyne that if the correction were made, he believed that his orbit "would be found to agree very nearly with observation." He thought that it might seem extraordinary that the results came out so accurate, but he explained how that must happen.

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[87] Henry Cavendish, "Comet, 1532"; "In Order to Compute the Return of a Comet," Cavendish Mss VIII, 38, 39. Nevil Maskelyne (1786, 429). Charles Blagden to Mrs. Grey, 5 Oct. 1786, draft, Blagden Letters, Royal Society 7:39.

[88] Nevil Maskelyne, "Remarks on Mr. Cavendish Paper on Finding the Orbit of a Comet," 16 Apr. 1789, enclosed in Henry Cavendish, "Method of Finding Comets Orbit Fair," Cavendish Mss VIII, 43; in Jungnickel and McCormmach (1999, 662).

[89] Henry Cavendish to Nevil Maskelyne, [after 16 to April 1789], in Jungnickel and McCormmach (1999, 664). Henry Cavendish, "On the Alteration Produced in Comets Orbit by Attraction of ⊕," Cavendish Mss VIII, 52; "Written for Person Thought of for Calculating Perturbation of Expected Comet," ibid., 53.

[90] Henry Cavendish, "To Find the Alterat. Produced in the Elements of a Planetary Orbit by a Small Alteration in Its Velocity & Direction," Cavendish Mss, Misc.

[91] Henry Cavendish, "To Find Whether 2 Parabolic Orbits Can Be Drawn So as to Agree with Observation." This concerns the question whether or not more than one parabolic orbit can be drawn through three points and other matters pertaining to comets. It is written partly on paper carrying the watermark 1797, which he was still using in 1799. Cavendish Mss VIII, 40. Another paper written partly on paper carrying the watermark 1797, but also partly on paper with watermarks 1802 and 1804, which may mean that it was written at different times, is Henry Cavendish, "Boscovics Method of Finding the Orbit of a Comet," Cavendish Mss VIII, 50. The next paper is undated, but since Cavendish drew on Boscovich for his study of the comet of 1799, it may belong to that time: "Example of Computing Orbit on Bosc. Principle without Graphical Operat.," Ibid. VIII, 42.

[92] Henry Cavendish, "Comet of 1799 Computed by the Table for Boscovic's Sagitta"; "Comet of 1799"; "Computation of Comet of 1799 by Fluxional Process," Cavendish Mss VIII, 44, 46, 47.

He used Boscovich's graphical method, which he thought had little error in it. (He found Laplace's method wanting.) "But I have tired myself too much with the former comp. to do any more," he said.[93]

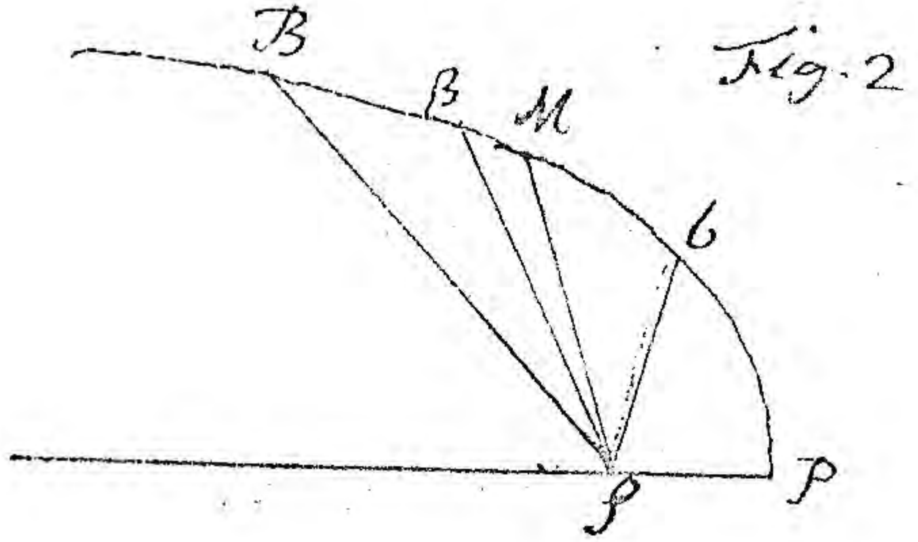

Figure 17.2: Comets Orbit. BbP is the orbit of a comet, S is the Sun, P is the perihelion, and B and b are the locations of the comet at the two extreme observations. "Method of Finding, Orbits Fair," Cavendish Mss VIII, 43. Reproduced by permission of the Chatsworth Settlement Trustees.

The very considerable labor Cavendish devoted to the paths of comets can be understood as a response to problems astronomers addressed at the time. There may have been an additional incentive too. Once regarded as transient phenomena of the atmosphere, comets were one of the triumphs of the Newtonian world. These seemingly capriciously appearing objects were found to be subject to the force of gravitation and therefore to theoretical calculation and prediction.[94] They recall the earliest record we have of Cavendish's thoughts, the poem from his Cambridge years: nature may mock us, but "She does lay bare hidden causes/ And the wandering paths of the stars." Cavendish's study of comets' paths in his later years may be seen as a vindication of that thought (and, perhaps, of his calling).

The final unpublished work of Cavendish's we consider belongs to optics. Among his papers we find a copy of a letter written by the astronomer William Ludlam about a manuscript of a text on optics, which he was critical of. The author left out Dolland's dis-

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[93] Henry Cavendish, "La Places Method," Cavendish Mss VIII, 41. Henry Cavendish to Nevil Maskelyne, [Oct. 1799], draft, Cavendish Mss VIII, 46; partially reproduced in Jungnickel and McCormmach (1999, 720).

[94] A. Wolf (1961, 159–160).

covery and the related doctrine of aberrations, “the most difficult as well as the most important part of optics.” Ludlam cited Experiment 8 in Newton’s *Opticks*, on which Newton based a dispersion law implying that all further improvement in refracting telescopes other than for increase in length was impossible. Ludlam attributed the over-fifty-years’ delay before John Dolland discovered the error to the indolence of man or to the difficulty of experiments. No experiments had been made after Dolland’s, and there needed to be, Ludlam said, setting bounds to the further improvement of Dolland’s lenses.[95] Cavendish and Ludlam were acquainted, Cavendish having brought him as his guest to the Royal Society and the Royal Society Club. Cavendish must have considered Ludlam’s letter sensible, since he kept it among his papers. It would seem that Cavendish agreed with him, as his researches in optics were mainly about aberration.[96]

Dolland repeated Newton’s experiment, finding both the experiment and Newton’s dispersion law wrong. With a double prism of glass and water, and with an adjustment of the angle of the water prism, he was able to achieve refraction without dispersion into prismatic colors. With further experiments with prisms, he found that by combining two kinds of glass with different powers of dispersion in the right proportion, he could again obtain refraction without dispersion. The success with prisms carried over to lenses, enabling Dolland to build an achromatic telescope using a compound lens of flint glass and crown glass, or ordinary window glass. A significant advance in astronomy was implicit in Dolland’s telescope, though its realization waited for improvements in glass, especially in flint glass. Through the last half of the eighteenth century, achromatic telescopes with lenses over five inches in diameter were unknown owing to the poor quality of the glass. The defect was overcome by the Swiss watchmaker and optician Pierre Louis Guinard, who for twenty years experimented with casting methods with the goal of freeing glass from defects. He had only limited success until 1805 when he joined the firm of Fraunhofer and Utschneider in Munich, where his method was perfected. Fraunhofer improved achromatic telescopes to where they rivaled the best reflecting telescopes.[97]

On a tour in Switzerland, Blagden met Guinard, who gave him a small piece of his flint glass, which he said had much greater refractive and dispersive power than common flint glass, and which moreover was free from veins. When Blagden returned, the fragment of glass was ground into a prism and given to Cavendish, who weighed it, finding its specific gravity larger than that of common flint glass.[98] He evidently followed this up with a series of experimental and mathematical researches in optics, begun in February 1789 and continuing into October. Because he did not write up a paper for publication or for a colleague, we have only his laboratory record, to which someone other than Cavendish gave the title “On Rays of Different Colours Transmitted through Prisms of Different Materials.”

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[95] “Mr. Ludlam’s Acct of Mr. Harris Ms.,” Cavendish Mss V, 3.

[96] Examples are: Henry Cavendish, “On the Aberration in Reflecting Telescope Used in Herschels Manner”; “On the Aberration of Rays Passing through Spherical Lens,” Cavendish Mss V, 10, 11.

[97] C.S. Hastings (1891, 344–345). H.C. King (1948).

[98] Charles Blagden to M.A. Pictet, 9 Apr. 1789, Blagden Letters, Royal Society 7:223. We assume that Cavendish weighed the glass after it was ground into a prism, but it could have been before. See next footnote.

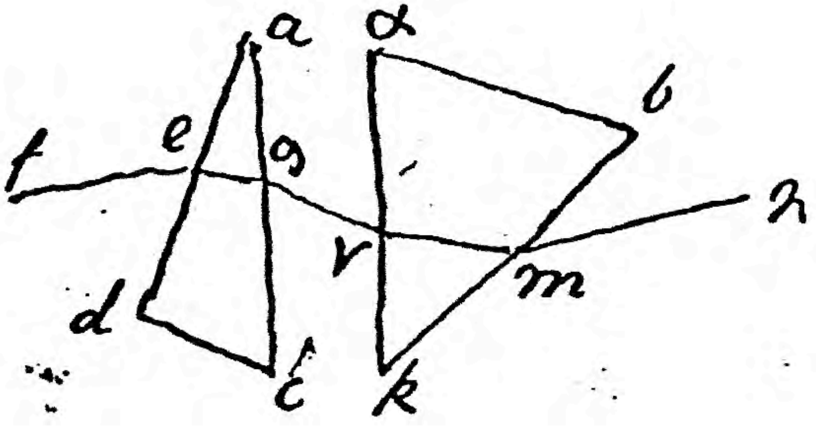

Figure 17.3: Compound Prism. The prism S is made of Swiss glass, the prism F of flint glass. Cavendish compared the refractions of the two prisms using colored light, red and blue. Cavendish Mss V, 4. Reproduced by permission of the Chatsworth Settlement Trustees.

Cavendish's first experiment was a comparison of a prism of "glass from Switzerland" with a prism of ordinary flint glass (Fig. 17.3). Given the timing we suppose that the glass from Switzerland is the one Blagden received from Guinard.[99] In the experiment, rays from the two extremes of the spectrum, red and blue, were directed through the two prisms, which were pressed together, and projected on a board, where one inch corresponded to seventy minutes of arc. Repeating the trial using flint glass and crown glass, Cavendish found the dispersion—the separation of the red and blue rays—of the flint prism by itself to be 91.7 minutes, of the crown prism by itself to be 58.1 minutes, and of the compound prism to be 4.9 minutes, a very considerable reduction in the spread of colors, as expected after Dolland. The refractions of the red and blue rays in passing through the compound prism were about eighteen degrees, the bending responsible for magnifying and focusing in telescope lenses. Cavendish developed rules for computing the difference in the refrangibility of red and blue rays by compound prisms. He experimented with prisms made of white glass and crystals and also with hollow prisms filled with water, spirit (alcohol), solution of Glauber's salt (sodium sulfate), and sugar of lead in water (lead acetate). He was interested in the breadth of images of colored light, for which he derived a formula, and also in their brightness and dilution. For this investigation, his experimental arrangement consisted of two separated prisms and two slits and a hole, each about 1/8 of an inch across. The Sun was usually the

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[99] Another reason for thinking the glass is the same as the one Blagden brought home is that Cavendish's drawing of a compound prism made up of a prism of Swiss glass and a prism of flint glass shows the former as the smaller of the two prisms, in agreement with the smallness of the prism made from Blagden's glass.

source of light, occasionally candles. As customary, he compared theory with experiment, computed values with observed values.[100]

Cavendish's experiments with light and prisms were probably connected with his interest in astronomy, as they took place around the same time as his mounting of Huygens's aerial refractors. If his experiments began with Blagden's interesting piece of glass from Switzerland, they turned into a study of the optical properties of various substances, paralleling his earlier studies of the chemical, thermal, and electrical properties of various substances. Experimental optics had not been a major field when he began his work in natural philosophy, a likely reason why he turned to it only after completing his main work. The part of experimental optics that interested him was the one most closely identified with Newton's optics: experiments on the refrangibilities of the colored rays of sunlight carried out with prisms, slits, holes, and screens. Newton had not solved all the problems of colors, as Dolland showed, nor had Dolland solved all of them, as Ludlam pointed out. Cavendish's late optical researches were both an acknowledgment of Newton's master experimental work and an expression of curiosity about where Newton's lapse led.

Published Work

Cavendish's last five papers published in the *Philosophical Transactions* all had to do directly or indirectly with astronomy, though only one of them, his paper on weighing the world, discussed above, was a major work. In one paper, he calculated the height of an aurora observed from three locations several years before, in 1784. The letters from the observers of the aurora were read to the Royal Society in 1786, and Cavendish's paper was published in 1790. Different from the common aurora borealis, which was seen towards the north low down in the sky in the form of a circle, this aurora was thought to be of the one kind whose height was measurable. Halley had proposed triangulation as a method of finding the height of auroras, and Cavendish was the first to use the method successfully. From the reported observations of the position of the aurora in question among the stars and from the distance between the observers, Cavendish found its height to lie between 52 and 71 miles, an "astonishingly exact result" for a measurement of this kind. It was not until the twentieth century that his result could be confirmed, and as was frequently the case with his work, his result was "not generally recognized" in his time.[101]

Cavendish's interest in the aurora extended beyond the calculation of its height. By analogy with the aurora borealis, he suspected that auroras of this kind consist of parallel rays of light shooting skyward, and he encouraged "people to attend to these arches" to help decide if his hypothesis was "true." His hypothesis had "some probability in it," but it was not yet a "theory of which I am convinced."[102] His paper was one of six papers, including the three letters, on auroras appearing in part 1 of the *Philosophical Transactions* for 1790; it can be seen as a contribution to an effort by the Royal Society to draw attention to auroras.

In 1792 Cavendish published a paper on the Hindu civil year.[103] We see his interest in the subject in the information he sought out at the time. He brought as a guest to the Royal

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[100] Henry Cavendish, "On Rays of Different Colours Transmitted through Prisms of Different Materials," Cavendish Mss V, 4.

[101] Harold Falck-Ytter (1983, 57, 60).

[102] Henry Cavendish (1790); Thorpe (1921, 67–68).

[103] Henry Cavendish (1792).

Society Club William Marsden, a fellow of the Royal Society and an orientalist and linguist, who published a paper on Hindu chronology in the *Philosophical Transactions*.[104] Cavendish commented on a paper on Hindu astronomy by Samuel Davis, another orientalist;[105] Davis was subsequently elected to the Royal Society on the recommendation of Cavendish, who appeared first on Davis's certificate.[106] Around this time, Cavendish added to his library a number of books on India and a subscription to the *Asiatick Researches*, the journal of the Asiatic Society of Calcutta, modeled after the Royal Society of London.

Cavendish began his paper by pointing out that much was known about Hindu astronomy but little about the Hindu civil year, and what was known varied, in part, because different methods were used in different parts of India. To clear up this uncertainty, Cavendish asked the Sanskrit scholar Charles Wilkins, a fellow of the Royal Society, to lend him three almanacs from different parts of the country. Before analyzing the almanacs, Cavendish discussed the Hindu astronomical, or "solar," year, which begins when the Sun comes to the first point on the Hindu zodiac. It is a little longer than the Julian year, by several minutes, so that it begins continually later than the Julian. The year is divided into twelve months; the length of each month is the time the Sun remains in some sign of the zodiac, so that the months are of unequal length. The day, which begins at sunrise, is divided into sixty parts, which again are divided into sixty parts. The civil year in the parts of India that use the Benares almanac is "lunisolar," divided into twelve months, with an intercalcary month inserted occasionally. The lunar month is divided into thirty parts called teethees, each equal to the time it takes for the moon to travel twelve degrees from the Sun. The teethee is sometimes longer than a day and sometimes shorter, two teethees ending on the same day. The counting of days, Cavendish said, is "sufficiently intricate; but that of counting the months, is still more so." We will not go through it here. Because the Hindu civil month, both solar and lunar, does not have a determinate number of days and is not fixed to a regular cycle, an ordinary Hindu has no way of knowing the day of the month other than by consulting the almanac, and at different locations the month might begin on different days. In answer to Cavendish's question if there was a way to avoid the ambiguity, Davis said that there was not, that months can begin on different days at different locations, but that in practice this did not matter much. The Brahmin in charge of the temple had an almanac, which he used to announce times of observances, and if he was an astronomer, he could make the corrections for location. It was otherwise with teethees, lunar days which regulated most religious festivals, which caused considerable perplexity.[107]

Cavendish described the almanacs beginning with Benares. He characterized its preface as a man of the Enlightenment might: it "begins with an invocation to the Deity, and then gives a whimsical account of the four Yoogas, or ages, and of the inferiority of each succeeding age to that preceding it, and concludes with astrological remarks." The almanac contains eleven columns, without titles or explanations, "but by a careful examination of the numbers, a person acquainted with astronomical computations may, without much difficulty, find out their meaning." Cavendish went through the columns one by one, giving his

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[104] William Marsden (1790). 17 May 1787, Minute Book of the Royal Society Club, Royal Society, 8. In that year Marsden was elected member of the Club.

[105] Davis asked Banks to show a paper of his to Cavendish, initiating the connection. Samuel Davis to Joseph Banks, 10 Mar. 1791, Banks Correspondence, Royal Botanical Gardens, Kew, l.38.

[106] 28 June 1792, Certificates, Royal Society 5.

[107] Cavendish (1792, 237, 242).

interpretation. The almanac contained other information such as tables of diurnal motion, places of the Sun and planets in the Hindu zodiac for each week, lunar and solar eclipses, times when the moon and planets come to certain situations, about which, Cavendish said, "there is not a great deal which I understand, and what I do, is not worth taking notice of." There were some tables and figures that he thought "relate only to astrology," falling outside his area of interest and competence.[108]

Another brief publication by Cavendish, in 1797, came about the following way: Mendoza y Rios was given permission by Cavendish to publish as an addition to a paper on nautical astronomy an extract from a letter by him on a method for computing the distance between the moon and a star. In nautical tables published several years later, Mendoza did not use Cavendish's method, which involved a series of corrections and was more complex than the one he chose.[109]

Cavendish's last publication was about a method for dividing astronomical instruments. The success of instrument makers depended on their ability to divide circles and straight lines accurately into equal parts. George Graham's eight-foot mural quadrant at the Royal Observatory was examined by James Bradley, who concluded that it was in error by over fifteen seconds of arc. The instrument maker John Bird replaced it with a quadrant that was accurate to within one second of arc.[110] In a class by himself, Bird never let more than one person into the room when he was working, since the heat could spoil his divisions. For his method of dividing astronomical instruments, Bird needed two kinds of equipment. One was a scale for measuring the radius to 1/1000 of an inch, the other a set of five beam compasses with magnifying glasses. The longest beam was for drawing the circles to be divided, and the others of different lengths were for measuring chords of the circle, the finer divisions of the circle being made by bisection. Beam compasses made scratches at the edge of the circle; points were made with a punch not exceeding 1/1000 of an inch across. In describing a mural quadrant divided by his method, Bird quoted from the *Nautical Almanac* for 1767 in what could be considered the joint faith of an instrument maker and a user of instruments: "a mean of several observations, made by good observers with accurate instruments, properly adjusted, will always lead us either to the truth itself, or extremely near to it."[111]

As was the practice up to his time, Bird made his divisions by hand, the accuracy of which depended critically on his skill. An alternative to his method was that of the dividing engine, which made graduations of instruments largely independent of the skill of the maker. In 1766, Jesse Ramsden built his first dividing engine, which was accurate enough for surveying but not for nautical instruments; he improved on it in 1775. Called his outstanding invention,"[112] his dividing engine consisted of a large horizontal metal circle, the circumference of which was divided into 2160 teeth, in which an endless screw turned, six revolutions of the screw turning the wheel through one degree of arc. The brass astronomical circle to be divided was screwed down on the wheel. A frame above the wheel held the dividing point, which could mark any angle on the limb of the circle "with great exactness."[113] Regarded as a versatile expert on instruments, Cavendish was appointed to a committee of

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[108] Ibid., 238, 242–243, 245.

[109] The extract from Cavendish's letter was published at the end of Mendoza y Rios (1797): "Addition. Contenant une methode pour reduire les distances lunaires," 119–22; Henry Cavendish (1797, 246–248).

[110] Allan Chapman (1993, 209).

[111] John Bird (1767, 2, 11, 13).

[112] E.G.R. Taylor (1966, 244).

[113] Jesse Ramsden (1777, 1).

the Royal Society in 1783 to find out why Ramsden was behind schedule in delivering a seven-foot equatorial circle to the Royal Observatory; at Ramsden's house, the committee found the "Circle ready for dividing."[114] When Ramsden completed a mural quadrant for Milan in 1790, he invited Cavendish along with others to see and try the instrument, which was "true much within a second." Ramsden told his visitors that "any common man in his workshop, with good eyes and hands, could, on the same principles, have divided it to equal perfection."[115] Such was the advantage of a dividing engine over the old method of dividing circles by hand.

In 1785, Cavendish communicated to the Royal Society a paper on dividing circles by John Smeaton. The paper contained two letters by Smeaton's friend the clockmaker John Hindley, who around 1739 "was the first to construct an engine for cutting the teeth in clock wheels and for dividing instruments," making use of the "roller method for the original division of the dividing plate, which was actuated by an endless screw."[116] Hindley's method depended on contact not sight, an advantage in an astronomical circle, the certainty of contact being fifteen times greater than that of vision. Smeaton's pyrometer, for example, which relied on contact, was accurate to 1/24,000 part of an inch, and he thought that 1/60,000 part of an inch was possible. Smeaton summed up the importance of the method of dividing circles: "Perhaps no part of the science of mechanics has been cultivated by the ingenious with more assiduity, or more deservedly so, than the art of dividing circles for the purposes of astronomy and navigation."[117]

Between 1775 and 1778, John Troughton built a dividing engine of Ramsden's construction, thought to be superior in accuracy.[118] He and his younger brother Edward were known for their dividing instruments, which were used by other instrument makers, the ultimate compliment. By the beginning of the nineteenth century, Edward Troughton, who then conducted the business alone, had succeeded Ramsden as the foremost instrument maker in England. In 1807 Cavendish was part of a visitation committee from the Royal Society who agreed with the astronomer royal that observations at the Royal Observatory would have greater accuracy if they were made with a circular instrument as well as with the existing mural quadrant. On the committee's invitation, Troughton recommended a circle six feet in diameter.[119]

In 1804, Troughton had perfected a new method of dividing circles, which he used in graduating the Goombridge Transit Circle, a four-foot transit instrument he made for the astronomer Stephen Goombridge; the instrument was to plague Troughton for years.[120] In the visitation committee, Cavendish spoke against Troughton's "proposed instrument" for the Royal Observatory,[121] his objection to Troughton being partly based on the Goombridge instrument. Cavendish, in Blagden's words, "thought Troughton deficient in judgment, con-

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[114] 31 July and 25 Sep. 1783, Minutes of Council, Royal Society 7:143, 146.

[115] These were Charles Blagden's words, reporting what Ramsden said. Letter to Joseph Banks, 23 Sep. 1790, BL Add Mss 33272, pp. 89–90.

[116] David Baxandall (1923–1924, 135).

[117] John Smeaton (1814, 170, 186).

[118] Baxandall (1923–1924, 136).

[119] Meeting of the committee on 22 Jan. and report of the meeting of the Council on 28 May 1807, "Visitations of Greenwich Observatory 1763 to 1815," Royal Society, Ms. 600, XIV.d.11, ff. 59–62.

[120] A.W. Skempton and Joyce Brown (1973, 246). In 1823 the instrument was examined for accuracy to "correct rumours harmful to Mr. Troughton." Taylor (1966, 289).

[121] 14 May 1807, Charles Blagden Diary, Royal Society 5:69.

trived some things very ill."[122] The committee reported to the Council of the Society that the "instrument recommended by Mr. Troughton is the best they are likely to procure under the present circumstances."[123] The less than wholehearted wording may have expressed Cavendish's reservations.

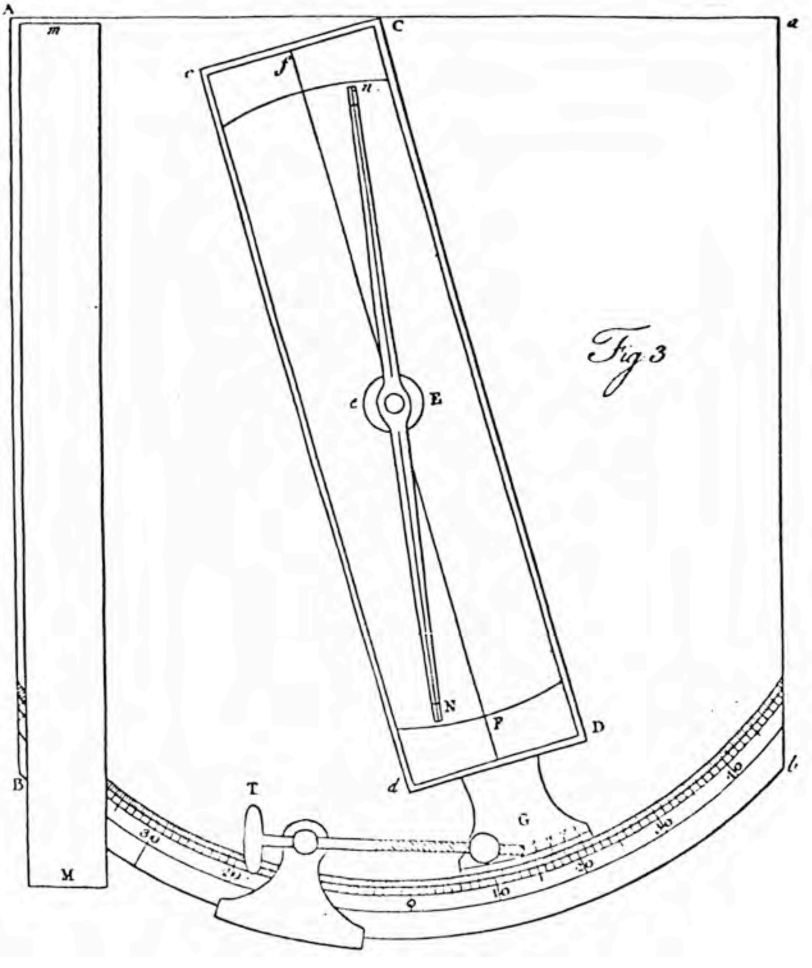

Figure 17.4: Dividing Instrument. From Henry Cavendish, "On an Improvement in the Manner of Dividing Astronomical Instruments," *PT* 99 (1809): 221–45; *Sci. Pap.* 2:289.

Troughton submitted a paper containing his new method of dividing circles to the Royal Society in 1808, and the next year it was published in the *Philosophical Transactions*.[124] He said that Bird was the greatest divider of his time, and after him came Ramsden, Smeaton,

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[122] 22, 23 Jan. 1807, Charles Blagden Diary, ibid. 5:29.

[123] 28 May 1807, Minutes of Council, Royal Society 7:503.

[124] Skempton and Brown (1973, 246).

and his brother John. For his part, he said, he had quickly come to reject beam compasses, finding that he could not bisect two points "without enlarging, displacing, or deforming them" with the tools then in use. Recognizing that only "turning" led to perfection, he used rollers to divide a circle, marking off the revolutions. In the paper Troughton mentioned the six-foot circle he was making for the Royal Observatory.[125]

Troughton's paper is mainly about reducing errors. Two other papers at the time dealt with the same subject. The astronomer John Pond wrote that it is "one of the many advantages of circular instruments, that from the observations made with them, we may infer with great precision not only the mean probable error, but likewise the greatest possible error to which they are liable." For his astronomical circle, he calculated the greatest possible error as 2.5 seconds and the mean error as 1 second.[126] The Lowndean Professor of Astronomy at Cambridge William Lax said that it was unsatisfactory to make observations of "extreme accuracy" with an instrument whose "exactness" cannot be judged. In his paper, he showed how to achieve high accuracy from an instrument that is not very exactly divided.[127] Cavendish's paper, "On an Improvement in the Manner of Dividing Astronomical Instruments," appeared the same year as Troughton's and Lax's papers, and like them it was concerned with reducing error.

Cavendish pointed out the great difficulty in the common method of dividing beam compasses, which required placing a point halfway between two nearby scratches on the limb of the circle, an action that was hard to achieve without the point slipping toward one or the other scratch. He supposed that this was why Troughton invented an alternative "ingenious method of dividing," which induced him to see if the older method of beam compasses could be modified to avoid the objection. His change was to use a beam compass with one point instead of two, replacing the second point with a microscope, in this way eliminating the need to set the point of the compass into any division, and the objection to the "old method" was "entirely removed."[128]

In Cavendish's apparatus, a movable frame rests on the circle to be divided, and there is a single beam compass with a retractable point near one end and a pivot at the other, fitted with a microscope that slides from one end of the beam to the other (Fig. 17.4).[129] Horace Darwin describes his method concisely: "the circle was first divided into 6 parts by setting a beam compass with the points apart at a distance equal to the radius. These spaces were divided again by the beam compass, sometimes into two equal parts, and sometimes into three and five equal parts, and so on till quite small spaces were left. Errors have to be calculated and allowed for, and the process is most laborious and slow."[130] Both his and Troughton's methods were free of the inaccuracy of setting a point of a compass in the center of a division, but his required "much less apparatus" than Troughton's and was "free from any danger of error" from irregularity and slippage of motion of a roller, and it had an additional "considerable advantage" in being free of mistakes in "computing a table of errors."[131] His method had "much advantage" over the common beam compasses

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[125] Troughton (1809, 105–106).
[126] John Pond (1806, 421).
[127] William Lax (1809, 232–233).
[128] Henry Cavendish (1809, 287)
[129] The auction catalog of Cavendish's instruments lists five beam compasses, items 22–24. *Catalogue of Sundry Very Curious and Valuable Mathematical, Philosophical, and Optical Instruments*.
[130] Horace Darwin, in Thorpe (1921, 74).
[131] Cavendish (1809, 293).

in accuracy, but whether or not it had an advantage over Troughton's method was left for instrument makers to decide.

Cavendish's method does not seem to have been adopted. It was for making original divisions, whereas later instruments were graduated using dividing engines, which copied existing divided circles. What is important here is the kind of instrument he was concerned with. His final contribution to science was about a tool for making instruments capable of measuring with more exactness. We close this section with a table from Troughton's paper in the same volume of the *Philosophical Transactions* as Cavendish's. In parts of an inch, the greatest error of six standards were, in order of accuracy:

- .000165 G. Shuckburgh's 5-foot standard
- .000240 W. Roy's scale of 42 inches
- .000273 G. Shuckburgh's equatorial of 2-foot radius
- .000465 Greenwich quadrant of 8-foot radius
- .000700 A. Aubert's standard of 5-foot length
- .000795 Royal Society's standard of 92 inches

Such accuracies had practical as well as scientific and technical significance. Troughton called attention to the place in the ranking of General Roy's scale, which was important because Roy used it to measure the baseline of the national trigonometrical survey.[132] For his paper in 1808 on a method of dividing instruments, Troughton was awarded a Copley Medal. This was not the first time the Royal Society rewarded exactness; earlier instances have come up in this book such as Roy's measurement of a baseline and Harrison's chronometer. Cavendish, who was seventy-seven when his paper on a method of dividing instruments was read before the Royal Society, was interested in furthering this direction of science, to which his earlier work had given impetus. His final contribution rounded out a lifetime's work.

Reasons for Not Publishing

It has been suggested that Cavendish's reluctance to publish more of his work was a consequence of his class and wealth, which isolated him from the scientists of the industrial age, who otherwise could have encouraged him. From a social and material standpoint, he was fortunate in the class he was born into, but from the standpoint of his avocation, scientific research, the argument goes, he was unfortunate. If he had had to earn a living, he would have had different associates and probably a different attitude toward his scientific work. As a scientist of the old school, he might have held Newton's chair in Cambridge or Halley's in Oxford, or as a scientist of the industrial age he might have found work in Birmingham or Glasgow, but being an aristocrat he could do neither. Instead he lived in London and associated with the old ruling class, which in the Royal Society formed a circle around its president Joseph Banks. At Cambridge he studied the science of the previous age, typified by Newtonian mathematics and the mechanics of the solar system, which remained his preference even as his researches led him to heat and chemistry, sciences associated with the rising industrial classes. With a foot planted in each world, the old and the new, he had difficulty in finding a means to communicate his researches.[133] In a general sense, there

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[132] Troughton (1809, 140).

[133] James Gerald Crowther (1962). His discussion of Cavendish is on 272–275.

may be considerable truth in this analysis, only it is hard to know how to apply it to specific cases. He developed furthest the science he modeled after Newton's, electricity, but he left unpublished half of his electrical researches needed to complete the work, even though he was in London in association with Banks's circle. Electricity was not yet a science of practical importance in industry, and it is uncertain that he would have received any more encouragement if he had been in Manchester.

We agree that Cavendish's social origins probably did affect his publishing, though isolation was not the most important way it worked. In the previous century the aristocrat Robert Boyle published his scientific writings, and in Cavendish's time Lord Mahon published a good book on electricity and Edward Delaval received a Copley Medal, but it is noteworthy that in the middle of the eighteenth century, very capable men of science who were aristocrats—Lord Charles Cavendish, Lord Morton, and Lord Macclesfield—published almost nothing on science, their stronger motivation being to perform a public service as scientific administrators. Like his father, Henry Cavendish received recognition for his work in the Royal Society, for which he did not need to publish any more than his father did. Able contemporaries of Cavendish's achieved prominence in scientific society by different routes: Herschel's was mainly through publication, Cullen's was by teaching science, Banks's by promoting it, and Aubert's by serving it. The desire of individuals to achieve recognition through published research could be strong, as priority disputes showed, but the understanding that published research was a uniform measure of an individual's scientific contribution was still in the future.

A number of general explanations of Cavendish's practice of publication have been suggested. One of them is Blagden's, mentioned earlier: Cavendish published everything he was satisfied with, and if he did not publish, it was because he was not satisfied. Another reason is that he carried out researches only or mainly to satisfy his curiosity and was indifferent to their publication. A problem with this is that he was committed to the advancement of science, which depends on publication as well as on curiosity. Another explanation is that he disliked controversy and priority disputes. This may have been the explanation at times, but rarely is there scientific work that does not overlap other work, and Cavendish sometimes did publish. It is said that he was ambivalent about publishing because he was shy and disliked attention directed at himself. He exhibited shyness in social situations, but he was not shy about expressing his scientific opinion, only cautious. Cautiousness is distinct from shyness. It is said that he may have found writing for publication irksome, and perhaps he did, but we know that he liked writing. Still other general explanations have been proposed.[134] The causes of Cavendish's reluctance to publish some of his work are no doubt

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[134] Hugo Lidbetter offers a psychological explanation for why Cavendish held back from publication. He thinks that Cavendish was autistic, for which reason he did not spontaneously share his interests and achievements with others. If Cavendish was autistic, this is a credible general reason for Cavendish's relative indifference to publication. Lidbetter misreads what Christa Jungnickel and I say in our Cavendish biography, where he says that we explain why Cavendish held back from publication by his "views on the inadequacy of language." That is not what we say, as he should know, since he quotes the relevant passage from our book earlier in his article. In a discussion of Cavendish's taciturnity we say that words, as used in normal speech, do not adequately represent Cavendish's world; for that mathematics and quantities are needed. Publications are, of course, exactly where mathematics and quantities are proper and necessary. We offer a suggestion arising from his work that refers to his habits of *speech*, not of *publication.* Jungnickel and McCormmach (1999, 370). Hugo Lidbetter (2009, 784). I thank Steve Silberman for the reference to Lidbetter's article.

complex and probably depend on the work and its timing, and because he said nothing about his reasons they will probably never be fully known.

Cavendish's laying aside researches after initially intending them for publication may not have had entirely to do with the work at hand. William Heberden, who drafted the certificate for Cavendish's membership in the Royal Society, wrote a paper (which he did not publish) on the advantages of writing but not publishing. Writing, he said, "enlarges the mind and improves the taste," a sufficient reason for going to the trouble. The writer, however, if he "has already established a reputation, loses it as soon as he ventures to give anything to the public." The happiest writer, Heberden thought, was one who wrote "always with a view to publishing, though without ever doing so."[135] For a person who relished his privacy as Cavendish did his, Heberden's advice might have seemed not only clever but wise. There were other ways of contributing to science that did not require publishing.

Coinage of the Realm

If Cavendish had been born one hundred years later, or two hundred, he might have directed a scientific institute, and there is reason to think that he would have been good at it. His publications on heat were commentaries on experiments carried out under his direction. He directed meteorological observations at the house of the Royal Society and for a meteorological station he set up on Dartmoor. He instructed travelers to make observations of the heat of wells and springs for determining average climates of the world. He drafted scientific instructions for voyages of discovery. He did basic planning for two major Royal Society projects, observing a transit of Venus and measuring the density of the Earth. His house at Clapham Common was a live-in forerunner of a research institute. Because of a combination of traits—intelligence, dexterity, knowledge, and a sense of fairness—he had an authority he did not have to assert. We recall these facts about him to provide the background for certain experiments he devised for the public good in his later years.

In his *Sentimental Journey through France and Italy*, Laurence Sterne wrote that he had in his pocket "a few King William's shillings as smooth as glass," explaining that "by jingling and rubbing one against another for seventy years together in one body's pocket or another's, they are become so much alike you can scarce distinguish one shilling from another."[136] That description of coinage was given in 1768, five years before a large recall of the smooth gold coins.

In 1787 Charles Jenkinson, Lord Liverpool, president of the board of trade, directed a committee of the privy council to look into the state of the coinage of the kingdom. It called on the mint to review its gold, silver, and copper coins and collected information for years. In 1796 the one man of science on the committee Joseph Banks gave Jenkinson a long list of questions about the "extravagant waste" of gold owing to the wear of coins and defects in their manufacture.[137] The next year the war with France strained the finances of Britain, and the stock of gold being uncertain Parliament ordered the Bank of England to cease payments of its notes in gold. At the same time the minting of gold coins was cut back, and in 1798 the

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[135] William Heberden, "Upon Composition, Authors, and Their Works in General, Either of Genius or Science," quoted in Humphry Rolleston (1933, 417–418).

[136] Laurence Sterne (1951, 165–166).

[137] Unsigned memorandum by Joseph Banks to Charles Jenkinson, Lord Liverpool, [1796], in Liverpool Papers, BL Add Mss 38422, vol. 233, ff. 320–324, on 321–322.

minting of silver coins was stopped completely.[138] That year the privy council committee on coins was reconstituted. Jenkinson opened the proceedings with a speech on the principles and history of coinage, pointing out that for eighty years gold coinage had been the de facto standard replacing silver. Gold was as plentiful as silver had once been, and he advocated adopting gold as the legal standard, replacing silver.[139]

The industrialist Boulton and the chemist Charles Hatchett were asked to write reports on the coinage, which were given to John Rennie, an engineer for Boulton & Watt, who undertook a complete study of the machinery at the mint. The reports were also given to Banks and Cavendish, who addressed the related problems of the wear of gold coins and the most durable alloy of gold for coins.[140] For the person to carry out experiments to decide if the loss of gold was due to defects in the quality of the gold or in the figure and impression of the coins, Cavendish recommended Hatchett, "whose accuracy can be relied on" (Fig. 17.5).[141] Cavendish was asked to assist Hatchett, and if it would help to persuade him (it was not needed) the king would appoint him a privy counselor.[142]

Cavendish planned the experiments to determine what kind of gold coin would best resist wear. To replicate the wearing of coins in Laurence Sterne's pocket, and any other kind of wear arising from their circulation, he designed machines for punishing coins, which were built by the instrument maker John Cuthbertson in whose house the experiments were carried out. One machine was a rotating cubic box in which batches of 200 pieces of gold of different ductility were agitated.[143] Another machine compared the effect of friction produced by various abrasive materials such as sand and metal filings when variously alloyed gold was rubbed against them. Another machine pressed pairs of coins together, moving them laterally across one another. In this machine, twenty-eight coins were placed in an upper horizontal frame and the same number in a lower horizontal frame, and with a weight placed on top, the two frames were moved independently at different rates back and forth by a person turning a wheel (Fig. 17.9). In a typical experiment with this machine, 573,380 cycles were run under a load of $3\frac{1}{2}$ pounds. The experiments were varied, using embossed coins and coin blanks and like and unlike paired metal coins.[144]

The two main questions were: first, whether soft or hard gold experiences the most loss to friction in the circulation of coins; second, whether a smooth coin or an embossed coin wears least. The experiments showed that when coins of the same qualities are rubbed together, the most ductile coins wear least, and that when dissimilar coins are rubbed together, the reverse is the case.

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[138] John Craig (1953, 260–262).

[139] Ibid., 267–268.

[140] Ibid., 268–269. Charles Jenkinson, Lord Liverpool to Joseph Banks, 10 May 1798, BM(NH), DTC 3:279–280.

[141] Henry Cavendish to Joseph Banks, 23 July and 6 Aug. 1798; in Jungnickel and McCormmach (1999, 708–709). Charles Jenkinson, Lord Liverpool to Joseph Banks, 13 Feb. 1799, BM(NH), DTC 3:195–196. On Cavendish's urging, a report was also given by A. Robertson, an Oxford mathematician who did research on coinage; Robertson's report was delivered and read by Cavendish, to whom Liverpool gave his thanks on 12 Apr. 1799; in Jungnickel and McCormmach (1999, 714).

[142] Charles Jenkinson, Lord Liverpool to Joseph Banks, 7 July 1798, BM(NH), DTC 3:19–20.

[143] Charles Hatchett to Joseph Banks, 14 Mar. 1800, BL Add Mss 33980, f. 225.

[144] J.C. Chaston (1974, 111).

Young Colleagues

Figure 17.5: Charles Hatchett. Engraving by F.C. Lewis from the painting by T. Phillips. Collaborator of Cavendish's. Wikimedia Commons.

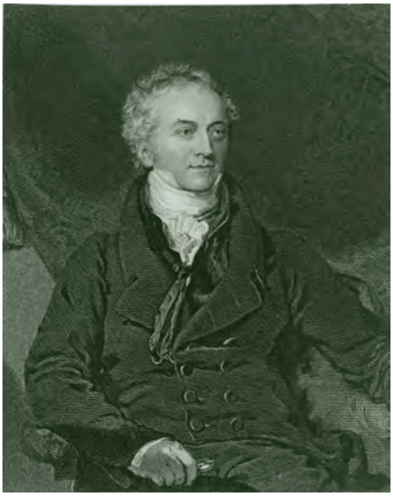

Figure 17.6: Thomas Young. Painted by Sir Thomas Lawrence, engraved by G. Adcock. Natural philosopher, Cavendish's colleague at the Royal Institution. Courtesy of Smith Image Collection, Van Pelt-Dietrich Library, University of Pennsylvania.

Figure 17.7: Sir Humphry Davy. Painted by James Lonsdale, engraved by W.H. Worthington. Chemist, Cavendish's colleague at the Royal Institution. Courtesy of Smith Image Collection, Van Pelt-Dietrich Library, University of Pennsylvania.

Figure 17.8: James Lewis Macie (Smithson). Tempera on paperboard, miniature portrait by Henri-Joseph Johns, 1816. Chemist, said to have worked in Cavendish's laboratory. Cavendish's. Wikimedia Commons.

The loss of weight in the experiments in any case was found to be miniscule, of the order of one grain per coin.The general conclusion was that whatever differences there are between different gold alloys, the loss that coins experience in normal circulation is trifling. The worn look of coins is explained by the prominences being simply pressed into the mass of the coins, not by any appreciable loss of weight. Any significant loss of gold would have other explanations.[145]

The experiments on the composition of coins turned out not to be particularly useful to the government, for they confirmed the practice of the minters, who proceeded with their alloys by experience without the aid of science,[146] but they did bring forward new facts of considerable scientific value. Hatchett said that knowledge of metal alloys had not "kept pace with the rapid progress of modern chemistry," being scarcely superior to what Pliny and the ancients knew.[147] As for knowledge of wear, a recent commentator writes, the grasp shown by Cavendish of its complex nature "was masterly; his work could have been studied with advantage by investigators a century later."[148]

Hatchett wrote the report for the privy council committee on coins. Cavendish prefaced it with a letter explaining that Hatchett had done the experiments and was best able to give an account of them. Hatchett's experiments were carried out with "great judgment & accuracy, & in the manner which to both of us seem best adapted to the object proposed," Cavendish said.[149] He appealed to the government to allow Hatchett to publish his results rather than keeping them a government "secret," as no "bad effect" could come of it.[150] In support, Banks told Liverpool that Cavendish and Hatchett were anxious that their findings on metallurgy might be anticipated, in particular by the French.[151] "At the request of Mr. Cavendish," Hatchett wrote in the abridged paper read to the Royal Society in 1803, "I have written the following account; but I should be highly unjust and ungrateful to that gentleman, did I not here publicly acknowledge how great a portion truly belongs to him." The machines and dies were "entirely contrived" by him.[152] The paper appearing in the *Philosophical Transactions* was very long, 151 pages, Cavendish contributing the section describing the instruments.[153]

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[145] Ibid., 111–112.

[146] Ibid., 112. In the practice at the time, the best compromise of hardness and color was obtained by an amalgam 1/12th to 1/13th of alloy; pure silver and pure gold were found unsuitable. Joseph Banks to Lord Liverpool, 11 May 1801, BL Add Mss 38424, ff. 158–59. Craig (1953, 269).

[147] Charles Hatchett (1803, 193).

[148] Chaston (1974, 112).

[149] Cavendish to the privy council committee for coins, prefacing Charles Hatchett's report, 28 April 1801; in Jungnickel and McCormmach (1999, 724). Joseph Banks to Charles Jenkinson, Lord Liverpool, 11 May 1801, BL Add Mss 38424, ff. 158–59. The report addressed to Lord Liverpool and the select committee for coins was signed by Hatchett, 28 Apr. 1801, BL Add Mss 38426. The title of the report of the experiments, which begins on f. 25, is "Experiments and Observations on the Various Alloys, on the Specific Gravity, and on the Comparative Wear of Gold."

[150] Henry Cavendish to Charles Hatchett, 15 Oct. 1802; in Jungnickel and McCormmach (1999, 726). This letter was enclosed in a letter to Banks by Hatchett, in which Hatchett said that Lord Liverpool was satisfied with Cavendish's opinion on the publishable nature of the material. Charles Hatchett to Joseph Banks, 24 Oct. 1802. Hatchett and Cavendish's desire to see the experiments published was first put to Lord Liverpool by Joseph Banks on 21 Aug. 1801, BL Add Mss 38424, ff. 160–161.

[151] Banks to Lord Liverpool, 21 Aug. 1801.

[152] Hatchett (1803, 45).

[153] Ibid., 140–147.

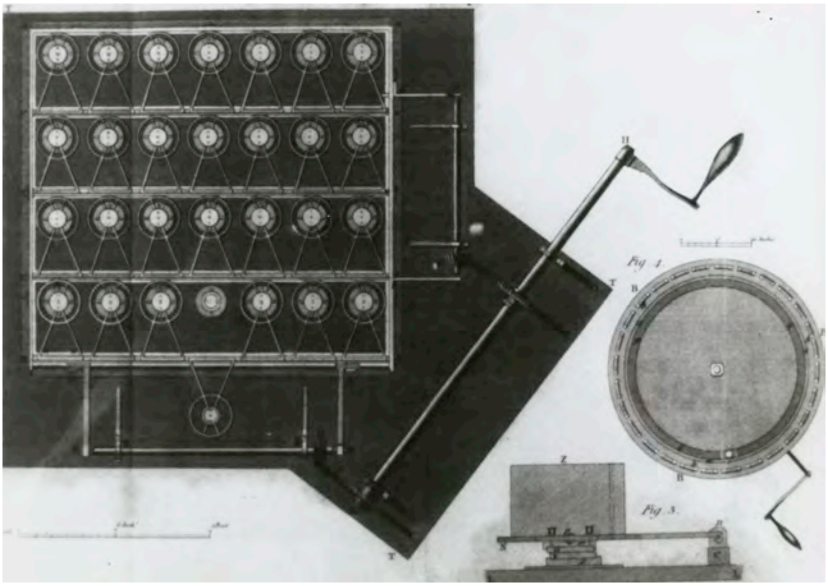

Figure 17.9: Coinage Apparatus. This drawing shows the apparatus invented by Cavendish for measuring the wear of coins, built for him by the instrument maker John Cuthbertson. Twenty-eight pairs of coins are pressed and rubbed together by turning the crank. Each pair of coins is separately weighted, and the frames holding the top and bottom coins vibrate at different rates to reduce grooving. Charles Hatchett, "Experiments and Observations on the Various Alloys, on the Specific Gravity, and on the Comparative Wear of Gold. Being the Substance of a Report Made to the Right Honourable the Lords of the Committee of Privy Council …," *Philosophical Transactions* 93 (1803): at end of volume.

There is a sense in which coinage and nature posed a similar problem. In his researches, as we have seen, Cavendish repeatedly introduced a "standard" by which to measure certain phenomena or substances, and he referred to substances or powers as being in a certain respect "equivalent." The same terms were used to understand the wealth of nations. In a letter to Liverpool on the subject of coinage, Cavendish referred to the "standard" of the fineness of gold.[154] Liverpool told his committee on coins that the "standard coin of every country is the measure of property in it," and unlike other kinds of measures it is also the "equivalent" of the property measured by it. The problem of coinage came about because the standard for measuring the value of things could not be fixed once and for all; money was an equivalent made of gold, silver, or copper, and the prices of those metals fluctuated. From its dual function as standard measure and equivalent, money acquired the "principal difficulties" that attended it in speculation and in practice.[155]

There was a long-standing tradition of scientific service in the government in matters of coinage. Newton had been master of the mint, and after Newton the connection of the mint

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[154] Henry Cavendish to Charles Jenkinson, Lord Liverpool, 13 July [1798]; in Jungnickel and McCormmach (1999, 704).

[155] Charles Jenkinson, Lord Liverpool (1805, 8–9). "Heads of So Much of Lord Liverpool's Speech," f. 402.

with the Royal Society remained substantial, most of its masters having been fellows of the Royal Society.[156] So far as we know, Cavendish was never considered for that office, but of the scientific men of his time in England he was closest to Newton in his skills and standing, a possible reason why he was selected as the appropriate scientific authority for examining the condition of the nation's coinage. For Cavendish it would have been performing a duty of service.

Royal Institution

For decades Cavendish served two institutions, the Royal Society and the British Museum, and in the last decade of his life he served a third, the Royal Institution. The last named was the creation of Benjamin Thompson, or as he was then better known, Count Rumford. He had served with the British army in the American Revolution, and later at the court of the elector of Bavaria, he had served as head of the army. He had also made inventions, performed experiments, and conceived of the idea of an institution of mechanics and heat. In 1798 he came to London, where his ideas on kitchens and heating had preceded him, put in place at the Foundling Hospital by the philanthropist Thomas Bernhard. Invited by Bernhard and the recently formed Bettering Society to draw up a plan, Rumford proposed an institution dedicated to teaching the applications of science and spreading knowledge of inventions. To fund it he organized a subscription whereby a person who gave fifty guineas or more became a perpetual proprietor. There was a quick response, and in 1799 the Royal Institution of Great Britain was launched.[157] The first lecture was announced for March 1800 in a house on Albemarle Street (Fig. 17.10).

Both Cavendish and the duke of Devonshire paid their fifty guineas about a year after the Institution was founded, by which time it looked respectable, with a substantial aristocratic representation.[158] The governing body consisted of nine managers, elected initially from the proprietors, and Cavendish promptly became a manager.[159] The meetings of the managers were irregular but frequent, attended as a rule by only three or four managers along with the secretary and treasurer, with Cavendish the most faithful attender. He was also a conscientious member of the "scientific committee of council," a standing committee set up to oversee the syllabus and scientific experiments, which included Blagden, Hatchett, and several other fellows of the Royal Society.[160] When the first scientific lecturer Thomas Garnett acted independently, Rumford got the managers to appoint a small committee consisting of Cavendish, Banks, and himself to supervise the drawing up and publication of the syllabus of lectures in the future.[161] In this and other ways Rumford leaned on Cavendish and Banks to establish his authority. The second year saw important changes of staff. On Banks's

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[156] John Craig (1964, 161–162).

[157] K.D.C. Vernon (1963). W.J. Sparrow (1964, 109–110). Sanborn C. Brown (1976).

[158] Cavendish became a proprietor on 10 Feb. 1800. The managers at their meeting on 17 Feb. said that the Royal Institution was "now established on a Basis so firm & respectable, that no Doubt can be entertained of its Success." Royal Institution of Great Britain (1971).

[159] He was elected at the annual meeting of proprietors on 1 May 1800. Entry for 5 May 1800, Minutes of the Meetings of Managers, Royal Institution 2:70.

[160] 31 March 1800, Minutes of the Meetings of Managers, Royal Institution Archive 2:39–41. The other members of the committee were James Rennell, Joseph Planta, E. Whitaker Gray, J. Vince, and William Farish. The last two were professors of experimental philosophy and of chemistry at Cambridge. Maskelyne was appointed but declined because he was too busy.

[161] 2 Feb. 1801, Minutes of the Meetings of Managers, Royal Institution 2:126–127. Vernon (1963, 18).

recommendation, Garnett was replaced by Thomas Young, and on Rumford's recommendation, Humphry Davy was hired as an assistant lecturer in chemistry (Figs. 17.6–17.7). By persistently attracting a fashionable audience to his public lectures and by doing outstanding chemical research, Davy ensured the success of the Institution.[162] Rumford's methods were dictatorial and his presence erratic, and as the Royal Institution departed from its original purpose his interest in it flagged; in 1802 this restless man left the Institution for good.[163] The next year the scientific committee was reappointed, with Cavendish, Banks, and Hatchett on it again.[164] That same year the committee recommended as Thomas Young's successor John Dalton, who gave occasional lectures at the Institution.[165]

Figure 17.10: Royal Institution. Distinguished Men of Science. Engraving by William Walker around 1862, from a drawing by Sir John Gilbert. The full title is "Distinguished Men of Science Living in Great Britain in 1807–8." The setting is the library of the Royal Institution, but the men shown in the print never gathered in this room. The artist created the group from individual portraits. Henry Cavendish is placed in the front, sitting apart, his eyes downcast; perhaps this is the artist's interpretation of Cavendish's solitude in company. Cavendish's profile and dress are based on William Alexander's sketch, with obvious differences: Cavendish's hat is removed; he is seated instead of walking; he faces the other direction; and he is made to appear thirty years younger. Cavendish was a manager of the Royal Institution from 1800. Wikimedia Commons.

Cavendish had long been a subscriber to the Society of Arts without taking part, whereas he was fully involved in the affairs of the Royal Institution from the start. The difference is likely explained by the stronger connection to science in the Royal Institution. Cavendish supported formal cooperation between the Royal Institution and the Royal Society, seconding Rumford's motion to direct the secretaries of the two institutions to keep one another regularly informed.[166] We have no way of knowing how much interest

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[162] Vernon (1963, 19, 22).

[163] Ibid., 24.

[164] 26 May 1803, Minutes of the Meetings of Managers, Royal Institution 3:137–138.

[165] 5 Sep. 1803, Ibid. 3:151.

[166] The motion seconded by Cavendish requested the Royal Society to inform the Royal Institution of those papers read at its meetings that were suitable for the Royal Institution's journal. It also required that an earlier resolution of 31 March 1800 be communicated to the Royal Society concerning the duty of the scientific committee to commu-

Cavendish took in the lectures at the Institution beyond what was required of him as a member of the standing committee. Among his papers is a letter from Thomas Young asking his opinion on a question about gearwork for his syllabus, and in his lectures Young gave an explanation of halos around the Sun that Cavendish had suggested to him.[167] Cavendish took considerable interest in the scientific research in the laboratory, over which he, Banks, and Hatchett had charge.[168] Through the last year of his life Cavendish followed Davy's experiments.[169]

In addition to his concern with the practical applications of heat, Rumford had an active interest in the science of heat, which he made his specialty. In the arsenal in Munich, he observed the heat generated in boring cannon, which suggested to him an experiment on the heat of friction. He forced a dull steel boring tool against a slowly rotating metal cylinder immersed in about sixty pounds of water, raising its temperature from 60° to the boiling point in about three hours. The heat seemed inexhaustible to Thompson, who concluded that on the basis of his experiment with friction, heat "cannot possibly be a *material substance*," and that it is impossible to imagine it as anything "except it be MOTION." He published his cannon-boring experiment in 1798. The following year he published an experimental investigation into the supposed weight of heat, arriving at the same conclusion: if heat were a substance it would have to be "so infinitely rare [...] as to baffle all our attempts to discover its gravity," whereas if heat were the "intestine vibratory motion of the constituent parts of bodies" it would not affect their gravity.[170] From the point of view of the Royal Institution, Rumford's understanding of heat was fortunate. When a tract on heat and light by Davy[171] came to his notice, he recognized in it ideas on heat similar to his own.[172] Garnett, who had studied under Black at Edinburgh University, gave a full account of Black's theory of "latent heats" in his lectures at the Royal Institution. Throughout his lectures, he used the word "caloric," which he understood to be independent of the cause of heat, but he spoke of it as being "combined" with ordinary matter, suggesting a material theory of heat. Rumford and Garnett had a falling out over another issue, but Rumford may have been dissatisfied with the contents of Garnett lectures as well.[173] Thomas Young, Garnett's replacement,

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nicate discoveries to the Royal Society. 5 Apr. 1802, Minutes of the Meetings of the Managers, Royal Institution 2:260.

[167] Thomas Young to Henry Cavendish, 3 Sep. 1801, enclosed in a paper, "On the Shape of the Teeth in Rack Work"; in Jungnickel and McCormmach (1999, 725). Young acknowledged Cavendish for the demonstration. Thomas Young (1802, paragraph 179; 1807, 2:308). Joseph Larmor's comment in Cavendish, *Sci. Pap.* 2:410.

[168] Vernon (1963, 27).

[169] John Davy (1836, 222).

[170] Benjamin Thompson, Count Rumford (1798); in Thompson (1870–1875, 1:490); Thompson (1799); ibid. 2:14.

[171] Davy was working in Thomas Beddoes's Pneumatic Institution at the time. Beddoes included Davy's "Essay on Heat, Light, and on the Combinations of Light" in his collection *Contributions to Physical and Medical Knowledge, Principally from the West of England* (Bristol, 1799), 3–147. David M. Knight (1971, 599).

[172] George E. Ellis (1871, 486).

[173] Garnett took up heat in his chemical lectures rather than in his lectures on natural philosophy. He accepted the new chemistry of Lavoisier's together with the new nomenclature: the phlogiston theory, he said, involved its supporters in "continual absurdities, and "the ancient language of chemistry was "very barbarous," "conveying false ideas." Following the new nomenclature, he called heat "caloric," whether it is an imponderable fluid or motion, but as a former student of Black's he talked about caloric in the way Black talked about heat, as if it were a fluid. When a quantity of heat becomes latent, it "becomes absorbed." Bodies become elastic fluids through their "combination" with caloric. Caloric occurs either in a "combined or free state." *Outlines of a Course of Lectures on Chemistry: Delivered at the Royal Institution of Great Britain, 1801* (1801a, 16, 36, 39, 45, 60, 66). He published at the same time *Outlines of a Course of Lectures on Natural and Experimental Philosophy, Delivered at the Royal Institution of Great Britain, 1801* (1801b). On his studies at Edinburgh, "The Life of the Author" (1804, vi–vii).

held a view of heat similar to Rumford's. For a time in the Royal Institution, there was a concentration of advocates of a minority opinion on the nature of heat: Rumford at the head of the Institution, Davy the experimenter and lecturer, Young the lecturer, and the natural philosopher of Rumford's inner circle, Cavendish. It is worth noting that near the end of his life, Cavendish was in the company of scientific investigators who broadly agreed with him on the nature of heat.[174]

When Davy arrived at the Royal Institution in 1801, he was received by Rumford, Cavendish, and Banks, who promised him any apparatus he wanted for his experiments.[175] When Cavendish died, his proprietorship in the Institution was inherited by his heir Lord George Cavendish, from whom Davy obtained some of Cavendish's chemical apparatus. Five months after Cavendish's death, Davy received permission from the managers to bring the apparatus into the Royal Institution for use in experiments and lectures.[176]

At the beginning, Rumford published a prospectus, explaining the need for the Royal Institution. For men of science, he wrote, a discovery was its own reward. Detached from the "ordinary pursuits of life, they lacked the "proper "moral and intellectual habits" to "descend from the sublime general theories of science and enter into the detail of weight, measure, price, quality," the practical side. The Royal Institution existed to close the gap between science and industry. Rumford's biographer says that he was unique in his "insight into the importance for society of the development of technology," and that an opportunity was lost when the Royal Institution did not become a school of mechanics,[177] though as it happened, neither the men of science nor the manufacturers were much interested in Rumford's idea. What Cavendish thought of it is unknown. We know that Rumford valued his active participation in the early years, and from what we know about his interest in industry from his journeys in the 1780s, he may have had some sympathy for Rumford's idea for the Institution, but his natural interest lay with the scientific research carried out there. In any case, the Royal Institution became a productive scientific research laboratory.

The Royal Institution benefited from Cavendish's services, and in return it enriched his life. In his last decade, through his activities at the Royal Institution, he was associated with several of the most talented physical scientists in the country: Rumford, Young, Davy, and Dalton. He did not live quite long enough to see the arrival of the greatest of the scientists to work in the Royal Institution, Michael Faraday.[178]

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[174] G.N. Cantor has noted the agreement on heat between Rumford, Davy, and Young, in "Thomas Young's Lectures at the Royal Institution," (1970, 90). In contrast to Garnett's implied preference for the fluid theory, Young in his lectures at the Royal Institution reasoned by an analogy with the vibrations of sound that heat is the vibrations of the parts of bodies. Young, *Course of Lectures on Natural Philosophy* 1:148–149, 656. Davy wrote in 1799, "It has then been experimentally demonstrated that caloric, or the matter of heat, does not exist" and that heat is a "peculiar motion, probably a vibration, of the corpuscles of bodies." (1839–1840, 2:13–14). Davy and Young included in their lectures the new understanding of radiant heat. With praise for Rumford's experiments, Davy explained that vibrating particles of bodies give rise to vibrations in the ether, which in turn communicate vibrations to particles of bodies. Humphry Davy (1802, 50–54).

[175] Humphry Davy to Davies Gilbert, 8 Mar. 1801, in John Ayrton Paris (1831, 78).

[176] Royal Institution of Great Britain, *Minutes of Managers' Meetings 1799–1900* 5:47, 62, 126, 160.

[177] Sparrow (1964, 110, 117).

[178] Three years after Cavendish's death, in 1813, Davy received from Faraday a copy of the notes he took of Davy's lectures at the Royal Institution, the beginning of Faraday's long association with the Institution.

Institute of France

While Rumford was still head of the Royal Institution, in late 1801 he wrote to Banks from Paris to inform him confidentially that he, Banks, headed the list of ten foreigners put up by the class of mathematics and physics of the Institute, the successor to the Royal Academy of Sciences.[179] Each of the several classes of the Institute proposed candidates for foreign membership to be balloted on at a general meeting, the number to be admitted fixed at twenty-four. Interested parties ranked candidates much like racehorses.[180]

Rumford reported that after Banks came Maskelyne, Cavendish, Herschel, Priestley, Pyotr Simon Pallas, Alessandra Volta, and three others, in that order. Rumford was himself proposed but in another class. Blagden, who also was in Paris, kept Banks closely posted on the rapidly evolving, rather undignified scene. Not himself a candidate, Blagden joined in the frenzied lobbying for persons who were. He pressed Cavendish's claim with the scientists he knew in the Institute, fully expecting him to be the first elected after the Institute had fulfilled its duty of electing the former foreign associates from the defunct Royal Academy.[181] His next letter was less certain. Pallas and Cavendish were tied on the first ballot, and on the second Pallas came up one vote ahead, not because the "people here are so ignorant as to think him superior to Cavendish," but because Pallas was a former associate of the Academy. Volta, whose high reputation was "here, perhaps a little exaggerated," Martin Heinrich Klaproth "deservedly," and Watt were very much in the running. Cavendish might be chosen at the next election, and although there was "no certainty" of that, very much in his favor was the opinion of the First Consul Napoleon, who took the opportunity of "expressing how much he esteems Mr. Cavendish."[182] In his next report, Blagden said that at the coming election, the mathematics and physics class intended to present, first, Cavendish, then Watt, "who ran him pretty hard," and third Paolo Mascagni, Volta being out of the running.[183] This time Blagden was proven right; Cavendish was elected.[184] The Institute listed the foreign members according to their merits in science: Banks was first, Maskelyne because of his lunar tables for determining longitude next, and then Cavendish.[185]

Wealth

After Cavendish's death, reports of his wealth appeared in various publications. Georges Cuvier, secretary of the physical sciences department of the reconstituted Academy of Sciences, wrote in his éloge of Cavendish that an uncle of his who had fought in a war in India formed an attachment to Cavendish and left him the entire great fortune he brought home with him. Cuvier said that when Cavendish died, he left behind £1,200,000,[186] which was high but not far off. The following year the French physicist Biot provided more detail. In

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[179] The Royal Academy of Sciences, founded in 1699, was abolished together with all academies in 1793. In 1795, the National Institute of Sciences and Arts was established, which brought together the old academies. The Institute of France was established in 1796, containing the Academy of Sciences, no longer "Royal."

[180] Benjamin Thompson to Joseph Banks, 22 Nov. 1801, BL Add Mss 8099.

[181] Charles Blagden to Joseph Banks, 19 June 1802, BM(NH), DTC 3:170–174.

[182] Charles Blagden to Joseph Banks, 15 Oct. 1802, BL Add Mss 33272, pp. 204–205.

[183] Charles Blagden to Joseph Banks, 26 Nov. and 6 Dec. 1802, ibid., pp. 210–213.

[184] Charles Blagden to Joseph Banks, 29 Jan. 1803, Fitzwilliam Museum Library, Perceval H205.

[185] Charles Blagden to Joseph Banks, 1 Feb. 1803, ibid., H206.

[186] Georges Cuvier (1961, 237).

a biographical sketch of Cavendish for an encyclopedia, he wrote that the uncle returned in 1773, when upon seeing that Cavendish was poorly treated by his family left all of his fortune to him, more than £300,000.[187] The uncle from India, the year 1773, and £1,200,000 became facts of Cavendish's life, as it was picked up in biographical works,[188] though an English biographical dictionary added £100,000 to his wealth, £1,300,000.[189] Thomas Thomson, who was the source of the latter figure, said correctly that Cavendish was left a "very considerable fortune" by his father. He also said that "an aunt who died at a later period bequeathed him a very handsome addition to it." He was right about there being an aunt, but she died four years before Lord Charles, to whom she left her considerable fortune. It came to Henry Cavendish by inheritance through the personal estate of his father. Thomson said correctly that because Cavendish did not spend all his yearly income, it steadily accumulated, leaving him very rich at the end.[190]

Wilson regarded the subject of wealth as being important in Cavendish's life, and he gave it appropriate attention. He placed most credence on Cuvier's account supposing that he got some of his information from Blagden.[191] He was right, as we know because Cuvier asked Mme. D. Gautier to thank Blagden for the details about Cavendish he sent him. When Blagden saw Cuvier's éloge, he wrote back that he approved what it said about Cavendish's merits, but that it "contains many inaccuracies taken from a paper published some years before in France under the name of Mr. Biot. Mr. Cavendish's fortune did not come to him in the manner there asserted, but he inherited it regularly from his father."[192] What is indisputable is that both Cuvier and Biot got the source of Cavendish's fortune wrong.

Wilson said that he was unable to discover the overseas general, or learn whether it was an uncle or an aunt who left Cavendish a fortune. He thought that this was not of great significance, but the date when Cavendish acquired the fortune was important because it was then that Cavendish acquired financial independence. According to Biot, Cavendish was forty when he became independent. Wilson put an upper date on it in the belief that Cavendish settled an annuity of £500 on Blagden in 1782 or 1783, when he was fifty-one or fifty-two, implying that he had to be well off by then to afford it.[193] Wilson was right about the time.

The wealth of Charles and then of Henry Cavendish had three sources: the family settlements and legacies, without which there would have been no wealth; financial prudence; and the public debt of the kingdom. In addition to the three revolutions we have discussed, scientific, political, and industrial, Charles and Henry Cavendish were beneficiaries of a fourth "revolution," this one commercial. One of the outcomes of the Revolution of 1688–89 was a change in the relationship between business and government. In the past, most government

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[187] J.B. Biot (1813, 233).

[188] "Cavendish (Henri)," in Arnault (1827, col. 294). "Cavendish (Henri)," in Hoefer (1855, 294). "Cavendish, Henry," in J.C. Poggendorff (1863, 1:406).

[189] John Aikin and William Johnston (1814, 283–285).

[190] Thomas Thomson (1830–1831, 1:336–337).

[191] Wilson (1851, 159).

[192] D. Gautier to Charles Blagden, 30 Apr. 1811; Charles Blagden to D. Gautier, 20 Apr. 1812, Blagden Letters, Royal Society, G11, G11a. There would seem to be a problem with what Blagden says. Cuvier's publication is dated 1812, and Biot's publication above appears in a volume of the encyclopedia for the year 1813. On the face of it, Cuvier could not have copied Biot. However, Blagden's letter pointing out Biot's errors was written in 1812, the year before the volume of the encyclopedia. In it Blagden does not refer to an encyclopedia but to a "paper" published by Biot several years before. This paper I am unfamiliar with.

[193] Wilson (1851, 160).

borrowing had been on the king's word, which events had proven untrustworthy. Parliament took over the responsibility for guaranteeing loans in 1693, from which time a "public debt" can properly be spoken of. The public had sufficient confidence in the financial stability of the country to deposit its money in the Bank of England, which was designated to handle the public debt in part, and to buy shares in it, known as the "funds." Because good land was becoming scarce, public loans appealed as an alternative source of income, with several to choose from. An enormous loan was offered by the South Sea Company and a smaller one by the East India Company, and a substantial loan was offered by the Bank of England, which also issued a group of annuities. The latter contained so-called perpetual annuities, or annuities requiring the government to pay a fixed rate of return in perpetuity. Over the course of the century, most of the public debt, and most of our Cavendishes' wealth, came to be held in annuities of this kind.[194] (Fig. 17.11).

Figure 17.11: Great Hall of the Bank of England. By Thomas Rowlandson, 1808. Wikimedia Commons.

The perpetual annuities owned by the Cavendishes were controlled by a new policy introduced in 1751. The outstanding loans paying 3%, some through the Bank of England and some through the exchequer, were consolidated into a single fund, which was named the "3% Consolidated Annuities," or "consols" for short. Other annuities paying more than 3% were united in another fund now paying only 3%, which were named "3% Reduced Annuities." Both of these funds were managed by the Bank of England, which paid out interest, or "dividends." The dividends were paid twice yearly; in other words, 3% annuities paid 6% annually. On stated days the dividends were drawn and signed for; if the owner of

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[194] Alice Clare Carter (1968, 2–9). John Carswell (1993, 8, 12, 18–20).

the stock was not present, the dividends were deposited through power of attorney with the Bank or the trading companies.[195]

Most of the owners of Bank of England stock lived in and around London. They were a varied lot, with many migrants, Huguenots and Spanish and Portuguese Jews, a good many gentry, gentleman, and peers, especially dowagers and ladies, corporate bodies such as Cambridge colleges, and increasingly spinsters and widows. Investors usually bought stock and kept it, withdrawing only dividends or else reinvesting them. Most of the stock was held by a very few persons, who included Henry Cavendish.

To the world, Cavendish's great wealth has proven nearly as intriguing as his discoveries, as is evident from Biot's French encyclopedia article: Cavendish was "the richest of the wise and the wisest of the rich."[196] The article on Cavendish in the *Encyclopaedia Britannica* says that he was "indeed not less famed in his country for the great accumulation of his property than for his intellectual and scientific treasure."[197] The interest in the subject and the erroneous statements about it justify a closer look at Cavendish's wealth.

Before his father's account was transferred to him, Henry Cavendish had stocks in his own name worth £17,388:[198]

- October 1776. New South Sea Annuities. £1100.
- 14 December 1781. Reduced 3% Annuities. £14,500.
- 23 August 1783. New South Sea Annuities. £872.
- 25 August 1783. South Sea Old Annuities. £916.

Henry Cavendish inherited from his father in 1783 the following funds:

- Bank Stock. £25,815.
- New South Sea Annuities. £48,900.
- Reduced 3% Annuities. £18,285.
- Consolidated 3% Annuities. £62,100.
- Old South Sea Annuities. £6000.

The total comes to £161,100 in funds from his father. On the last day of 1783, through his father, he inherited his aunt Elizabeth Cavendish's funds worth £97,100:[199]

- Reduced 3% Annuities. £22,100.
- Consolidated 3% Annuities. £75,000.

Adding the above amounts gives Cavendish's wealth in funds in 1784 as £275,588. At age fifty-three he was moderately rich. He lived another twenty-five years, over which time his wealth quadrupled, so that at the end he was very rich. We can see how this happened by looking at the growth of several of his funds.

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[195] Eugen von Philippovich (1911, 135). John Clapham (1945, 1:77, 97–98). Carter (1968, 10).

[196] In literal translation, Biot's epigram is wordier: Cavendish was "the richest of all the learned and probably also the most learned of all the rich." Biot (1813, 273).

[197] "Cavendish, Henry," *Encyclopaedia Britannica*, 9th ed., vol. 5 (New York, 1878), 271–272, on 271.

[198] He had no money in Consolidated 3% Annuities and Bank Stock. It is possible that he had a small investment in other issues.

[199] The Elizabeth Cavendish inheritance of stocks and mortgages was legally transferred to Henry Cavendish after his father's death. Lord Camden who was named with Lord Charles executor of Elizabeth Cavendish's will agreed to transfer to Henry Cavendish the £75,000 in 3% annuities and the £22,100 in reduced annuities together with mortgages worth just under £50,000. "Lord Camden and the Honourable Henry Cavendish. Assignment and Deed of Indemnity, 31 Dec. 1783, Devon. Coll., 88/66.

Bank Stock. Lord Charles had £25,815 in Bank Stock.[200] Cavendish did not touch this fund, which at his death was worth £71,120. At that time, it represented about $8\frac{1}{3}$% of the value of his funds.

Reduced 3% Annuities. In October 1783, Cavendish received £18,285 from his father's estate, which he added to his own holdings, £14,500. In January, he received £22,100 from Elizabeth Cavendish's estate. Between 16 January 1782 and 5 [?] 1783, he sold £8500 of this, leaving £58,385 in his account.[201] The value of the fund on several dates gives a picture of its growth:

- 5 April 1785. £58,385.
- 13 June 1788. £86,000.[202]
- 2 November 1791. £115,000.[203]
- 5 April 1801. £216,504.[204]
- 5 July 1805. £281,528.[205]
- 5 April 1807. £347,809.[206]
- 1810, at his death. £433,852.[207]

Consolidated 3% Annuities. In 1782, Lord Charles held £47,100 of this stock. On 3 September of that year, he added £7000, and on 3 December, £8000, giving a total at the beginning of 1783 of £62,100.[208] The value of Henry Cavendish's account in this stock was:

- 22 October 1783. £62,100. From Lord Charles.
- 7 January 1784. £137,100. The increase came from Elizabeth Cavendish, £50,000, and her husband Richard Chandler Cavendish, £25,000.[209]
- 15 August 1786. £145,000.[210]
- 2 November 1791. £172,600.[211]
- 17 November 1796. £240,739.[212]
- 12 April 1802. £322,857.[213]
- 9 September 1808. £505,000.[214]

The last figure was the value of this fund when Cavendish died. He never sold any of this stock.

New South Sea Annuities. At his death, Lord Charles had £48,900 in this fund.[215] From October 1776, Cavendish had £1100 in it. On 23 August 1783, £872 was deposited by the

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[200] Bank Stock 1783–1798, Bank of England Archive, No. 59, p. 389.
[201] Reduced 3% Annuities, Bank of England Archive, Supplement Ledger 1781–1785, p. 10614.
[202] Ibid., Ledger 1785–1793, p. 1505.
[203] Ibid., p. 2242.
[204] Ibid., Ledger 1793–1801, p. 1727.
[205] Ibid., Ledger 1801–1807, p. 1801.
[206] Ibid., p. 1937.
[207] Ibid., Ledger 1807–1818, pp. 4449–4450.
[208] Consolidated £ 3%, Bank of England Archive, 1782–1788, p. 3854.
[209] Ibid., p. 3927.
[210] Ibid.
[211] Ibid., 1788–1792, p. 8000.
[212] Ibid., 1792–1798, p. 8730.
[213] Ibid., 1799–1804 (part 1), p. 8001.
[214] Ibid., 1804–1812, p. 8001.
[215] New South Sea Annuities, Bank of England Archive, 1776–1793, vol. 154, p. 65.

earl of Hardwicke, a relative on his mother's side. By 5 July 1793, the value of his account had increased to £59,255, where it remained to the end of his life.

As in other ways, in matters of finance Cavendish followed his father's course, investing in gilt-edged securities and almost never touching them. Shortly before his father's death, when he was establishing an independent life and considering buying properties, he sold a small part of his securities, receiving £8500 for them, but that was the exception. During the Napoleonic Wars, the government offered a higher return on loans and very substantial bonuses as a percentage on capital on top of the half yearly dividends,[216] but we see that throughout the time after his father's death Cavendish's account rose fairly steadily.

On the day Cavendish died, 24 February 1810, his personal property was worth the following:

1. Stocks. He owned shares in ten funds. On face value, they were worth £1,080,681. Their market value at that date was £821,050. Three quarters of the value were in two stocks, Reduced 3% Annuities and Consolidated 3% Annuities.
2. Funds held in trust. All of these stocks and annuities stand in the names of Cavendish's first cousins the earl of Hardwicke, Lord George Augustus Cavendish, and Lord Frederick Cavendish. There were five funds, with face value £21,755 and actual value £17,832. Most of the value was in one fund, Old South Sea Annuities.
3. Mortgages. He had three mortgages, worth £48,000.
4. Balance in banker's hands. £11,373.[217]

Apart from his funds, Cavendish's wealth at the end of his life consisted of his land and his houses at Clapham Common and Bedford Square together with their contents, and probably other property.[218]

Cavendish's worth was in line with great fortunes in the eighteenth century. Lady Bute was said to have inherited around £800,000 in 1761 from her father, E. Wortley Montague. Lord Bath was said to have left £1,200,000 at his death in 1764. Sir Samuel Fludyer was said to be worth £900,000 in 1767.[219]

In his biography of Cavendish, Wilson gives an account of an exchange between Cavendish and his banker. The banker called on Cavendish unannounced, and Cavendish's displeasure at the interruption is the point of the story. However, the beginning of the story is relevant here: "The bankers where he kept his accounts, in looking over their affairs, found he had a considerable sum in their hands, some say nearly eighty thousand pounds, and one of them said, that he did not think it right that it should lay so without investment."[220] The

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[216] Clapham (1945, 2:39–40, 46).

[217] "The Personal Property of the Hon. Henry Cavendish 24 February 1810," Devon. Coll., 114/74. The evaluation was from Mess^{rs} Snow & Co. The family obituary gave different figures for Cavendish's wealth: Cavendish "died worth 1,175,000*l* in different public funds, the value of which is estimated at 700,000*l*." This information was given to Wilson by a member of the family. Wilson quotes the above sentence, except that two digits are reversed: 1,157,000*l*. Wilson (1851, 176). The family obituary says that "50,000*l*, also were in the hands of his bankers," and Wilson repeats this. The discrepancy between the family's account of Cavendish's worth and what the bank documents say may have to do with the lapse between Cavendish's death and the time his funds were distributed to his heirs. The discrepancy in any case is not large, and the point is made that Cavendish had a great deal of money invested in funds at the time of his death.

[218] The family obituary says that at his death Cavendish had "freehold property about 8,000*l*. a year and canal and other personal property." Wilson quotes from the obituary (1851, 176).

[219] L.B. Namier (1929, 164).

[220] Wilson (1851, 175).

banker who called recommended investing £40,000, and Cavendish agreed. The amounts, £40,000 and £80,000, are plausible, as we see from receipts for purchases of funds. Cavendish was accustomed to buying additional stock in the same funds every year, but sometimes a large balance accumulated. The year 1788 is an example. On 13 June, he bought £18,000 of Reduced 3% Annuities, and on 7 July he bought £17,000 of Consolidated 3% Annuities, a total of £35,000. (He paid £13,500 and £12,580.) In 1791, he bought £35,000 of these same two funds. In 1805, he bought £51,000 of the same. In 1808, he bought £45,000 of Consolidated 3% Annuities.[221]

The story about the banker could give the wrong idea about Cavendish's management of his wealth. Take 1793, for example. At Chatsworth, there is a bundle of receipts for purchases in March and April. The first of these reads:

- Messrs Denne & Co. 25 March 1793. Please to layout the sum of twenty-six thousand pounds in the purchase of four different stocks as under & charge to my account. H. Cavendish
- Old South Sea £26,000
- New South
- Cons.: & Red.

Cavendish's order produced the following transactions. On 26 March, he bought £8400 of Consolidated 3% Annuities and £4000 of New South Sea Annuities. On 30 March, he bought £4383 of New South Sea Annuities. On 20 April, he bought £8333 of Reduced 3% Annuities and £5000 of Old South Sea Annuities. On 24 April, he bought £2000 of the same. On 26 April, he bought £1370 of Reduced 3% Annuities. The total came to £33,486. Cavendish paid the actual value, which was below par, plus commission, £25,965, which is just under the £26,000 Cavendish specified.[222] After receiving a purchase order like this from Cavendish, Robert Snow, his main contact with his banker Messrs. Denne & Co., would write to him, "Agreeable to your order of the [date], we yesterday purchased [the amounts and the funds]…," closing with, "This sum is as near the order as possible to keep the stock in even sums." Cavendish's directions were straightforward and consistent; his dividends were reinvested alternately in four securities: new and old South Sea annuities and consols and reduced 3% annuities, primarily in the latter two.[223] His farm and other rents went directly to his bankers, and his business was transacted through them. He had enough wealth that he did not have to spend much time with it, an ideal life which he did not want disturbed by house calls from his bankers.

As Biot said, Cavendish was the richest of the wise, and insofar as his investments were concerned, he was at least one of the wiser of the rich; over the long run, during the years in which he amassed his fortune, he could hardly have managed his inheritance better than to reinvest its earnings in consols and reduced 3% annuities, especially since he was a man who had other things to do with his days than to spend them in his counting house.

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[221] Bundle of receipts for purchases of annuities, Devon. Coll., 86/comp. 3.

[222] "1790–1816. Accs. & Receipts. Case & Opinions," ibid.

[223] Correspondence from Cavendish's bankers, ibid.

Religion

From what Wilson was able to learn from persons who had known him, Cavendish "separated himself… apparently from God." The qualification "apparently" would seem to have referred to Everard Home's account of Cavendish on his deathbed: "Cavendish sent his servant out of the house, 'ordering him not to come near him till night, as he had something particular to engage his thoughts, and did not wish to be disturbed by any one!'" Wilson said that he "would willingly believe that the 'something particular,' which he told his servant was to engage the undisturbed attention of his last, and solemn, silent hours, was his preparation for the unseen world into which he knew he was about to pass."[224] Being a deeply religious man himself, Wilson wanted to believe that Cavendish saw the spiritual truth too. Let us consider the evidence for his belief.

In the one published comment on Cavendish's religious persuasion, Biot wrote that Cavendish was "religious in the manner of Locke and Newton." Wilson assumed that Biot had some authority on this point, but he considered his statement to be ambiguous. Because Cavendish showed none of the earnestness of Newton and Locke on the subject of religion, Biot would have had in mind religious doctrine not religious fervor, and as such his statement was ambiguous, since at the time Newton's position on the doctrine of the Trinity was uncertain. Wilson supposed that Biot intended to say that Cavendish's religious views resembled Newton's only in the sense that they were unorthodox, probably Arian or Unitarian. He was told that at Cavendish's college in Cambridge there was a kind of hereditary belief that he was a Unitarian, but he could find no foundation for it.[225]

In the last two decades of his life, as we have seen, Cavendish shared Clapham Common with evangelical members of the Church of England known as the Clapham Sect, who were distinguished for their spiritual intensity. They were troubled by what people did on Sundays, which they insisted should be dedicated to quiet devotion.[226] At a meeting in 1798, the inhabitants of Clapham parish agreed unanimously that in the interest of both the individual Christian and civil society, it was "highly improper, on that Day [Sunday], to exercise our worldly occupations, to travel, except in cases of urgency, or for purposes of benevolence, or to employ our domestics in any thing interfering with their public or private religious duties."[227] In this way, the evangelicals imposed on Clapham Common the quiet contemplation of the life to come, known later as the Victorian Sunday. There was a call for the prosecution of violators. Wilson noted that Cavendish's decisive experiment on the composition of water was done on a Sunday. We add that Cavendish performed the fifth part of his experiment on the density of the Earth on a Sunday in 1797. He treated Sunday like any other day of the week; he worked, doing what he always did. He had no known run-ins with his evangelical neighbors. After his death, his house had a brush with the movement; John Thornton, son of Samuel, a member of the Clapham Sect, lived in the house for a few years.

Wilson received a few comments on religion from his inquiries. A member of the Cavendish family heard his grandmother say that Cavendish once came to a christening, but he may only have stayed for dinner. A fellow of the Royal Society said that "as to

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[224] Wilson (1851, 184–185).

[225] Biot (1813, 273). Wilson (1851, 180–181). Privately, Newton held a Unitarian view.

[226] R. de M. Rudolph (1927, 89).

[227] *Resolution Agreed to by the Inhabitants of Clapham for the Better Observance of the Lord's Day, 1798.*

Cavendish's religion, he was nothing at all." A neighbor of Cavendish's at Clapham believed that he "never attended a place of worship."[228] Other than to list a church as a landmark in taking bearings, his one known reference to a church occurred during a dinner of the Royal Society Club, when he said that some wood at Clapham Church was eaten "thro' by the insects… working their way out."[229] In the absence of any outward display of interest in religion on Cavendish's part, Wilson concluded "that the World to come did not engross his thoughts."

Newton wrote in the *Principia* that the discussion of God "does certainly belong to Natural Philosophy." In the previous edition of this biography, which I prepared with Christa Jungnickel, we said correctly that Cavendish did not record any thoughts on religion in his writings on natural philosophy. We did not mention in this connection Cavendish's contribution to the University of Cambridge's volume of lamentations in Latin in honor of the crown prince Frederick, to which I now give more weight. It was Cavendish's first publication, and because it is his only publication on a subject other than science, it holds an interest for us. The poem follows form for memorials of this kind, but it is also revealing of its author. Cavendish writes that by understanding nature we can understand the occasion of the lament, the prince's death. Nature has nothing to do with human comforts and desires. Libitina, goddess of death, "spares no beauty, no youth, no faith," but to the "intimate" of nature, by which I take him to mean the student of nature, "natural truth" is disclosed, and what is disclosed is the destination of the royal prince, "a dweller in heaven."[230] Cavendish may have had in mind natural religion or a version of religion close to it, certainly a religion without the notion of a personal God, though one that seemed to promise an afterlife. Cavendish was only eighteen when his poem was published, and as he matured his thoughts on religion may have changed, or never returned. He may have rejected religion altogether, an impression he gave the world, or he may have rejected only its social forms. Because after his youthful poem, he wrote nothing again on the subject, we cannot know his subsequent religious leanings, if he had any.

The End

The later years of Cavendish's life were ones of peril for the nation. He met with men of science as always, at the Royal Society, at his clubs, and at Banks's house, but the talk was now often more about politics, impending war, and battles than about science. In the year Cavendish weighed the world, the Council of the Society put to the ballot a motion to pay £500 to the Bank of England "as a voluntary contribution towards the defense of the country at this critical period.[231] Blagden's diary, a main source of information about Cavendish's comings and goings during these years, is mainly concerned with the general agitation, when it is not about his private agitation over Madam Lavoisier or his difficulty in getting a passport to return to France. There is little about science. Even Cavendish was caught up in the events of the world at large. At the Royal Society, he said "that if Pitt came in against K[ing]'s inclinations, the K. if quite well, wod soon find the means of getting him

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[228] From Lord Burlington, in Wilson (1851, 181).

[229] 19 Feb. 1807, Charles Blagden Diary, Royal Society 5:39.

[230] Henry Cavendish (1751).

[231] 22 Feb. 1798, Minutes of Council, Royal Society 7:353.

out again"; to Blagden's observation that North Germany was then quiet, Cavendish "still thought Holstein wod be attacked at some moment."[232]

Toward the end of his life, as at any time during it, what was most conspicuous about Cavendish was his steadfast desire to learn more about and to practice science. When Blagden was given a paper by Herschel to look over, he knew Cavendish was the "best person" to read it,[233] and this was one year before Cavendish died. Two months before he died, Cavendish told Blagden that he had "doubts about some part of Malus's paper, & did not know if [he] understood it."[234] The French physicist Étienne Louis Malus had just begun publishing his important work on optics, and Cavendish was following it. In the last year of his life, Cavendish saw much of Davy. At one point Davy thought he had converted azote (nitrogen) into oxygen, an extraordinary finding if true. Blagden reported that "Mr.Cavendish has gone thro' the experiment with him [Davy], & detects no source of fallacy"; he was "quite satisfied that the gases convertible," seeing "no way of explaining Davy's expt but by conversion of nitrogen."[235] Cavendish was actively following and in this case aiding in Davy's researches in chemistry. As late as 1806, he was still doing experiments of his own in chemistry, undertaking a long series on platina that year.[236]

In their few surviving letters, Henry and his brother Frederick addressed one another as "Dear Brother," and Frederick closed his letters with "your affectionate brother." Henry was "alarmed" upon hearing on good authority that Frederick was ill, but Frederick reassured him that he had never felt better other than for the gout that cramped his handwriting, keeping occupied "as usual visiting my friends or riding out most days."[237] Frederick lived in Market Street, as he had from about age forty, first in the home of a clergyman, then in a small house and later in a larger house of his own, attended by two "confidential domestics." This was a quiet village in Hertfordshire, just across the border from Bedfordshire, near the Benedictine Monastery of St. Albans, and there is a brief letter from Henry to Frederick setting a time to meet with him at "St Albans."[238] Frederick spent much of his time visiting in the neighborhood, where he was regarded as a harmless eccentric. He was a skillful drawer of leaves and other natural objects and fond of displaying his portfolios, which he intended to leave to the British Museum (he did not). He had a large library of classics in literature, which he read and remembered, reciting poetry with such accuracy that he was called a "living edition." His preferences among the modern poets, such as Thomson, Akenside, and Mason, were thought to be influenced by their politics. Extremely proud of his family, he often quoted the epitaph of the first duke of Devonshire, friend of good princes and enemy of tyrants. With his bag wig, cocked hat, and deep ruffles, Frederick in his later years was a quaint relic. Whig, bookish, unfashionable, unmarried, without a profession, proud of his family name, in several respects Frederick resembled his brother Henry. In other respects he differed; he was drawn to literature and art instead of to science, and to society rather than to solitude, having a "very social disposition."[239]

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[232] 26 Mar. 1804, Charles Blagden Diary, Royal Society 5:214.

[233] 16 Feb. 1809, ibid. 5:286.

[234] 3 Dec. 1809, ibid. 5:396(back).

[235] Charles Blagden to Richard Chenevix, 1 May 1809, draft, Blagden Letters, Royal Society, C.35.

[236] In January 1806, for example: "White Book No. 1," 68.

[237] Henry Cavendish to Frederick Cavendish, n.d., draft; Frederick Cavendish to Henry Cavendish, 10 Sep. 1809; in Russell McCormmach (2014, 260).

[238] Henry Cavendish to Frederick Cavendish, n.d. [1784], draft, Cavendish Mss, Misc.

[239] "Memoirs of the Late Frederick Cavendish, Esquire," *Gentleman's Magazine* 82 (1812): 289–291.

Frederick was known to be a soft touch. One of his last letters to Henry is about a young married man who was just getting started and needed £150 to pay off his upholsterer's bill. Frederick asked Henry for this amount, since he did not have it, "confident [it] will do a great deal of good."[240] Henry obliged him. When Frederick exceeded his modest income, he asked Henry for money.[241] He needed help with his taxes, which were then, as ever, baffling. Henry was sympathetic: "the printed forms sent both by the commissioners of Income & assessed taxes are intricate & not clearly expressed."[242] On his side, Frederick was mindful of his brother's interests: "As I believe you attend a good deal to the observation of the barometer," he sent Henry a careful account of his reading of the barometer that morning. Frederick was two years younger than Henry, and he outlived him by two years. The life span in this branch of the Cavendishes was long and remarkably constant: the three of them, Charles, Henry, and Frederick, lived to the age of seventy-eight and seventy-nine.

Up to the end Henry Cavendish was vigorous, physically and mentally. His physician was John Hunter, whom we hear of in that capacity for the first time in 1792, when Cavendish was sixty. Blagden went to Clapham Common only to be told that Cavendish was ill. He responded with sympathy (and perhaps hurt): "If you had chosen that I should wait upon you, I cannot doubt but you would have sent to me."[243] That same day upon learning that Cavendish was being seen by Dr. Hunter, he wrote again to Cavendish to say that he "could not do better" and to ask only if he could visit him "as a friend."[244] Blagden told Banks the next day that he was "engaged to be with Mr. Cavendish (who is much indisposed) at Clapham."[245] We know what was wrong with Cavendish from another friend, Alexander Dalrymple, who sent a sympathy note to him together with a folk remedy: he was "very sorry yesterday to hear that You were prevented from coming amongst us by an attack of the Gravel."[246] Gravel, a common complaint then, meant painful or difficult urination possibly caused by a deposit of urinary crystals.

Because there was a famous contemporary surgeon and anatomist named John Hunter, we need to point out that Cavendish's doctor was not that John Hunter. He is not well known today, but at the time he was (Fig. 12.4). When he was proposed for membership in the Royal Society in 1785, his certificate was signed by twenty-five fellows,[247] which was the same number James Cook received ten years before in an extraordinary expression of support. Cavendish was one of the signers, along with Cavendish's colleagues, Dalrymple, Aubert, Heberden, Blagden, Nairne, Smeaton, Maskelyne, and others including the other John Hunter. Hunter, then a physician to the army, was according to his certificate "well versed in various branches of natural knowledge." A graduate of the University of Edinburgh, his writings on medicine show that he followed the teachings of William Cullen. His dissertation in 1775 was unusual because of its subject, anthropology, but just as he has been eclipsed by his namesake, his dissertation has been eclipsed by a better-known work on the

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[240] Frederick Cavendish to Henry Cavendish, 5 and 12 Feb. 1810; in McCormmach (2014, 61).

[241] Frederick Cavendish to Henry Cavendish, 9 Feb. 1810, ibid.

[242] Frederick Cavendish to Henry Cavendish, 28 Oct. 1806; Henry Cavendish to Frederick Cavendish, n.d., draft, ibid., 259–260.

[243] Charles Blagden to Henry Cavendish, 12 Mar. 1792, draft; in Jungnickel and McCormmach (1999, 689).

[244] Charles Blagden to Henry Cavendish, 12 Mar. 1792, draft, ibid., 690.

[245] Charles Blagden to Joseph Banks, 13 Mar. 1792, draft, Blagden Letters, Royal Society 7:626.

[246] Alexander Dalrymple to Henry Cavendish, 16 Mar. 1792; in Jungnickel and McCormmach (1999, 691).

[247] 12 Jan. 1786, Certificates, Royal Society 5.

subject appearing in the same year by J.F. Blumenbach.[248] Hunter regarded humans as a species, circumscribed within limits by Divine Wisdom, and the differences among them as varieties; in this respect, humans were like plants, butterflies, and shell creatures, in which natural history took greater interest. He had no need for the Scriptural explanation of Cain as the father of the blacks, nor need for a Deity to explain differences in mental faculties. He looked instead to "natural causes" to explain differences in human color, stature, parts, and minds. One of the principal natural causes of such differences was "heat," which is where his path crossed Cavendish's.[249] Before Hunter set sail for Jamaica in 1780 to superintend military hospitals, Cavendish suggested that he observe the heat of springs and wells while he was there. His paper on the subject, appearing in the *Philosophical Transactions* for 1788, gave a full account of Cavendish's hypothesis: assuming that the heat of the Earth comes solely from the Sun, not from the Earth's interior, precise measurements of the temperature underground, where the temperature remains constant through the seasons, ought to provide the mean temperature of any climate; in this way a few observations of the heat of springs and wells could be as informative as "meteorological observations of several years."[250] Hunter included this discussion in his main publication, *Observations on the Diseases of the Army in Jamaica*.[251] Other publications of his appeared in medical journals, but the judgment on his work is that it did not live up to its early promise. When he died at the age of fifty-four, in 1809, the year before his famous patient Cavendish died, he had not published any new work in over ten years.[252]

From Blagden we learn of Cavendish's next illness. Cavendish came faithfully to Banks's open houses, so when he was absent one Sunday in 1804 Blagden made note of it.[253] A few days later Blagden was informed that Cavendish was ill.[254] This time he was attended by the physician Everard Home, who told Blagden that Cavendish had a rupture, nothing more serious; he would need a truss, that was all. Home was about the same age as Cavendish's previous physician Hunter, and had served at the same time as Hunter with the army in Jamaica; the two were well acquainted, both active members of a medical club founded in 1783 which met at Slaughter's Coffee House.[255] By the time Cavendish called on his services, Home was eminent both professionally and scientifically. He had succeeded the anatomist John Hunter as surgeon to St. George's, and he was known as a prolific writer on surgical and anatomical subjects. Cavendish would have met him at the Royal Society, where he repeatedly was chosen to give the Croonian lectures.[256] With Home, as with Hunter, Cavendish formed a scientific as well as a medical connection, performing an op-

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[248] Blumenbach's *De generis humani varietate nativa* was translated by T. Bendyshe and published together with a translation of Hunter's inaugural dissertation, *Disputatio inauguralis quaedam de Hominum varietatibus, et harum causis exponens* ...(Edinburgh, 1775) in *The Anthropological Treatises of Johann Friedrich Blumenbach* [...] *and the Inaugural Dissertation of John Hunter, MD On the Varieties of Man* (London, 1865).

[249] Hunter, On the Varieties of Man, 365–368, 378.

[250] John Hunter (1788, 53, 58, 65). Charles Blagden to William Farr, 21 Jan. 1788, draft, Blagden Letters, Royal Society 7:107.

[251] Hunter included the paper from the *Philosophical Transactions* as an appendix to the second edition of his *Observations on the Diseases of the Army in Jamaica* (1796). The first edition was in the same year as the paper, 1788.

[252] Lise Wilkinson (1982, 235–236).

[253] 12 Feb. 1804, Charles Blagden Diary, Royal Society 4:201.

[254] 16 Feb. 1804, ibid. 4:202(back).

[255] The Society for the Improvement of Medical and Chirurgical Knowledge, whose leading member was the "other" John Hunter. Wilkinson (1982, 234)

[256] William LeFanu (1972).

tical experiment on the cornea in response to a paper by Home.[257] Home would remain Cavendish's physician to the end.

When Cavendish had his rupture, Home told Blagden that the disorder began with a swelling of the legs: "as if old the first time," Blagden wrote in his diary that day.[258] Cavendish was ill on 16 and 17 February 1804, and Blagden went to see him on the 18th, on which day Cavendish made out his final will, though it seems he did not show it to Blagden.[259] Either Home or Blagden, or both, evidently had an insight. Cavendish was seventy-two, and he had an intimation of death. On a day when the Royal Society Club met in 1807, Blagden recorded in his diary, "Spoke to Cav. about parallax of fixed stars; it seemed as if he began to forget."[260] Cavendish was perhaps a bit forgetful, but after a meeting of the Council of the Royal Society in 1809, eight months before he died, Blagden wrote that he "looked in excellent health."[261]

Within natural philosophy, Cavendish's breadth of competence was impressive, but as a sensible and observant man, he recognized that he knew only some things well and that other persons knew other things well. He declined to advise Bristol on its sewage problem partly on the grounds that "physicians" knew more about health and "engineers" knew more about rivers than he did. Physicians, engineers, and the men of science came together in clubs and societies based upon what they knew better than other people. With one exception, Cavendish did not take part in them. The Society of Civil Engineers, centering on Cavendish's colleague John Smeaton, was founded in 1771 and reorganized in 1792; honorary or regular members included colleagues of Cavendish's such as Banks, Rumford, Hatchett, James Cockshutt, and Charles Greville, but not Cavendish himself. He was not a member of the patriotic Society for the Improvements of Naval Architecture, founded in 1791, which brought together practical men and certain men of science who were colleagues of Cavendish's such as Banks, Hatchett, Aubert, Maskelyne, and Hutton.[262] He did not belong to the Linnean Society, founded in 1788, nor would we have expected him to; but he did not belong to the Mineralogical Society, founded in 1799, or the Geological Society, founded in 1807, though mineralogy and geology were favorite subjects of his. Near the end of his life, a number of small, private chemical societies were founded in and around London: the London Chemical Society, announced in 1807 by Friedrich Accum, a chemistry teacher and briefly Davy's assistant at the Royal Institution; the Lambeth Chemical Society, launched around 1809; and the Society for the Improvement of Animal Chemistry begun in the same year.[263]

The Society for the Improvement of Animal Chemistry had a close connection with the Royal Society, as is clear from the founding resolution at a meeting of the Council of the Royal Society in April 1809. The new society was designated an "assistant society," in no

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[257] In 1795 Blagden sent Cavendish a paper by Home. Evidently the paper contained Home's account of what would have appeared in John Hunter's Croonian Lecture if he had not died before he could give it. Everard Home (1794). Hunter believed that the cornea can adjust itself by its own internal actions to focus the eye at different distances. Cavendish, assisted by Blagden, performed an experiment to detect changes in the convexity of the cornea accompanying changes in the focus, using a divided object-glass micrometer. Entries for 8, 11, and 16 Nov. 1795, Charles Blagden Diary, Royal Society 3:75(back), 76, and 77(back).

[258] 17 Feb. 1804, Charles Blagden Diary, Royal Society 4:202(back), 203.

[259] "Copy of the Will of Henry Cavendish Esq.," In "Account of the Executor of Henry Cavendish Esq. as to Money in the Funds," Devon. Coll., L/31/65.

[260] 4 June 1807, Charles Blagden Diary, Royal Society 5:76.

[261] 8 June 1809, ibid. 5:328(back).

[262] Gwendoline Averly (1989, 26–29).

[263] Gwendoline Averley (1986, 102, 108–109, 113).

sense in competition with the original. To underscore the continuity with the host society, and to add prestige to the new, at the same meeting the Council resolved "that Mr.Cavendish be requested to allow his name to be added to those of the members of this new society."[264] The meetings, which took the form of dinners and conversation every three months, were held alternately at the house of Cavendish's doctor, Home, and at the house of his collaborator, Hatchett. Other members included Davy, William Thomas Brande (who would succeed Davy as professor of chemistry at the Royal Institution), the physician William Babington (one of the founders of the Geological Society), and the physician Benjamin Collins Brodie (the outstanding pupil of Home's).[265] Later the Society turned into a dinner club, but at the beginning it was given to serious scientific discussion. In 1809, the year of its founding, the Society sponsored two papers printed in the *Philosophical Transactions*, one by Home and one by Brande, both electrochemical. Homes's paper continued the study of the electric eel or torpedo, Cavendish's subject; it is revealing of the change in science that Cavendish heard Home describe the torpedo as a "Voltaic battery" instead of Cavendish's battery of Leiden jars, the torpedo having become a problem addressed by a chemical society.[266]

If Cavendish came to the few meetings of the Society for the Improvement of Animal Chemistry before his death, he would have been an interested party to the discussions. He had given considerable thought to plant and animal substances in his study of putrefaction and fermentation in his first paper on pneumatic chemistry in 1766. In his study of the phlogistication of air in 1784, he based his preference for phlogiston theory over the new chemistry on the greater complexity of a living plant over a burnt one. His active interest in living things was directed to what they had in common with non-living things, such as the electricity of the torpedo. His young colleague James Lewis Macie offered him an appropriate problem: to determine the density of tabasheer, a rock-like substance found in the joints of tropical bamboo, which for the product of a plant had improbable properties (Fig. 17.8). Macie found it to be indestructible by fire, totally resistant to acids, and glass-like when fused with an alkali, concluding correctly that it was "siliceous earth." Tiny specimens of tabasheer were given to Cavendish, who took "great care" in weighing them in water.[267]

The Society for the Improvement of Animal Chemistry was the only specialized society Cavendish belonged to, and as an extension of the Royal Society, it was a special case, Cavendish being included as an honorary member on the initiative of the active members. His distance from specialized societies might be explained by his age, but he was vigorous; in 1805 Banks proposed to augment the Board of Longitude and to include Cavendish.[268] The most likely explanation is that specialized societies largely belonged to a different stage of science, emerging together with the professional identity of the scientific expert. Cavendish was content with the national scientific body, the Royal Society, which acknowledged specialized skills in the membership of its committees.

To the end Cavendish was fully active in the work of the Society, as shown by his agreement to superintend the construction of an apparatus for measuring the temperature at different depths of the sea. He did not have time to oversee the experiment.[269] He attended

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[264] 27 Apr. 1809, Minutes of Council, Royal Society 7:527–31.

[265] Benjamin Collins Brodie (1865, 88–92).

[266] Everard Home (1809, 386).

[267] James Lewis Macie (1791, 370, 384–385, 388).

[268] 23 Feb. 1805, Charles Blagden Diary, Royal Society 4:313.

[269] Joseph Banks to William Scoresby, Jr., 8 Sep. 1810, copy, Whitby Literary and Philosophical Society.

Council the last time on 21 December 1809, missing only one meeting, on 15 February 1810. Henry Cavendish died on 24 February 1810.

The several accounts of Cavendish's last days vary but agree in this particular: he was fully conscious and resigned to the imminent end. The account most at variance with the others was given by Home to John Barrow, who published it long after the event. It is also the most likely. When one of Cavendish's servants came to Home to say that Cavendish was dying, Home went directly to Clapham, finding Cavendish "rather surprised" to see him. His servant should not have bothered him, Cavendish said, since he was dying, and there was no point in prolonging the misery. Home stayed all night at Cavendish's bedside. Through it all Cavendish was calm, and shortly after dawn he died.[270]

Home was certainly there, as we know from an entry in Blagden's diary from the time. Heberden would seem to have been there too, as we know from Lord George Cavendish, who as Cavendish's executor paid his fee as well as Home's.[271] This Heberden was William Heberden, son of Charles and Henry Cavendish's old friend, who had died in 1801. The younger Heberden, who was as distinguished as his father, being physician in ordinary to the king and queen, prescribed neutral salts, which Cavendish could not keep down. At Banks's house, where Blagden learned of Cavendish's death, Home gave him an "affecting account" of Cavendish the previous day. There was a "shortness of questionings," Home said; Cavendish "seemed to have nothing to say, nor to think of any one with request." He told Home "it is all over, with unusual cheerfulness, & at parting wished Home good by with uncommon mildness." Cavendish ordered that his heir Lord George Cavendish "be sent for as soon as the breath was out of his body, but not before."[272] Home, who had treated Cavendish six years before for a rupture, told Blagden that the rupture had nothing to do with Cavendish's death, even though he evidently had refused to wear a truss. Cavendish had an "inflammation of the colon," which for the past year had caused diarrhea and which in the end obstructed the passage of food.[273] Banks lamented the loss to science, but that was all; he "felt nothing." Blagden, by contrast, was moved, noting in his diary that he "continued all day to feel the effect of this event on my spirits." He also noted that it was a cloudy, threatening day, as if a mirror to his spirits.[274] Two weeks later Blagden watched from his window the "funeral procession of my late friend; with much emotion."[275]

We now pass to another, all-too-human emotion. Cavendish's fortune was on everyone's mind, including his physician Home's; on the morning Cavendish died, Home had Cavendish's servant give him the keys, with which he prowled through the house opening drawers, trunks, and cupboards looking for treasures, which he found and noted.[276] In a few days word was out that no will had been located. Blagden had seen it but not "since the time I was intimate with him," and he thought that Cavendish had probably changed it since then.[277] Blagden told the company at Banks's that Cavendish's income was above £40,000 a year. Because Cavendish was not a "person who gave the £40,000 to hospitals,"

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[270] John Barrow (1849, 153–154).

[271] Heberden gave a prescription. 25 Feb. 1810, Charles Blagden Diary, Royal Society 5:426(back), 427. Home's fee was £105, Heberden's £21. Lord George Cavendish, "Mr.Cavendish's Executorship Agenda," Devon. Coll.

[272] 25 Feb. 1810, Charles Blagden Diary, Royal Society 5:426(back), 427.

[273] 4 Mar. 1810, ibid. 5:429(back), 430.

[274] 24 Feb. 1810, ibid. 5:426, 426(back).

[275] 8 March 1810, ibid. 5:431(back), 432.

[276] Barrow (1849, 154–155).

[277] 1 Mar. 1810, Charles Blagden Diary, Royal Society 5:428(back).

and because he did not spend more than £5000 a year he had to have left a fortune.[278] In good time the will was found. Of the funds, valued at over £800,000 on the market, as we have seen, one sixth went to Frederick Ponsonby, third earl of Bessborough, and five sixths to his executor Lord George (Augustus Henry) Cavendish and his family; the latter was apportioned into two sixths for Lord George and one sixth each for Lord George's three sons, William and, still minors, the namesakes of our branch of the family, Henry and Charles. At the Sunday soirée at Banks's house, a gossip told Blagden that "Lord George Cavendish courted Henry Cavendish abundantly."[279] If he did, it was unnecessary. Both Charles and Henry Cavendish had a history of dealing with Lord George over property, and Henry having early on decided on him as his principal heir met with him once a year for a half-hour or so.[280] Lord George had married sensibly and was rich even by Cavendish standards; Henry Cavendish's legacy had nothing to do with need but only with principle and, within rather narrow limits, preference. The dukedom would eventually revert to Lord George's descendents, an eventuality Henry Cavendish might well have considered.

Apart from his brother, Henry had outlived his own generation of Cavendishes. In the next generation, there were seven prospective male heirs, two of whom Henry named in his will, Lord George Cavendish, who as his main heir probably surprised no one, and Frederick, third earl of Bessborough, son of Caroline Cavendish, daughter of the third duke of Devonshire. Cavendish is said to have enriched Bessborough because he was pleased by his conversation, and that may well have been. Bessborough and Cavendish met often at the British Museum, where Bessborough was an active trustee, serving on the standing committee and attending meetings regularly. In the last years they also met at the Royal Institution, where they were both managers. Because of their family connections, they both visited Devonshire House, where Cavendish heard talk about Bessborough's quick and capable drawings of Italy. Unambitious politically, Bessborough declined office under the Grenville ministry. His biography in the *History of Parliament* describes him as "a man of little political consequence." Henry Cavendish did not consider this a disqualification of an heir of his.[281]

The last five living male Cavendishes of the next-generation were Horatio, George, and Robert Walpole, sons of Rachael Cavendish and Horatio Walpole; George Ponsonby, son of Elizabeth Cavendish and John Ponsonby; and William Cavendish, fifth duke of Devonshire, the older brother of Cavendish's main heir, George Cavendish. We have no indication that Henry Cavendish associated with the Walpole brothers, and nothing suggests that their paths would have crossed, but we note that the great political connection between the Walpoles and the Cavendishes at the time of the second duke had been replaced by a connection with the Walpoles through marriage. Horatio Walpole was a Whig Member of Parliament for about thirty years, during which time he gave only one speech, and he seems to have left little imprint.[282] George Walpole was a major general and a Whig Member of Parliament for twenty-three years, and though not a cabinet member he held a number of offices, evidence of a respectable political career. George Ponsonby, lawyer and Whig Member of Parliament

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[278] 1 and 2 Mar. 1810, ibid. 5:428 (back), 429.

[279] 17 Sep. 1809, ibid. 5:330.

[280] Wilson (1851, 173).

[281] Wilson said of Lord Bessborough that Cavendish "was not, I believe, a connexion of his." He missed the family connection, though it was close. Wilson (1851, 190). 1 Sep. 1794, Charles Blagden Diary, Royal Society 3:14. J.M. Collinge (2016)

[282] R.G. Thorne (2016b).

for about fifteen years near the end of his life, was the son of the speaker of the Irish House of Commons and served over twenty years in the Irish Parliament. He was said to be a man of unimpeachable integrity, however "a slow, and, in politics, a timid and narrow-minded man."[283] Again his and Henry Cavendish's paths were unlikely to have crossed. One obvious *Cavendish* who was not in Henry's will was, formally speaking, the first and most expectant Cavendish, the tenant for life of the vast family estate, the fifth duke of Devonshire. Lady Sarah Spencer speculated on why Henry Cavendish forgot the duke's existence in his will: perhaps Cavendish "thought that said existence was something of a *disgrace* to the noble name of Cavendish," and we have grounds for thinking he did. She did not regret that the duke gained nothing from Cavendish's death, since he and his heir, Hartington, were "*pretty well* off."[284] For his part, the duke was "quite convinced" that Cavendish would leave him nothing.[285] Resigned to nothing, he was said to be delighted to learn that Cavendish had left his money to the family, specifically to the earl of Bessborough. He was, however, "disgusted to see the disposal of so vast a property in a few lines, as if to save trouble."[286] We have seen many wills from the time and with the exception of his father's, none briefer or clearer than Henry Cavendish's. This would agree with Home's observation that on his deathbed Cavendish seemed to think of no one.

Grandson of Henry de Grey, duke of Kent, Henry Cavendish had three living male relatives of his own generation on the Grey side: John, second earl of Ashburnham, who was eighty-six and very infirm, and the brothers John William Edgerton, seventh earl of Bridgewater, and Francis Henry Edgerton, future eighth earl of Bridgewater. He had only one male relative of the next generation on the Grey side: George, future third earl of Ashburnham. The two earls of Bridgewater were fellows of the Royal Society, and Francis Henry, the eighth earl, is well known to historians of science as the founder of the *Bridgewater Treatises*, the authors of which were selected by the president of the Royal Society and the Bishop of London to demonstrate the "Power, Wisdom, and Goodness of God, as manifested in the Creation."[287] This clergyman was strongly interested in science but probably not in a way that would have brought him and Henry Cavendish together. Charles Cavendish kept a correspondence with his sister-in-law Lady Ashburnham, Jemima de Grey,[288] but we have come upon no record of contact between Henry Cavendish and the Ashburnham or Bridgewater families. Henry Cavendish would not have included his Grey relatives in his will in any case, since the source of his wealth was the Cavendish side of the family. His wealth would remain within the Cavendish family; his will made perfect sense, its surprises being minor variations on the standard theme.

Henry Cavendish's landed property was left to his brother, Frederick. This consisted of his fifteen-acre freehold estate on Clapham Common, which returned £200 a year in rent, and his farmland in Derbyshire and Nottinghamshire, which at the time of his death returned over £3000 a year. In 1784 Frederick made a will, which he did not revise, leaving his personal estate and his real estate in Market Street to his brother, Henry, but since he outlived Henry it went instead to his maternal first cousins, the earl of Ashburnham, the

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[283] R.G. Thorne (2016a).

[284] Lady Sarah Spencer quoted in Hugh Stokes (1917, 315, 350).

[285] Letter from the fifth duke's second wife, Elizabeth Foster, to Augustus Foster, 1 Mar. 1810, in Foster (1898, 345).

[286] Quotation from the "Journal" kept by the duchess of Devonshire, in Dorothy Margaret Stuart (1955, 174).

[287] Charles C. Gillispie (1959, 209).

[288] Henry Cavendish, "Papers in Walnut Cabinet," Cavendish Mss Misc.

earl of Bridgewater, and Francis Henry Edgerton, the earl of Bridgewater serving as his executor.[289] After Frederick, the estate in Derbyshire and Nottinghamshire reverted to the duke of Devonshire.

The funeral procession that Blagden watched from his window set out with the body from Clapham Common at seven in the morning on 8 March 1810. Five private carriages belonging to the duke of Devonshire and to Henry Cavendish's heirs, Lord George Cavendish, Lord Bessborough, and Lord George's oldest son, William, traveled northward through London on their way to Derby.[290] There they were met at the gates of the city by twenty-four burghers, twenty-four constables, and a retinue of city officials (all of whom were paid to do this) dressed in black. They then proceeded to the Church of All Saints, where Cavendish was buried in the family vault. The pomp and ceremony were invariable for the Cavendish dead, and it was elaborate and expensive. Everything had to be rented, the hearse and coach ornamented with black ostrich feathers and drawn by six horses, eight men on horses, and on and on. The bill for nine days came to about £750.[291]

In his will, Henry Cavendish left £15,000 to Blagden, and £5000 each to Dalrymple and Hunter, though both of them had already died. Some of Cavendish's "warmest admirers have expressed regret that no portion of that vast wealth was appropriated to scientific objects."[292] Blagden thought that Davy had expectations: "Davy said, Mr. C[avendish] has at least remembered one man of science [Blagden], in a tone of voice which expressed much."[293] It was rumored that Blagden was disappointed, having expected more,[294] but there is no indication of this in anything we have seen, including his frank diary. In the days following Cavendish's death, Blagden stood up for his old friend.

The scientific colleagues who gathered at Banks's house in the weeks following Cavendish's death had other concerns too. There was Cavendish's large library, which passed along with his other personal possessions to Lord George Cavendish. Blagden said that at some point Cavendish wanted his library not to be dispersed but to be kept accessible, as it had been in his lifetime.[295] No doubt there was talk about Cavendish's instruments and apparatus, for Davy was soon to be given his pick of them, while other pieces went to the instrument maker John Newman of Regent Street, son of the maker of Cavendish's wind-measurer. The remainder was sold at auction by Lord George.[296]

From the beginning, there was discussion of an edition of Cavendish's published works, but what to do about his unpublished papers was an open question.[297] Blagden thought that these papers would be found in a state unfit for publication, but Lord George Cavendish wanted Blagden to look over the papers anyway, and so on 6 April Blagden, Banks, and evidently other interested colleagues met with Lord George at Cavendish's house on Clapham

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[289] W. Ware to John Heaton, 27 Feb. 1810, Devon. Coll. "Memoirs of the Late Frederick Cavendish," 291.

[290] Lord Bessborough to Charles Blagden, 7 Mar. 1810, Blagden Letters, Royal Society, B.149.

[291] "Mr. Swift's Bill for Expenses at the Funeral of Hen: Cavendish Esq.," 29 Aug. 1810, Devon. Coll., L/114/74.

[292] "Cavendish, Henry," *Encyclopaedia Britannica*, 9th ed., 5:271.

[293] 8 Mar. 1810, Charles Blagden Diary, Royal Society 5:431(back), 432.

[294] Brougham (1845, 1:258).

[295] 3 and 4 Mar. 1810, Charles Blagden Diary, Royal Society 5:429, 429(back), 430.

[296] Wilson (1851, 475). *A Catalogue of Sundry Very Curious and Valuable Mathematical, Philosophical, and Optical Instruments*.

[297] This discussion of Cavendish's papers is taken from Russell McCormmach (1988, 37–38). The first mention of a proposed edition of Cavendish's works appears in the entry for 8 Mar. 1810 of Blagden's diary; other entries on this subject are on 10, 26, and 27 Mar. 5, 8–9, 11–12, and 26 Apr. and 24 May 1810, Charles Blagden Diary, Royal Society 5.

Common to inspect the manuscripts. After spending about four hours on them they decided that the papers were, for the most part, "only mathematics." Blagden returned to Cavendish's house, and for the next two weeks he was kept busy with the papers, after which he reported to Lord George:

> We have now finished the search which your Lordship desired us to make, in the hope of finding, among the papers of the late Mr. Henry Cavendish, something which he had prepared & thought fit for printing. Our search has in this respect been fruitless; a result for which we are sorry, though we must confess that it was not unexpected to us; because we knew that Mr.Cavendish was always ready to publish whatever he had made out to his full satisfaction. There are some few small scraps, which are transcribed nearly fair, as if he had thought of communicating them to the R.S.: but as it is apparent that they have been laid by, in that state, for a considerable time, it is to be supposed that he afterwards discovered some weakness or imperfection in them, or that they had been anticipated in a manner of which he was not aware when he composed them; in short, that he had some good reason for not giving them to the public. In truth, Mr. Cavendish's fame stands so high already in the scientific world, that no papers but of the most perfect kind could be expected to increase it, whilst it might be lowered by anything of an inferior nature.[298]

Blagden and his colleagues firmly recommended against including any of the unpublished papers in the proposed edition of Cavendish's papers, but they expected that dates and circumstances of his discoveries might be found among them that would be useful for the introduction. Since the papers were in "great disorder," some qualified person with time to spare would have to be found to go through them. They could think of only one person, the clerk of the Royal Society, George Gilpin, but they decided that he was probably too ill. They supposed that Lord George might ask around. Three months after Cavendish's death, Blagden and Banks between themselves agreed to postpone plans for an edition of Cavendish's works.

Blagden, Banks, and others recognized the peril of trying to improve a reputation posthumously, but they were mistaken about the worth of Cavendish's manuscripts. That could hardly have been otherwise, since the papers contained much that was original, and much more than the work of a few hours or a few days was required to appreciate this. Blagden was right in thinking that Cavendish's reputation was then so high that no unfinished papers could increase it, but he was wrong about the future interest in them. Today Cavendish is nearly as well known for what he did not publish as for what he did. One eminent scientist after another has studied his manuscripts and has come away impressed at what he achieved with the instruments and concepts available to him. To them it has seemed as if Cavendish were not of his own century but of the next.

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[298] Charles Blagden to "My Lord" [George Augustus Henry Cavendish], n.d., draft, Blagden Collection, Royal Society, Misc. Matter – Unclassified.

Chapter 18
Cavendish

By pursuing a working life in science, Cavendish made his life an experiment of another kind. In the laboratory he adapted nature to respond to his questions, and outside the laboratory he did not accept the life course that was his birthright but adapted it to his natural interest. To practice natural science was, as he said, to settle for "tolerable certainty." To experiment with life's possibilities was to follow a path that has not been completely charted.

By his choices Cavendish made a life of natural philosophy. This is the central meaning of Blagden's observation that Cavendish always knew what was right for him. A life of natural philosophy was not a complete way of life, but in Cavendish's case it came close to that. It contained a social life which, if limited in variety, was all that he wanted. The intellectual challenge of the life was limitless, its interest was inexhaustible. Through duty of service in the public sphere of science, the life had an ethical dimension. As Cavendish understood it, at least when he was a student, it made a connection with the spiritual world. Built into it was a motivation to act, since to be a natural philosopher meant to work to improve natural philosophy. It is indicative of his life of natural philosophy that he turned his houses into places of science; he lived *inside* it. When this is recognized, his life takes on a different aspect; it is a fulfilled life for him, not a deficient life. Like the early Greek philosophies, natural philosophy offered the good life,[1] and evidence strongly suggests that it offered Cavendish the good life. In this section we will meet some different opinions on the subject.

In contemplating Cavendish, Wilson decided that a "more eventless life, according to the ordinary judgment of mankind, than that of Cavendish, could scarcely be conceived."[2] Readers who have reached this point know that Cavendish's life was not without events, only these events had almost entirely to do with his scientific interests; with that qualification, Wilson's point is well taken. In the absence of the kind of events that make up most lives, we have organized Cavendish's biography partly by subject rather than by strict chronology. Departing furthest from the narrative form of biography, this final chapter is devoted to an analysis of Cavendish's personality, and as such it applies to the entirety of Part II. It could go at the beginning, but then Cavendish's life would be unfamiliar to readers and the analysis would lack a subject; we think that it belongs where we place it, at the end.

Blagden spoke of the "temper & character of the philosophers of this country."[3] In the eighteenth century the English distinguished between character and temperament, as we do. We speak of "character" as one part of "personality," a word they occasionally used, the other part being temperament; character is shaped by life experiences, temperament largely

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[1] John M. Cooper (2012b, 16; 2012a, 2–6).

[2] George Wilson (1851, 165).

[3] Charles Blagden to Joseph Priestley, 11 June 1785, draft, Blagden Letterbook, Yale.

by inheritance.[4] After a meeting, Blagden wrote in his diary, "talk about Mr. Cavendish, & explanation of character."[5] Unfortunately Blagden did not say what the explanation was, as it would have been the best informed of any we have. In this chapter, we consider the question.

Early Interpretation

We can speak confidently of Cavendish the *man of science*, but can we speak of Cavendish the *complete man*? In a course on chemistry given in 1855, the lecturer gave an emphatic no to this question. He began by warning his students about Cavendish: "It may be fairly asked, why bring such a character forward for examination?… Is it enough *not* to be a villain, a debauchee, a murderer? Or, rather, is it not our duty to be *something* that shall create and influence for *positive good* on our fellow-men? To this the answer must be made, that the character of Cavendish is not introduced as a subject of admiration, or for imitation, but rather as a warning to all men who cultivate the intellect, that they do not neglect the social portion of their nature."[6] This lecturer regarded Cavendish as a "calculating machine," having read a book published four years before, George Wilson *The Life of the Hon*^{ble} *Henry Cavendish*. Francis Bickley, a historian of the Cavendish family, concluded from Wilson's *Life* that "there is something pathetic about such an existence as Henry Cavendish's, so fruitful and yet so utterly barren."[7] Thorpe, the general editor of Cavendish's papers, wrote to a fellow editor that Cavendish was "not a man as other men are, but simply the personification and embodiment of a cold, unimpassioned intellectuality."[8] Cavendish's recent biographer Berry, quoting Wilson, speaks of Cavendish's "striking deficiencies as a human being."[9] Jonathan Norton Leonard's *Crusaders of Chemistry*, a book I read when I was young, contains a chapter entitled "Henry Cavendish, the Measuring Machine," citing Wilson's biography; it concludes, "So lived and died the coldest, most unhuman mortal who ever wrote his name large in the history of science…. His sole interest was to measure the objects in the material universe."[10] W.R. Aykroyd's *Three Philosophers (Lavoisier, Priestley and Cavendish)*, another book I read at around the same time, describes Cavendish as "a great brain, and a very small man!" It continues: a psychiatrist would find it interesting to guess what made his "full human development impossible, allowing one small part of his being to hypertrophy, and the rest to waste away"; this "dirty, semi-insane old aristocrat,"

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[4] The English in the eighteenth century were likely to speak of "character" where we speak of "personality," but we note that an eighteenth-century meaning of "personality" was a distinctive individual character, which is close to our meaning. Character and temperament have long been distinguished by psychologists: character is what people become intentionally; temperament is their inborn emotional predisposition. For the purposes of psychobiological research, the distinction is put differently, though not incompatibly. Character and temperament each have distinct brain systems and independent psychological dimensions. Temperament is the "dynamic organization of the psychobiological systems that regulate automatic responses to emotional stimuli," and it is "moderately heritable and stable throughout life." Character, by contrast, is "moderately influenced by family environment and only weakly heritable," and it develops into adulthood. To temperament belong the "automatic associative responses to emotional stimuli that determine habits and moods"; to character belong "self-aware concepts that influence our voluntary intentions and attitudes." C. Robert Cloninger (1994, 266–267).

[5] 14 July 1795, Charles Blagden Diary, Royal Society 3:65(back).

[6] Introductory lecture to a course on chemistry at the National Medical College by Lewis H. Steiner (1855, 6).

[7] Francis Bickley (1911, 207).

[8] Edward Thorpe to Joseph Larmor, 7 Feb. 1920, Larmor Papers, Royal Society Library, 1972.

[9] A.J. Berry (1960, 22).

[10] Jonathan Norton Leonard (1930).

who “cut a pathetic and ridiculous figure” in society, of a sort “to be found in any lunatic asylum,” was a great scientist, a rare instance of so “marked a degree of maladjustment” combined with brilliance.[11] These are some of the ideas readers have come away with from Wilson’s biography.

Of the characterizations of Cavendish, Humphry Davy’s is the most succinct, “A great Man with extraordinary singularities.”[12] We consider first the “great Man,” and we begin by questioning it. Cavendish published no books and fewer than twenty papers, a good half of which were on minor topics, and he left much of his good work unfinished and unpublished. He founded no school, he inspired no acolytes to insist that the world pay attention.[13] His mathematical theory of electricity drew almost no notice. His experiments on factitious air drew some notice, though Lavoisier scarcely acknowledged them. His understanding of heat was so little known that an admiring colleague thought he held the opposite theory. His experiment on the density of the Earth was thought up and planned by someone else. We might conclude that Davy misspoke, but Davy knew his subject. Cavendish raises questions about what is meaningful in a life of science.

“Greatness” implies superior abilities or accomplishments, usually both. With respect to what is great, a person is seen to hold advantages over most others. Because the judgment has a subjective element, consensus usually is not expected or attained,though there is a measure of agreement on Cavendish’s advantages (and disadvantages). Wilson, who approached his subject as a “student of chemistry,” said that Cavendish made no significant contribution to the apparatus or instruments of chemistry, in which regard he could not begin to compare with Hales and Priestley. Wilson generalized the point: “Cavendish, in truth, was not remarkable for an inventive spirit,” finding “novelty” uncongenial owing to his “great caution and love of simplicity.” He regarded Cavendish as a “discoverer,” whose merit was to set for himself a “standard of accuracy” that few fellow chemists cared to acknowledge.[14] The historian of science Robert Schofield writes that “Cavendish’s analytical imagination was unequaled in Britain in the years between Newton and Maxwell, but he lacked that ingenuity which invents new problems. His researches, therefore, tended to be elaborations of the ideas of others, which he defined with a precision and developed to an extent beyond the conception of the originators.”[15] These two evaluations, separated by over a century, agree that Cavendish was not inventive; in the one case he was not inventive of instruments, in the other of new problems. They also agree that Cavendish’s merit lies in his accuracy and precision. They are correct as far as they go. Cavendish, like most other notable scientists, was not exceptional in all ways, as an inventor of instruments and apparatus, as a master of analysis, and as a proposer of new problems for research.

Let us consider some other merits of Cavendish’s. He had mathematical-theoretical and experimental skills of a high order, a rare combination. Davy said, “Of all the philosophers of the present age, Mr. Cavendish was the one who combined, in the highest degree, a depth and extent of mathematical knowledge with a delicacy and precision in the methods of experimental research.”[16] Other early biographers said much the same. A second merit

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[11] W.R. Aykroyd (1935; 1970, 75–76, 78).

[12] J.C. Fullmer (1967, 133).

[13] Blagden may be an exception, but he was paid by Cavendish.

[14] Wilson (1851, 196).

[15] Robert E. Schofield (1970, 254).

[16] Humphry Davy, quoted from a chemical lecture he gave in 1810. John Davy (1836, 221). Similar wording: Humphry Davy (1812, 37). Humphry Davy, quoted in George Godfrey Cunningham (1837, 69).

was his understanding of which problems to take up; no colleague of his addressed the state of natural philosophy with more surety. In chemistry, he introduced methods to distinguish between gases, which held the key to understanding chemical composition and to rethinking the fundamentals of the science. He saw that for electricity to take its place alongside mechanics and the law of gravitation, the law of electric force needed to be determined and experimental consequences drawn from it. He carried out experiments to establish the basic laws of heat, the foundation of an exact theory of the field. A third merit was his conscientious service of fifty years in the work of the Royal Society and the breadth of knowledge, skills, and experience he brought to it. The scientist who engages with his fellows scientists in organized activities can affect the course of science as significantly as an author of many publications. Standards of practice, rules of communication, venues of scientific exchange, material resources of research, and much else move on as surely as does the frontier of science as recorded in publications. A fourth merit was a standard of excellence. By this measure, Davy compared Cavendish, whom he called "great," with the greatest: "Since the death of Newton, if I may be permitted to give an opinion, England has sustained no scientific loss so great as that of Cavendish."[17] Upon learning of Cavendish's death, John Walker described him to a colleague as "a man of wonderful mind, more nearly approaching that of Newton than perhaps any individual in this country since his time."[18] Blagden writing to a colleague after Cavendish's death said that he was "by much the best philosopher in my opinion that we have, or have had, in my time, at the R[oyal] S[ociety]."[19] Making allowance for the tendency to exaggerate the virtues of the recently deceased, these appraisals give us some notion of how Cavendish was seen by his contemporaries. The idea of the great man in history has long been out of favor, regarded as a relic of the nineteenth century. Today the truth of the past is sought in a complex of social and material forces, from which a skepticism about learning anything useful to history from biography follows. But by "great Man," Davy clearly meant that Cavendish was a very good scientist, not that he heroically transformed science.

Cavendish's early biographers were scientific men, who were naturally more interested in Davy's "great Man" than in his "extraordinary singularities," which they touched on by retelling anecdotes if at all, generally relegating the subject to what Cuvier called the "trivia of life."[20] His later biographer Wilson, however, saw a connection between the great man and certain singularities, which was not trivial or accidental but evidence of a strong will. Cavendish's attachment to inflexible routines, Wilson said, arose from his desire to replicate in his small world the invariable rhythms of the great world such as the rotations of the planets about the Sun.[21] We agree that Cavendish had a strong will, but it unlikely took the form Wilson suggested. An alternative, if partial, explanation of his regularities is an inborn proclivity, leading to behavior analogous to periodicities observed in the physical world. The strength of his will is to be seen in his life course.

Two passages of Wilson's have been frequently quoted, one having to do with Cavendish's range of emotions, the other with the way his mind worked. Cavendish's character "can be described only by a series of negations. He did not love; he did not hate; he did

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[17] Humphry Davy, quoted in John Davy (1836, 222).

[18] John Walker to James Edward Smith, 16 Mar. 1810 in Lady Smith (1832, 170–171).

[19] Charles Blagden to B. Delessert, 20 Mar. 1810, draft, Blagden Letters, Royal Society, D.44g.

[20] Georges Cuvier (1961, 236–238).

[21] Wilson (1851, 187).

not hope; he did not fear; he did not worship as others do. He separated himself from his fellow men, and apparently from God. There was nothing earnest, enthusiastic, heroic, or chivalrous in his nature, and as little was there anything mean, groveling, or ignoble. He was almost passionless."[22] For all of its positive qualities, its fairness, truthfulness, and insightfulness, Wilson's biography is a vivid portrait of Victorian negations, of a man deficient in piety, poetry, friends and family bonds, of a man estranged from humanity, who cared only for science. We recognize a foreshadowing of the portrait in the judgment on men who abuse science by Cavendish's evangelical neighbor John Venn, quoted earlier. We might agree that Cavendish cared only for science, but this did not preclude his humanity (or spirituality). It seems to us that quite the opposite was the case. Science included Cavendish in the world, for it was through science that he formed all of his meaningful connections with his fellow humans. Science is foremost a social endeavor.

Denied the everyday human qualities, Wilson's Cavendish is allowed only those traits required for his scientific work: intelligence, good eyes, and skillful hands. His horizon was correspondingly constricted. "His Theory of the Universe seems to have been, that it consisted *solely* of a multitude of objects which could be weighed, numbered, and measured; and the vocation to which he considered himself called was, to weigh, number, and measure as many of those objects as his allotted three score years and ten would permit."[23] From the testimonies, Wilson decided that Cavendish's brain "seems to have been but a calculating engine."[24] This characterization of Cavendish's view of the world and of the brain that conceived it is insightful but incomplete. In the laboratory, Cavendish worked with measurements, numbers, and calculations, but he also took account of much that lies outside mechanical calculation: the selection of the phenomena to study, the handling of instruments, the registering of sense impressions and their interpretations. Most of Cavendish's researches were both quantitative and qualitative, and some of them, for example his geological observations, were almost entirely qualitative. Wilson's description of Cavendish's brain as a calculating engine is furthest off the mark in his theoretical work. Although it was mathematical, it had little to do with calculation and much to do with understanding. Cavendish sought concepts that describe the physical world and he invented experiments that correspond to them. For him, numbering, weighing, and measuring were not an end in themselves but an aid to "strict reasoning," the way to scientific truths. The expression "calculating engine" suggests the mind of a savant, not Cavendish's.

Wilson's likening of Cavendish's mind to a calculating engine was timely. His biography was published in 1851, the year the first commercially successful mechanical calculating machine was manufactured, Thomas de Colmar's "Arithmometer." This was an adding machine with a moving carriage, which allowed for multiplication and division. Designed to meet the manufacturing capabilities of the time, it was durable and reliable, and it launched a new industry, calculating engines. Businesses, banks, insurance companies, government offices, and other operations that used a flow of calculations began to depend on it. Its use spread around the world, and for forty years it was the only mechanical calculator for sale. It had many non-commercial predecessors, however. The mathematician Charles Babbage's

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[22] Ibid., 185.

[23] Ibid., 186. Quantity being the distinguishing mark of Cavendish's work in Wilson's view, he may have looked to the bible for a passage to give it proper emphasis, though he could have found it elsewhere: "Thou hast ordered all things in measure, and number, and weight." (Wisdom 11:21).

[24] Wilson (1851, 185).

calculating machines, the "difference engine" begun in 1821 and the "analytical engine" begun in 1834, have been called "one of the startling intellectual achievements" of Wilson's century. To encourage Babbage, the Astronomical Society of London awarded him a gold medal on the expectation that when his difference engine was built, it could be used to calculate astronomical and navigational tables.[25] Such a grand calculating machine would have been helpful to Cavendish in some of his work,[26] but the scientific uses he put his mind to could not have been taken over by it, no matter how ingenious. This is the weakness of Wilson's metaphor, though given that calculating engines created a stir in his time, it is not surprising that he borrowed one to describe his unusual biographical subject.

Normality and Eccentricity

In this section and the next we consider two perspectives on Cavendish the "great Man with extraordinary singularities," those of eccentricity and autism. Originally a technical term in geometry and astronomy, "eccentricity" acquired its figurative meaning in the late seventeenth century. In the late eighteenth century, the *London Times* called it "a departure from the general conduct of society," the meaning we give it today. The word "eccentric" came to stand for an individual with eccentricities only in the early nineteenth century.[27]

There has been little scientific interest in eccentricity. To make a start, the psychologist David Weeks and his colleagues undertook a psychological study of about 1000 self-professed British eccentrics. They included in their study about 150 historical figures who were thought of as eccentric in their time. Cavendish, who is one of them, they characterized as shy and introverted "to a highly eccentric degree," whose "selective avoidance of people probably amounted to a social phobia."[28]

They single out five eccentric traits as most important, four of which apply to Cavendish: nonconformity, creativity, strongly motivating curiosity, and obsession with one or more hobbyhorses.[29] The fifth trait is idealism, or the ambition to change the world. Cavendish no doubt favored improvements, but he showed no dissatisfaction with the society in which he was fortunately placed.

In the eighteenth century certain traits of character were seen as distinctively English for which there was a word, which entered dictionaries near the end of Cavendish's life, "Englishness." Earlier the expression "English national character" was used, meaning the same. "National character" has fallen out of favor for its suggestion of ethnic and racial

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[25] Anon., "Mechanical Calculator" (http://en.wikipedia.org/wiki/mechanical_calculator). Anon., "Arithmometer" (http://en.wikipedia.org/wiki/arithmometer). Computer History Museum, "The Babbage Engine" (http://www.computerhistory.org/babbage/engines). Simon Schaffer (1994, 203).

[26] The auction catalog of Cavendish's instruments lists two calculating machines, but no description is given, item 69. *Catalogue of Sundry Very Curious and Valuable Mathematical, Philosophical, and Optical Instruments.*

[27] Sophie Aymes-Stokes and Laurent Mellet (2012). Victoria Caroll (2008, 12–13). Anon, "Eccentricity (Behavior)" (http://en.wikipedia.org/wiki/Eccentricity_(behavior)).

[28] David Weeks and Jamie James (1995, 10–12, 42, 49–50, 107–108).

[29] Ibid., 27–28, 32–33, 181–182. They regard eccentricity as a continuum of behaviors, which vary over time, place, and social level. Their empirical findings tell us about categories of eccentricity and about the personality traits that accompany them, but their method of selection of eccentric persons fails to identify some kinds of eccentrics. If Cavendish had been alive at the time of their studies, he would not have been included, for he would not have volunteered as a self-defined eccentric to undergo an interview with the researchers. An atypical eccentric in their sample, Cavendish was an introvert who held normal ideas, whereas most of their eccentrics were extroverts who held eccentric ideas.

personality traits, but in the eighteenth century it was thought to stand for a valid concept of social analysis.[30] Observations of English national character at the time were often perceptive, but their generalization to all English was fanciful; although institutions and manners in England were distinctive, they did not come about through a particular collection of national personality traits. Given this admission, the concept of English national character still has a limited use for us as a contemporary benchmark for assessing Cavendish's behavior. By informing us what was thought of as native behavior in Cavendish's day,[31] English national character helps us recognize what was seen as eccentric about him. In this section, "national character" means behaviors that English and foreign observers often regarded as distinctively, though not uniquely, English. We are dealing with subjective perceptions.

The English had a problem with national character; for it implied uniformity, the opposite of individuality, a valued trait. Priestley said that the English were thought to have the "least of an uniform national character, on account of their liberty and independence, which enables every man to follow his own humour."[32] The answer to the problem was found in the notion of "eccentricity," which implied a norm of behavior, related to the national character. Eccentric departures from the norm were understood to arise from an unrealistic view of the world; they were benign, often found engaging, occasionally troubling but definitely not disruptive of the social order. They were an excess of a good thing, individuality.

In a historical study of English national character, Paul Langford identifies six "supposed traits of Englishness“: eccentricity, decency, candor, taciturnity, reserve, and energy. We make use of his list here, beginning with the first trait, eccentricity, which was seen to fit Cavendish. His colleague Thomas Young said that his "severe scientific study" alone spared him from "absolute eccentricity."[33] We take Young to mean that outside of science Cavendish was eccentric. Others at the time might have considered Young overly cautious in excluding Cavendish's severe scientific study, since a person who was obsessional was often considered eccentric.[34] In his biography of Cavendish, Wilson did not use the word "eccentric," but he used words that mean the same, "difficult character," "singular oddities of character," and "peculiarities of his character."[35]

Decency, a second presumed national trait, we recognize in Cavendish's management of his farms; he restrained his steward from taking actions that could hurt delinquent tenants. Cavendish was known for his candor, or love of truth, a third presumed national trait. He had the "most amiable candor" and the "strictest integrity," Blagden said.[36] Traits related to candor are honesty, sincerity, directness, openness, and simplicity, all of which apply to Cavendish. Simplicity is seen in Cavendish's writing, a perfect fit with the original statutes of the Royal Society: "in all reports […] the matter of fact shall be barely stated, without any prefaces, apologies, or rhetorical flourishes."[37] Davy said that Cavendish wrote with the "greatest dignity and simplicity and in the fewest possible words, without parade or

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[30] Paul Langford (2000, 1–2, 7–8, 26).

[31] Peter Mandler (2006, 2, 53, 57).

[32] Before Priestley, the philosopher David Hume used almost the same words: because of the "great liberty and independency which every man enjoys," the English "of any people in the universe, have the least of a national character." Langford (2000, 22, 291–292, 300–303).

[33] Thomas Young, (1816–1824, 444)

[34] Langford (2000, 303).

[35] Wilson (1851, 167, 170).

[36] Charles Blagden to William Cullen, 17 June 1784, draft, Blagden Letterbook, Yale.

[37] Quoted by Edward Thorpe (1921, 6)

apology," stating the "simple truth."[38] Likewise Young said that Cavendish's publications were "expressed in language which affords a model of concise simplicity."[39] The style of Cavendish's handwriting was in keeping: clear, without flourishes. The library stamp in his books was simple, his name only, with no embellishments. His preference for simplicity carried over to his scientific work; his apparatus was simple, making use of plain fir, not hardwood.[40] Simplicity was a widely held value in a time when nature was coming to be opposed to artiface as the standard of behavior. Newton, the authority on the subject, wrote that "nature will be very conformable to herself and very simple," that "truth is ever to be found in the simplicity, and not in the multiplicity and confusion of things."[41] Cavendish's search for truths of nature was at the same time a commitment to simplicity. He wrote in a planned treatise on mechanics that Newton's second law of motion is "the most simple & therefore the most likely to be true of any law one can invent."[42] Blagden said Cavendish had "a truly philosophical simplicity of manners."[43] Simplicity marked his every action in the world.

Openness was valued by the English,[44] who suspected that anything that could not be said openly concealed something discreditable.[45] In the management of his farms, Cavendish told his steward that the condition of his employment was complete openness. In his scientific activity he encouraged openness. When Michell asked him to keep "secret" the principle of an astronomical method until his paper was read before the Royal Society six months from then, Cavendish said he was "sorry" he wanted him to do that, for "the surest way of securing merit to the author is to let it be known as soon as possible & those who act otherwise commonly find themselves forestalled by others."[46] Michell agreed with Cavendish, giving him permission to show his paper to any interested persons. Cavendish asked the government not to keep "secret" Hatchett's experiments on gold alloys for coinage carried out under his direction, and the government complied.[47] When the author of a pamphlet on the Royal Society's dissensions wanted to remain anonymous, Cavendish advised otherwise on the grounds that the only way for it to have an effect was for the author to supply his name, and the author agreed to put his name on the pamphlet.[48] In response to Marum's complaint that Cavendish had not provided him with the information he requested about an experiment, Cavendish published the letter he had sent to Marum three years earlier to enable readers to judge the fairness of the criticism, for he "should be sorry to be thought to have refused any necessary information."[49] Clarity of communication is related to openness, and Cavendish prescribed methods of using scientific instruments to enable researchers to understand one another without question. Openness was a guiding principle of the Royal Society, as it was of Cavendish's.

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[38] Humphry Davy, quoted in Thorpe, ibid., 5–6.

[39] Young (1816–1824, 436)

[40] Wilson (1851, 178).

[41] Isaac Newton (1952, 372). Newton quoted in Frank Edward Manuel (1974, 120).

[42] Henry Cavendish, "Plan of a Treatise on Mechanicks," Cavendish Mss., VI(b), 45:17.

[43] Charles Blagden to William Cullen, 7 June 1784, draft, Blagden Letterbook, Yale.

[44] Langford (2000, 90–92).

[45] Ibid., 96, 99.

[46] Henry Cavendish to John Michell, 27 May 1783, draft; in Jungnickel and McCormmach (1999, 567).

[47] Henry Cavendish to Charles Hatchett, 15 Oct. 1802; this letter was enclosed in a letter by Charles Hatchett to Joseph Banks, 24 Oct. 1802, BL Add Mss 38424, f. 160.

[48] Charles Blagden to Joseph Banks, 24 and 26 Oct. 1784, BM(NH), DTC 3:83–86.

[49] Henry Cavendish to Martin van Marum, published in Cavendish (1788b, 231–232).

Foreigners who were sensitive to English inconsistencies "made an exception for taciturnity, one constant characteristic of an Englishman," a presumed fourth national trait. To a foreigner, English clubs seemed quiet, their members respecting one another's silences.[50] When dining at one of his dining clubs, Cavendish suddenly broke the silence. "I am told that you see the stars round, Dr. Herschel." "Round as a button," Herschel replied. Silence returned until nearly the end of dinner, when Cavendish asked in a doubtful voice, "Round as a button?" "Exactly, round as a button."[51] The exchange is recalled as an example of Cavendish's silent manner and his occasional departure from it, which it is, though because Herschel said no more than Cavendish, it could equally be taken also as an example of English dinner conversation. According to Brougham, Cavendish "uttered fewer words in the course of his life than any man who ever lived to fourscore years, not at all excepting the monks of La Trappe." Less colorfully, and more accurately, Playfair said that Cavendish "speaks with great difficulty and hesitation, and very seldom." To a colleague, Banks referred to Cavendish "who you know is little given to talking."[52] But when he was familiar with a person, on occasion his "conversation was lively, and full of varied information."[53] He had the manners of a silent English gentleman, who in a reassuring setting could become almost loquacious.

English gentlemen were known for their reserve, a presumed fifth national trait. Brougham, we recall, said that Cavendish had "a most reserved disposition,"[54] a behavior consistent with his taciturnity. He showed several other traits similar to reserve: preference for solitude and privacy, shyness, avoidance of women, apartness from servants, and coldness. Henry Holland, who knew Cavendish from the Royal Institution, spoke of his preference for the "umbratilis vita," an ancient expression: *umbratilis*, keeping out of sight, as it were in the shade (Virgil); *umbratilis vita*, retired, contemplative life (Cicero).[55] Barrow said that Cavendish seemed "to consider himself as a solitary being in the world, and to feel himself unfit for society."[56] Davy said that Cavendish "lived latterly the life of a solitary." A Clapham neighbor said that Cavendish's "desire seemed to be alone and to be left alone."[57] "A singular love for solitariness, and the reluctance to mix with his fellows" was the "most striking" peculiarity of Cavendish, Wilson concluded from the totality of reports of his behavior.[58] An Englishman placed high value on privacy; jealous of his freedom and independence, he "could not tolerate ease of access to his home." It was in the worst of taste for an acquaintance to arrive at his house at dinnertime and expect to be fed,[59] or for a banker to call on him unannounced and expect to do business. Cavendish's banker made this mistake. He identified himself to Cavendish's servant, who passed the information to his master: "Mr. Cavendish, in great agitation, desires he may be sent up, and before he entered the room, cries, 'What do you come here for? What do you want with me?'" The banker proposed an action, and in ill humor Cavendish agreed: "'Do so!

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[50] Langford (2000, 179).
[51] Constance Lubbock (1933, 102).
[52] Joseph Banks to William Hamilton, 30 Nov. 1794, BL, Edgerton 2641, 155–156.
[53] Davy (1836, 222).
[54] Henry Brougham (1845, 258).
[55] Henry Holland, in Archibald Geikie (1917, 225).
[56] Barrow (1849, 144).
[57] Dr. Sylvester quoted in Wilson (1851, 170).
[58] Ibid., 165.
[59] Langford (2000, 107, 119–120).

Do so, and don't come here to trouble me, or I will remove it [his account at the bank].'"[60] Cavendish's value on privacy was firmly anchored in English social customs.

Shyness is not the same as reserve, but their behaviors are close. In accounts of Cavendish from the time, the words "shy" and "shyness" appear regularly and, if less often, related words such as "diffident," "bashful," and "embarrassed." Brougham said that Cavendish had "peculiarly shy habits," which accounted for his "singularity of manner." He entered "diffidently into any conversation," and then only when it was on a scientific subject that interested him.[61] According to a member of his club, if someone tried to draw him into conversation, "he always fought shy." The best way to engage him was "never to look at him, but to talk as it were into vacancy."[62] Banks advised visitors "to avoid speaking to him [...] [but] if he speaks to you, continue the conversation."[63] Blagden wrote of his "shyness and diffidence natural to his disposition."[64] Barrow spoke of his "extreme shyness," as confirmed by "all his habits."[65]

Embarrassment and shyness are often confused, and they are close. Thomson said that Cavendish was "shy and bashful."[66] Any attention to Cavendish's person caused him acute embarrassment, as shown by the following incident. Introduced to a foreign visitor as a celebrated natural philosopher, he was subjected to a flattering speech. "Mr. Cavendish answered not a word, but stood with his eyes cast down quite abashed and confounded. At last, spying an opening in the crowd, he darted through it with all the speed of which he was master; nor did he stop till he reached his carriage, which drove him directly home."[67] In addition to drawing unwanted attention to his person, the encounter with the foreign visitor involved a stranger, who was another problem. Strangers made a mistake if they tried to become "familiar" with an Englishman, for this implied the right to intrude, an un-English liberty. According to a foreign observer, if strangers "should venture to address them [the English], they receive it with the air of an insult."[68] Strangers were advised that unless Cavendish spoke to them first, they should not speak to him "as he would be offended."[69] He had a "perfect horror" of a strange face, according to a former stranger: "My eye caught that of Cavendish, and he instantly became silent: he did not say a word."[70] Having obtained permission to use Cavendish's library Alexander von Humboldt was cautioned that if he should encounter the owner "he was on no account to presume so far as to speak, or even greet" him.[71] Foreign and out-of-town visitors and other strangers were invited to the Sunday conversational gatherings at Banks's house. Cavendish's arrival at Banks's house was described by a fellow of the Royal Society: "I have myself seen him stand a long time on the landing, evidently wanting courage to open the door and face the people assembled,

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[60] Wilson (1851, 175–176).

[61] Brougham (1845, 258).

[62] A chemist, quoted in Wilson (1851, 169).

[63] Pepys, quoted ibid., 168.

[64] [George Augustus Henry Cavendish and Charles Blagden], *Gentleman's Magazine* (March, 1810, 292). Family obituary of Henry Cavendish.

[65] Barrow (1849, 144).

[66] Thomas Thomson (1830–1831, 1:337).

[67] Ibid., 337–338.

[68] Langford (2000, 238, 249, 255).

[69] Pepys, quoted in Wilson (1851, 168).

[70] Children, quoted, ibid., 169.

[71] From K. Bruhn's *Life of Alexander von Humboldt*, quoted in James Thorne (1876, 1:111).

nor would he open the door until he heard someone coming up the stairs, and then he was forced to go in."[72]

English males were brought up to behave "with extreme caution where women were concerned," and some never learned how to relate to them. With exceptions, Cavendish avoided women, whom we might think of as a variety of strangers, but this would overlook the intensity of his aversion, as Wilson described it. In his neighborhood, Cavendish was regarded as a woman hater,[73] and at the Royal Society Club he gave the impression that he despised men who liked female company.[74] A supposed instance of this occurred at a dinner of the Club, where members noticed a pretty girl watching them from a window across the street, and they gathered around their window to admire her. Thinking they were looking at the moon, Cavendish joined them at the window, but when he saw what they were about, he turned away in "intense disgust."[75] Misogyny, if that describes Cavendish, was an extension of familiar English male behaviors.[76]

Relations between masters and servants in English homes were characterized by an absence of human warmth.[77] When Cavendish encountered one of his maids with cleaning tools on the stairs, he immediately had a back stairs built.[78] This has been taken as evidence of his misogyny, which it may be, but it can be seen another way too. The addition of back stairs in British houses was common, the object being to remove servants as far as possible from their masters except when they were called to present themselves. It has been called a "revolutionary invention," but by the time Cavendish built his back stairs, it was no longer revolutionary, having been around for a century.[79] To avoid encountering his female servants, Cavendish followed another plan, leaving a note at a certain hour on the hall table with instructions for his dinner.[80] If a female servant "ever showed herself she was immediately dismissed."[81] This behavior might fall under the heading of misogyny, which is where Wilson places it, but if it does, it also belongs under servant and master behavior, falling under Englishness.

Relations between masters and servants were a more rigorous instance of a general characteristic, coldness. "England is not the country of emotions," a foreign visitor put it.[82] One evening after Cavendish had left the company at the Monday Club, Blagden and Aubert talked about him, agreeing that he had "no affections, but always meant well."[83] Blagden and Aubert considered themselves Cavendish's friends, and he evidently gave no sign of affection in return. Cavendish sought out colleagues, but if their conversation strayed from science, he "turned aside, and all the cold indifference of his nature returned."[84] One

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[72] Wilson (1851, 169).

[73] Mrs. Herbert, quoted in Wilson (1851, 178).

[74] Barrow (1849, 145).

[75] A fellow of the Royal Society, quoted in Wilson (1851, 170). John Timbs regarded this anecdote as apocryphal, though he used it all the same. It may be apocryphal, but we have no way of knowing, and it is consistent with less colorful reports of Cavendish's aversion to women. Timbs (1866, 1:143).

[76] Langford (2000, 304).

[77] Ibid., 241–244.

[78] Wilson (1851, 170).

[79] Patricia Meyer Spacks (2003, 6).

[80] Brougham (1845, 258–259).

[81] Wilson (1851, 169).

[82] Langford (2000, 250).

[83] 15 Sep. 1794, Charles Blagden Diary, Royal Society 3:16(back).

[84] A fellow of the Royal Society, quoted in Wilson (1851, 182).

of Wilson's informants called Cavendish the "coldest and most indifferent of mortals."[85] By the end of Cavendish's life, the English national character was identified with stolidity, impassivity, and self-control, a source of pride for the English, evidence of rationality and disproof of superficiality.[86] Cavendish had an abundance of this English virtue.

The final presumed trait of Englishness is energy. Persons of high achievement commonly display more energy than others, but on this point we are unsure about Cavendish. He did a great deal of original work in science, much more than his publications would suggest, as we know from his manuscripts, but he also had a great deal of time in which to do it. What Blagden wrote to a colleague in 1790, "Mr.Cavendish does not seem to be very busy,"[87] we suspect could have been said of him at other times as well. In response to a correspondence begun by Priestley, Cavendish said that he would send an account of his experiments in the future, "but I am so far from possessing any of your activity that I am afraid I shall not make any very soon."[88] Compared to the tireless Priestley, any person might feel slow, but for Cavendish this description was self-characterizing. For six months Priestley's second letter went unanswered; Cavendish apologized, "as I make not a tenth part of the exper that you do & as my facility in writing falls short of yours in a still greater proportion I am afraid will think me a bad correspondent & that the advantage lies intirely on my side."[89] During the dissensions of the Royal Society, Cavendish said that his only objection to assuming leadership was "his unfitness for active exertion."[90] We can say with reasonable confidence that Cavendish was not supercharged.

Of the six traits of Englishness, in the liberal interpretation given to them here, two of them, taciturnity and shyness, contain nearly all of Cavendish's markedly eccentric behaviors. They relate to his silences, solitariness, wariness of strangers, aversion to women, and emotional coldness. As we have seen, his eccentricities were extensions of behaviors thought to be characteristically English; they were not original departures from them but confirmations of them. Other eccentric behaviors of his are not particularly English nor are they very eccentric; for example, the regularity of his daily activities and his old-fashioned dress.

Eccentric behavior can seem comical or absurd, as it should, since the judgment is made by normal people, whose normal behavior makes sense to them. Lest we leave Cavendish at the mercy of his eccentricities, we should be aware that there is another way of looking at them, which is thought to be quintessentially English. In the early nineteenth century, a genre of popular writing was invented, the eccentric biography, consisting of collections of brief biographies of persons famed for their eccentricities. An early English author of an eccentric biography John Timbs wrote in *English Eccentrics and Eccentricities*, "how often do we find eccentricity in the mind of persons of good understanding." However "outlandish, odd, queer" the eccentric appears, he "may possess claims to our notice which the man who is ever studying the fitness of things would not so readily present."[91] Later in the century, the English philosopher John Stuart Mill welcomed eccentricity as an antidote to oppressive popular opinion, which he expressed as a mathematical observation: "the amount

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[85] Quote from one of Willson's informants, ibid., 173.

[86] Langford (2000, 250).

[87] Charles Blagden to Richard Kirwan, 20 Mar. 1790, draft, Blagden Letters, Royal Society 7:322.

[88] Henry Cavendish to Joseph Priestley, n.d. [after May 1784], draft; in Jungnickel and McCormmach (1999, 594).

[89] Henry Cavendish to Joseph Priestley, 20 Dec. 1784, draft; ibid., 598–599, on 599.

[90] Charles Blagden to Joseph Banks, 5 Apr. 1784, BM(NH), DTC 3:20–21.

[91] Timbs (1866, 1:iii–iv).

of eccentricity in a society has generally been proportional to the amount of genius, mental vigour, and moral courage it contained."[92] In the next century, Edith Sitwell, author of *English Eccentrics*, and herself an eccentric, wrote an appreciation of eccentricity: "the man of genius and the aristocrat are frequently regarded as eccentrics because genius and aristocrat are entirely unafraid of and uninfluenced by the opinions and vagaries of the crowd."[93] In his history of aristocracy, the English baron Lord Montagu of Beaulieu writes that an aristocrat did not need to make a display of his wealth or observe flawless etiquette or restrict his social life to his own stratum: "individuality and eccentricity, the product of security, were class characteristics of the British aristocracy."[94] The psychologists of eccentricity Weeks and Kate Ward defend eccentrics: "in an era when human beings seem typecast by their culture or genes, eccentrics are a refreshing reminder of everyone's intrinsic uniqueness. By heedlessly flouting norms of behavior that most of us never question, they remind us how much of our liberty we forfeit without thought, and how great our ability is, in fact, to forge our own identities and shape our own lives."[95] With the positive case for eccentricity in mind, we look at Cavendish again. Mills recognized eccentricity as "strength of character," and Wilson recognized Cavendish's "peculiarities" as "tokens of a strongly developed will." If we bring their thoughts together with Langford's on eccentricity as a trait of Englishness and Sitwell's and Montagu's on genius and aristocracy, we have our subject, Cavendish the willful investigator of nature and an eccentric example of the complete Englishman.

From that positive perspective, which admittedly ignores much else that can be said about Cavendish's eccentricity, we see his shyness not so much as a handicap as a useful protection of his privacy, freeing him for what he knew was best for him, scientific work. Likewise we think of his shyness as the social expression of a native circumspection, which in the laboratory took the objective form of the "error of the observer" and "corrections" for the totality of extraneous factors influencing the experiment. We think of his solitariness and taciturnity not as social withdrawal but as an indication of self-sufficiency and maturity.[96] When Cavendish *did* speak, Playfair said, it was always "exceedingly to the purpose, and either brings some excellent information, or draws some important conclusion."[97] Davy said that when Cavendish *did* speak, his "conversation was lively, and full of varied information," and that "upon all subjects of science he was luminous and profound; and in discussion wonderfully acute."[98]

Autism

In 2001 the eminent neuropsychologist Oliver Sacks diagnosed Henry Cavendish with Asperger's syndrome, a less severe form of autism, in a communication to the scientific journal *Neurology*. He said that he is wary of recent claims of Asperger's syndrome for historical figures, but he considers Cavendish an exceptional case, finding the evidence for his autism

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[92] John Stuart Mill (1859). Quoted in Carroll (2008, 11).

[93] Edith Sitwell (1965, 145).

[94] Edward Douglas-Scott, Lord Montagu of Beaulieu (1970, 142–143).

[95] David Weeks and Kate Ward (1995); quoted in Clifford A. Pickover, (1998, 279).

[96] Philip G. Zimbardo (1977, 2, 16, 20). Anthony Storr (1988, 29). Susan Sontag (1969, 19–20, 26).

[97] John Playfair quoted in Wilson (1851, 166).

[98] Humphry Davy, quoted in John Davy (1836, 2:222).

"almost overwhelming."[99] Upon rereading Wilson's biography of Cavendish, he wrote in his memoir *Uncle Tungsten* the same year that Cavendish was a "unique autistic genius."[100]

In our biography of Cavendish in 1996, we said that because of his strange behaviors he invites a psychological approach, but that it was not the approach we took, as we explained. At the end of the biography, we briefly mentioned possible psychological descriptions of his behavior such as social anxiety, shyness, and embarrassment, and we pointed out that he showed "autistic-like traits," which we listed.[101] As a source, we cited an earlier publication by Sacks, containing a moving account of the autistic scientist Temple Grandin.[102]

We published an improved version of our biography three years later, and we again briefly brought up psychological descriptions, though this time we left out any mention of autism, since we wanted the biography to be solid. Autism is a disorder that begins in childhood, and almost nothing is known about Cavendish's childhood, and also certain criteria for autism seemed to us a questionable fit. Since then we find in recent writings on the subject a growing acceptance of a more inclusive understanding of autism together with a trend in clinical thinking that favors an autistic continuum approach. In this section, we consider Sacks's diagnosis of Cavendish's autism, which was written up in *The New York Times*, "A Disorder Far beyond Eccentricity."[103]

Definitions and diagnostic criteria of autism are given in the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM*), published by the American Psychiatric Association, and in the *International Classification of Diseases* (*ICD*), published by the World Health Organization. A fifth edition of the *DSM* was published in 2012, with changes in the classification and diagnosis of autism, but since most of the recent literature on autism refers to the fourth edition, which is not contradicted by the new edition, we use the earlier edition here. "Classic" autism is a disorder with three areas of difficulties. The first is social interaction, which includes unresponsiveness to others, lack of friends, disinterest in sharing, and atypical eye contact, facial expressions, and responses to the emotions of other people. The second is verbal communication, which includes difficulty with language and conversation and atypical intonation, pitch, and emphasis in speech. The third is repetitive behaviors, which include preoccupation with narrow interests, insistence on fixed routines, and mannerisms such as hand flapping. Other difficulties commonly found in autistic persons include intellectual disability, heightened or diminished sensitivity to sensory stimuli such as sight and sound, and perceptual problems in making sense of sensory stimuli.[104] Because of the range of autistic behaviors, it is meaningful to speak of an "autism spectrum disorder." At one extreme of the spectrum are persons who are unable to speak and are otherwise severely handicapped. The autism we are interested in is at the other extreme, the

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[99] Oliver Sacks (2001a, 1347).

[100] Oliver Sacks (2001b, 121).

[101] Hugo Lidbetter writes that Jungnickel and McCormmach "got very close to suggesting" that Cavendish may have had Asperger's syndrome by emphasizing his shyness. We got closer than that, we said it: "We observe in Cavendish a number of autistic-like traits: single-mindedness, apparent inability to feel certain emotions, secludedness, rigidities of behavior, odd gait, harsh voice, strange vocalizations, panic attacks, self-acknowledged social unfitness." Jungnickel and McCormmach (1996, 368). The author's purpose is to make a "systematic exploration" of Sacks's claim that Cavendish had Asperger's syndrome. His article consists of matching Cavendish's behaviors with the Gillberg diagnostic criteria for Asperger's syndrome. He says Cavendish had this disorder. "Henry Cavendish and Asperger's syndrome: A New Understanding of the Scientist" (2009, 784).

[102] Oliver Sacks (1995). Temple Grandin (1995).

[103] Erica Goode (2001).

[104] Ilona Roth (2010, 3–4, 38–41).

normal- or high-IQ end, which in *DSM* and *ICD* (10th edition) enters as two separate and closely related categories: high-functioning autism and Asperger's syndrome.[105]

Ordinarily autism is diagnosed early, the average age falling between three and four; in the case of Asperger's syndrome, it is often later, six or older.[106] The one reference we have to Cavendish's early years comes from Blagden, who wrote in the family obituary that his "habits had, *from early life*, been secluded."[107] It tells us that autism is not ruled out. The case *for* his autism depends on his adult behaviors, which are all we know. If we conclude that Cavendish was autistic, we think that if we knew about his childhood, we would find autistic traits there, as Blagden suggested.

To make the case, it is not enough to check off the symptoms in the diagnostic criteria as laid out in the *DSM* or *ICD*. Agreement between the symptoms and the criteria is important in making a reliable diagnosis, but so are the severity of the symptoms and their effect on the disorder. The determination normally requires clinical training. In this section, we show that a good many of Cavendish's personality traits are similar to ones commonly found in persons who are diagnosed with autism. The match is suggestive and perhaps significant, but on this basis alone we cannot conclude that Cavendish was autistic.

Autistic people "tend to be unconcerned about fashion or whether what they wear is contemporary."[108] They can differ from others in their way of moving, owing to poor balance and coordination.[109] Walking with scarcely any arm motion is an autistic trait.[110] Clumsiness is another, according to Gillberg's criteria, an alternative to the *DSM*'s criteria, often preferred by clinical workers.[111] Cavendish's dress was always the same; he walked with one hand behind his back; he "bustled up to us in his odd way."[112]

Withdrawal upon eye contact, involuntary vocalizations and repetitive patterns of speech are common autistic behaviors. *DSM* criteria for Asperger's syndrome refer to "abnormalities in inflection," "talking too much" or "too little." Speech can be "unusually high-pitched" and have unusual "stress and rhythm." Autistic persons speak in facts, and without wishing to, they are often tactless. They frequently fall silent for no clear reason.[113] Cavendish's speech was shrill and hesitant, and he repeated parts of speech. As we saw in the previous section, he was usually silent, but when he was seated near persons he liked, he frequently talked a "great deal."[114] Eye contact could bring an immediate end to

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[105] "High-functioning autism" refers to autism with a normal or above-average IQ and with language delay; Asperger's syndrome is without the delay. The distinction between high-functioning autism and Asperger's syndrome may depend on the circumstances of the individual, and in practice the terms are interchangeable. There are the other sub-types of autism. "Pervasive developmental disorder—not otherwise specified" (P.D.D.N.O.S)—is the term used when autistic features are insufficiently pronounced for a definitive diagnosis of autism or Asperger's syndrome. "Atypical autism" is used when autistic features are only partly seen. The "autism spectrum" includes all these types. Simon Baron-Cohen (2008, 14, 21–26). Roth (2010, 42).

[106] Baron-Cohen (2008, 37). Temple Grandin (2011, 8).

[107] Blagen's contribution to the family obituary of Henry Cavendish. Italics added.

[108] Asperger Management (http://www.aspergermanagement.com/personal-appearance).

[109] Tony Attwood (2007, 259).

[110] Ledgin (2001, 46).

[111] Christopher Gillberg's diagnostic criteria are seen as closer to Hans Asperger's original descriptions. Attwood (2007, 53).

[112] Wilson (1851, 168, 170). In the sketch of him, his other hand is inside his coat. It is possible that the drawer invented the hand inside the coat, a common pose for formal portraits.

[113] Uta Frith (2011, 128–129). Attwood (2007, 37, 206, 224, 266–267). Grandin, quoted in Ledgin (2001, xiii).

[114] Wilson (1851, 167–168, 175). Barrow (1849, 144). Thomson (1830–1831, 2:337).

conversation, and when approached by a stranger, he might abruptly turn away, perhaps with a cry.

Autistic persons lack the emotional relatedness we call "affections." As a result, they learn social skills by conscious observation and study rather than acquiring them instinctively as other persons do. As we have seen, Cavendish's colleagues agreed that he showed "no affections, but always meant well." If he lacked affections, he learned compensating social skills, which translated as "always meant well."

A craving for solitude can be a sign of autism. Solitude is a powerful "emotional restorative," above all if the autistic person is occupied with an absorbing interest.[115] Cavendish showed "a singular love for solitariness."[116] Except for the servants' wing, Cavendish's houses were places of solitude. His laboratory was such a place, where he pursued his interest, the investigation of the physical world, and his study was another, where he read and wrote about the physical world; if he was autistic, he experienced no impairment in either place. On his deathbed he had no parting words for anyone: consistent to the end, he banished his servant so that he could experience his last moments in the "tranquility of perfect solitude."[117]

Autistic persons can acquire encyclopedic knowledge in their fields of special interest.[118] Their interest cannot be considered a mere hobby; on the contrary, it takes over their lives.[119] Nature was the most common interest of the children Hans Asperger studied, some of whom showed remarkable abilities and specialized knowledge in natural science, chemistry, and mathematics. The only subjects that interested Cavendish were scientific,[120] and his knowledge of them approached encyclopedic.[121]

Autistic persons have a strong desire for certainty and its companions, objectivity, perfection, accuracy, and truth. Early in life they are often drawn to mathematics with its logical truths, possibly developing a skill in it.[122] Cavendish brought a mathematical way of thinking to his interest in the physical world, with obvious success.[123] In her field, Temple Grandin has "a reputation for being totally objective," her emotions playing no part,[124] and the same can be said of Cavendish. Autistic persons frequently are "perfectionists with high self-imposed standards of achievement"; each of Cavendish's works was said to be "perfect at the moment of its production." Autistic persons are recommended for employment for their accuracy, which they prefer to speed in accomplishing any task. Accuracy was a hallmark of Cavendish's researches; Priestley referred to him as "that most accurate philosopher." The quest for truth comes naturally to autistic persons, as does persistence in detecting and avoiding errors. Obsessive in identifying and dealing with errors, Cavendish was said to be motivated by disinterested "love of truth and of knowledge."[125]

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[115] Attwood (2007, 55–56).

[116] Barrow (1849, 144). Wilson (1851, 165).

[117] Wilson (1851, 182–184). Young, "Cavendish," 445–446.

[118] Attwood (2007, 179–180).

[119] Ibid., 172.

[120] Wilson (1851, 182).

[121] Playfair (1822, 1:lxxxiv).

[122] Atwood (2007, 241).

[123] Humphry Davy, quoted in John Davy (1836, 221).

[124] Grandin, "Comments," in Norm Ledgin (2000, 202).

[125] Attwood (2007, 141, 238, 254, 295). Humphry Davy, quoted in John Davy (1836, 221). Joseph Priestley (1788, 327).

Autism was unknown to the medical world of the eighteenth century, but there is little doubt that there were autistic persons then, who could have included a gifted natural philosopher. Simon Baron-Cohen, an authority on autism, explains how this could come about: "People with autism, whose minds differ from what we consider typical, frequently display both disability and exceptional aptitude. Genes that contribute to autism may overlap with genes for the uniquely human ability to understand how the world works in extraordinary detail—to see beauty in patterns inherent in nature, technology, music and math." He suggests that genes associated with autism persist over generations because they are co-inherited with genes responsible for mathematical and technical talent, which society welcomes.[126]

Eccentricity, Autism, and Other Explanations

In the previous two sections, we looked at Cavendish's life from the perspectives of eccentricity and autism. In this section we look at Sacks's diagnosis of Asperger's syndrome, and we also consider alternative diagnoses: shyness, introversion, and several medical disorders. We begin with the way Cavendish was seen by his contemporaries. His expression showed a "nervous irritation";[127] his manner was "nervous."[128] Eighteenth-century meanings of "nervous" were: characterized by an agitation or disordered state of the nerves; suffering from a disorder of the nerves; excitable, easily agitated, timid.[129] Cavendish's speech was "excited"; he had "an air of timidity"; he had "a quickness and sensibility almost morbid,"[130] "morbid" meaning diseased; he was "shy and bashful to a degree bordering on disease."[131] The above words relate to what were called "nervous disorders." Hypochondria, hysteria, and dyspepsia are examples, minor illnesses attended by frequent calls on physicians. What they had in common was a presumed disturbance of the nervous system, the origin of the name. Cavendish was seen to have behavior in common with persons with nervous disorders, though his colleagues stopped short of labeling him with a disorder, speaking instead of "bordering on" and "almost." Young may have had this in mind when he attributed Cavendish's speech mannerisms to the "constitution of his mind" rather than to a "deficiency of his organic powers."[132] Cavendish was an eccentric person, not a person normally considered mentally ill or physically handicapped.

From Cavendish's time to the present, he has been regarded as eccentric. His prominent eccentricities, as we have seen, were exaggerations of generally admired traits of the English "national character": his inordinate shyness and penchant for solitude, his coldness, possibly his special interest to the near exclusion of all other interests, and possibly some of his regularities. Just how extreme these traits appeared at the time is open to question.

Let us consider possible explanations of his eccentricity. Obvious ones are shyness and introversion, which although they do not rise to the level of disorders can be mental handicaps, often severe. We saw that among strangers, Cavendish showed embarrassment, self-consciousness, and tension; he avoided eye contact, fell silent, and on occasion fled.

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[126] Simon Baron-Cohen (2012, 74–75).

[127] Brougham (1845, 259).

[128] Humphry Davy (1839–1840, 7:139); quoted in Wilson (1851, 167).

[129] "Nervous," *Oxford Universal Dictionary*, 3d ed., 1321.

[130] Pepys, quoted in Wilson (1851, 168). Young, "Cavendish," 444.

[131] Thomson (1830–1831, 2:337).

[132] Young (1816–1824, 444).

In the event that a stranger had interesting information, he showed a mix of avoidance and attraction typical of very shy people.[133] In other ways, Cavendish was atypical of shy people: he probably did not have low self-esteem and did not spend time thinking about his feelings and actions and how they appeared to other persons, mental states associated with shy behavior.

Introversion and reserve have different motivations than shyness. People who are introverted or reserved voluntarily limit their contact with others, since they gain no reward from it; people who are shy avoid contact because they fear it, not because they are unsociable.[134] Introverts, according to one study, are insistent on ethical standards, reliable, cautious, retiring, unemotional and have few close friends. According to another study, they are self-sufficient, serious, silent, skeptical, critical, precise, objective, rigid, and prone to sulk. They are rule-bound, limited in interests, hard workers, and retiring, especially with the opposite sex. They are drawn less to people than to impersonal objects such as mathematics, music, and science.[135] Like shyness, introversion is largely inborn. Introversion describes Cavendish, but it leaves out what shyness includes, unease and awkwardness, which he showed, and its motivation does not fit very well; he gained reward from contact with others if they had knowledge that interested him.

We pass from handicap to disorder. For a diagnosis of Asperger's syndrome, the *DSM* requires the presence of several social and behavioral impairments.[136] Under social, two or more of the following four criteria must be met:

- Impairment of nonverbal behaviors; for example, eye to eye contact, facial expression, posture, and gestures.
- Lack of relationships.
- Lack of spontaneity in seeking out and responding to persons with shared interests.
- Lack of social or emotional reciprocity.

Cavendish satisfied all four criteria. Under nonsocial behavioral criteria, at least one of the following must be met:

- Interests restricted in subject and abnormal in intensity.
- Adherence to nonfunctional routines.
- Repetitive physical mannerisms.
- Preoccupation with parts of objects.[137]

Cavendish satisfied the first of the four criteria. By *DSM* criteria, then, Cavendish showed Asperger behaviors. Gillberg's twenty criteria for Asperger's syndrome are divided into six categories. One of the six, "speech and language peculiarities," contains five parts, at least three of which must be met: delay in the development of speech, superficial perfection in expressive language, pedantic language, impaired comprehension of language, and "odd prosody, peculiar voice characteristics."[138] Because only the last one of the five is known

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[133] Caroll E. Izard and Marion C. (1986, 151, 153). W. Ray Crozier (1990, 48); Crozier, "Summary of Conclusions,"54.

[134] Anonym, ibid. "Extroversion and Introversion" (https://en.wikipedia.org/wiki/Extraversion_and_introversion). Anon. "Shyness" (http://en.wikipedia.org/wiki/Shyness).

[135] Anthony E. Kemp (1996, 36–39, 49). Lawrence A. Pervin (1993, 283).

[136] In the new revision, DSM-V, Asperger's syndrome is subsumed under "autism spectrum disorder" and the category "Asperger's syndrome" does not appear.

[137] Attwood (2007, 41).

[138] Ibid., 37.

to fit Cavendish, Asperger's syndrome would seem to be ruled out. Depending on which criteria we use, Cavendish was or was not autistic, but it is unclear how much we can read into these negative or positive matches; diagnostics apply to living subjects, and diagnoses are made by professionals.

Being a professional, Sacks's diagnosis has ready credibility. He bases his diagnosis of Cavendish's Asperger's syndrome on the following seven characteristics:

1. Striking literalness and directness of mind.
2. Extreme single-mindedness.
3. Passion for calculation and quantitative exactitude.
4. Unconventionality.
5. Stubbornly held ideas.
6. Rigorously exact, rather than figurative, language.
7. Virtual incomprehension of social behaviors and human relationships.[139]

Sacks's agreement with the *DSM* is not immediately obvious, since he uses different words than the manual, and he pays more attention to Cavendish's way of thinking than to his social behavior. The evidence for the working of Cavendish's mind comes mainly from his writings, which are on science, technology, and business, where we would not normally expect figurative language or non-literalness. For the same reason, characteristics 1 and 6 have a large overlap. It is not clear what "stubbornly held ideas" refers to. Cavendish was evidently the first British chemist to abandon the phlogiston theory, while Priestley and other colleagues still held to it. He probably did not invent new ideas, but he could change his mind about an idea if reason and experience called for it. "Virtual incomprehension of social behaviors" is too sweeping. He demonstrated a good understanding of human motivations during the dissensions of the Royal Society. Studies show that autistic people understand basic motivations quite well, their difficulty coming with more complex emotions and points of view.[140] Sacks does not say what he means by "unconventional." As we saw in the section above on eccentricity, Cavendish can be considered conventional, his eccentricities being conventional behaviors carried to excess. The main exception, which may be Sacks's meaning, is Cavendish's choice of a life of science rather than a career in politics; given his birth, this was unconventional. The *DSM* requires a match with at least two social criteria, and Sacks has only one, the seventh characteristic, but because of the generality of his wording, it could cover all of the *DSM* social criteria. With these comments in mind, Sacks's list matches the *DSM* criteria. Cavendish's work was quantitative; his language was literal and exact; he pursued science single-mindedly; and he had difficulty with social behaviors and human relationships. The same caution in the previous paragraph applies in this case: diagnostic criteria in the *DSM* being brief and general, they by themselves are an incomplete basis for a credible diagnosis. For that, experience is required,[141] and the person making the diagnosis here is Sacks, who supplies the experience.

Before we agree that Cavendish was autistic, we should consider the adequacy of the evidence. At least five arguments call into question the diagnosis. Because the arguments have their own weaknesses, we consider counter-arguments as well. First, because the testimony about Cavendish came from people who knew him late in his life, the central developmental

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[139] Sacks (2001a).

[140] Anonym, "Autism" (http://en.Wikipedia.org/wiki/Autism, 7).

[141] Attwood (2007, 40–41).

feature of autism goes unaddressed for lack of evidence. Second, Cavendish bore similarities to today's scientists, who often are obsessive, follow routines, exhibit social anxieties, and in general show autistic-like traits. They behave this way to do their work, about which they have strong feelings; they are rarely autistic. Third, Cavendish met frequently with many colleagues in the city, an intensity of social activity unusual for an autistic person. A counter-argument is that his interaction was highly selective, consistent with his private ways and narrow interests, his social world being a direct extension of his special interest, the physical world. Fourth, Cavendish's peculiarities did not seriously interfere with his chosen life and if anything supported it by sheltering him. By contrast, people with autism have a hard time managing their lives, requiring help with their work, daily affairs, and finances. A counter argument is that until Cavendish was past fifty, he lived at home where he could count on his father's help, and when he left home he took on an associate; and he always had servants. Fifth, Cavendish made major changes in his life, and autistic persons tend to dislike major changes, and if they make them, they are unlikely to have initiated them. In 1782 he took a house in a suburb, Hampstead, which served temporarily as a country house. In 1784, he bought a house on Bedford Square. In 1785, he bought a permanent country house on Clapham Common. In 1782 he took on an associate, Blagden, and in three summers, 1785–87, he and Blagden made long journeys. The counter-argument is that the changes may have been integral to his scientific plans, and he may have found the journeys sufficiently interesting to distract him from the break in his routines. As a general point, it is not uncommon for gifted autistic persons to do things that are atypical of autistic persons.

Two more arguments against Cavendish's autism have been raised by Fred Volkmar, a psychiatrist at the Yale Child Study Center. He thinks that autistic diagnoses of historical persons have got out of hand, becoming a cottage industry. "Certainly, Henry Cavendish sounds like a very strange person," Volkmar says, but even in Cavendish's case he remains skeptical. The reasons he gives are sensible, though not conclusive. One is that Cavendish was taciturn, whereas autistic persons talk endlessly about their special interest. An objection is that if persons approached Cavendish the right way, they could set him going. The second reason is that Cavendish was successful, whereas autistic persons usually do not accomplish much.[142] An objection is that autistic persons occasionally are very accomplished. Hans Asperger followed the adult lives of several of the children he studied: one who had shown spontaneous talent in mathematics became an outstanding astronomer, another received a Nobel Prize in literature.[143] On the question of Cavendish's autism, we have a difference of opinion among authorities, not uncommon in this field.

If Cavendish had a disorder, autism is not the only conceivable one. Sacks's diagnosis implies that no other disorder can account as well for Cavendish's behavior, and we need to consider what has been ruled out; the *DSM* provides us with alternatives. Of personality disorders, perhaps the most promising fit is "schizoid personality disorder," which is characterized by lack of interest in social relationships, desire for solitude, taciturnity, unresponsiveness to social cues, and emotional coldness; all of these behaviors apply to Cavendish. However, other characteristics of the disorder such as bizarre ideas, lack of motivation, and

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[142] Goode (2001).

[143] Roth (2010, 10).

underperformance at work effectively rule out Cavendish.[144] Another suggestive disorder is "social phobia," the fear of making a fool of oneself. Persons with social phobia have a pressing concern of "how to react to the gaze of others," and they are ever ready to take flight or to fend off attacks. They are shy, introverted, self-conscious, and prone to embarrassment; they are reticent when spoken to, and they rarely speak in groups; they do not like to be the center of attention, and they have a "terror of social interaction," which isolates them, often resulting in depression. Social phobia and its close relative "social anxiety disorder" describe Cavendish, but they say nothing about his obsessive interests and clocklike routines. They are also accompanied by low self-esteem,[145] which does not describe Cavendish; having a mastery of natural philosophy, and having important relatives, Cavendish was assured an ample measure of esteem from within and without. On first glance, obsessive-compulsive disorder also looks like a possible match. Cavendish exhibited obsessive behavior in following his special interest, but there is no evidence that he tried to resist the obsession or was disturbed by it—on the contrary, his life was scientific study, which by its nature is obsessive[146]—and resistance is required for a diagnosis of the disorder. Further, the disorder fails to account for Cavendish's unusual eye contact and facial expressions, common to autistic persons. Michael Fitzgerald has compiled an extensive table of disorders that share some but not all of the traits of autism, noting where they agree and disagree. Few of the disorders show one of Cavendish's most conspicuous traits, a preoccupation with a special interest, and those that do show it such as obsessive-compulsive disorder fail in other respects.[147] Of all potential disorders, autism fits Cavendish best. To this point, we have considered Cavendish's personality from different perspectives. What remains is the question of which, if any, we find most compelling.

How Do We Decide?

Granted that Cavendish had autistic-like traits, were these traits the result of a neurodevelopmental disorder? If we answer yes, we agree that the evidence supports a diagnosis of a disorder, that the disorder was in all likelihood autism, and that the posthumous diagnosis of Cavendish's autism is based on more than a superficial match of his behavior with autistic traits according to current texts on autism. We acknowledge at the same time that the dividing line between autism and normality is imprecise. If our answer to the above question is no, we have alternatives to fall back on. Either the evidence is insufficient to decide one way or the other, or the evidence is unfavorable. In either case, we can say that Cavendish showed eccentric behaviors, which were variations on a generally accepted mode of conduct, his genetic make-up and his choices accounting for the variations.

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[144] Michael Fitzgerald (2004, 37–39). The relationship between autism and schizoid personality is given in Sula Wolff (1995). Anon., "Schizoid Personality Disorder" (http://www.mayoclinic.org/diseases-conditions/schizoid-personality-disorder/home/ovc-20214901).

[145] John R. Marshall (1995, xviii, 23–24, 56, 110). Anon., "Social Phobia" (http://www.behavenet.com/social-phobia). Anon., "Social Anxiety Disorder" (http://www.Wikipedia.org/wiki/Social_anxiety_disorder). Anon., "Social Anxiety Disorder" (https://socialanxietyinstitute.org/). A third to one half of persons who suffer from this disorder experience depression, and also frequently anxiety, panic, and embarrassment. Jeralyn Ross (1993, 5–7).

[146] Lennard J. Davis (2008). Davis writes that science is itself an obsessive activity characterized by repetitive focusing on one subject. He develops this idea in many places in his book.

[147] Fitzgerald (2004, 36–41). Rab Houston and Uta Frith (2000, 147).

There are two strong arguments in favor of Cavendish's autism. One is Oliver Sacks's opinion that the evidence for such a diagnosis is "almost overwhelming." Given who he is, his opinion carries very considerable weight. His analysis of Cavendish's behaviors is the historical counterpart of the clinical evaluation required of any reliable diagnosis meeting *DSM* standards. The other argument is the correlation between Cavendish's behaviors and autistic behaviors. His behaviors taken one or a few at a time invite alternative explanations, but there is only one explanation that agrees with most of them and excludes none, autism.[148] The correlation could be explained by Cavendish's choices, but it would be a remarkable coincidence.

Let us tentatively agree with Sacks that Cavendish was probably autistic, and let us also agree that any diagnosis of autism for a person living in the eighteenth century is subject to uncertainties. In light of the agreement, and given the ever unsettled state of medical definitions and diagnostic criteria of autism, it is reasonable to speak of Cavendish as having a cluster of traits rather than to make the essentialist claim that he was autistic, in the way we say a person was blind, for example. We would regard the cluster of traits known from Cavendish's adult years as sufficient for us to talk about him as a person who very likely had autistic traits, not just autistic-like traits, at the same time acknowledging that the label "autism" is problematic. We would recognize that whatever wording we adopt and whatever weight we give to the historical evidence, we cannot alter a basic reservation: any autistic diagnosis of Cavendish has an irreducible speculative element. This approach is compatible with the scientific caution of Baron-Cohen, who writes: "there are clues that Cavendish may have had some degree of Asperger's syndrome. He shows abnormalities in social relationships, communication, and some routine-bound repetitive behavior. We must assume that his scientific pursuits were strongly obsessional in nature. However, missing from the historical record are any details of his childhood."[149]

There are "degrees of autism," Baron-Cohen writes, and "you could have a little or a lot of it."[150] It is hard to know how much Cavendish may have had, since he was highly intelligent, and by the time we get to know him he had had a long while to learn how to adapt to or to conceal certain difficulties. If, as we tentatively assume here, the hypothesis of autism holds the advantage, Cavendish had a sufficient degree of autism that if he were to undergo a psychological evaluation today, he would probably be diagnosed with autism. This statement is hypothetical in another way too: he would not have sought help, since he was getting along fine with his life. A diagnosis of autism does not imply a need for treatment.

According to the official terminology of the *DSM*, Cavendish's autism would have been either Asperger's syndrome or high-functioning autism, both located at one end of the "autism spectrum disorder."[151] Baron-Cohen uses an alternative terminology because he considers it uncertain if the two high functioning classes should be called "disorders" in

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[148] Lidbetter would seem to have something like this in mind where he says that only by acknowledging that Cavendish was autistic "can we get anywhere near attempting to understand Cavendish 'the complete man.'" (2009, 786).

[149] Baron-Cohen, quoted in James (2006, 63). Consistent with the quotation, James says that his profiles "are not to be regarded as case studies," 11. Cavendish's profile is on 63–68.

[150] Simon Baron-Cohen (2003, 157).

[151] In the new edition, DSM-V, autism has three levels of severity. The one requiring the least support applies to persons who have some difficulty initiating social interaction and responding to social overtures and may have little interest in social interactions. This, if any of the three, would apply to Cavendish.

the first place. Persons diagnosed with "autism spectrum *condition*" would, by definition, have social difficulties, but they would often have above-average nonsocial skills. The term "condition" acknowledges that they have a disability arising from neurobiological factors, but it is not a "*global* disability, and may in some individuals result in talent."[152] Cavendish exercised his talent with little or no sign of disability. Baron-Cohen's "condition" describes Cavendish's behavior better than "disorder."

How do we decide between the interpretations, or can we? Did Cavendish show "extraordinary singularities" because he was autistic, in which case he had no choice? Or, making allowance for factors other than autism that affect behavior, did he have a choice? Was his personality one of countless possible personalities compatible with a normal brain? It seems to me that the sources on Cavendish's life support both interpretations about equally well, and that the sources are too incomplete to decide between them with high confidence. Coming to psychology as an outsider, I am a part of the limitation. For my part, I think it is doubtful we can ever know the answer.

Many readers will surely agree with Sacks that the evidence for Cavendish's autism is compelling. Those who do not agree on the grounds of evidence may still have a preference: based on what they know about Cavendish and about autism, and trusting to their *intuition*, they may decide that Cavendish was or was not autistic.

With any psychological evaluation of a historical figure, a red flag comes up. The path I take through a familiar minefield of objections to psychologizing the nonliving allows me to introduce scientific literature into the sources on Cavendish's life without giving him a label, which constantly undergoes revision. From a psychological perspective, aspects of Cavendish's life and work are brought together through a common explanation rather than through metaphor and analogy if at all. This is clear if our preference is for an autistic diagnosis, but if we find Cavendish to be an English eccentric of his time who lacked the biological basis of autism, still his traits of shyness or possibly introversion account for a range of his behaviors and correlate them. With the benefit of clinical observations, Cavendish appears less weird, and he joins the human race. The latter would be a truism if it were not for a popular characterization of him as robot. Far from disparaging Cavendish, a psychological view endows this truth-seeker with considerable humanity. His strangeness is seen as normal behavior for a minority of persons who share his disorder or eccentric personality traits. We have a different view of him and a different feeling about him, affecting our interpretation of him.

For all of his privileges and native gifts, Cavendish had a psychological liability, whatever its origin, over which he had little if any control. Its outward expression took the form of extraordinary shyness and embarrassment, which could be viewed as indications of unconfidence, however unfounded it was. From an objective standpoint, there is no question that he felt distress in some personal encounters. We have only to recall the image of Cavendish at Banks's door, frozen in place until new arrivals forced him to enter. What is important for his scientific work is that he had got to Banks's threshold, and that he *did* cross it to join the guests, some of whom were likely to be discomforting strangers. He did not allow his shyness to stand in the way of a public life and with it a successful activity in science. Day in and day out he arrived at the threshold, so to speak, and crossed it and made his entrances. Had he not been so determined, he might still have pursued science, but it would have been

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[152] Baron-Cohen (2008, 14).

as a reclusive hobby. To contribute to science required him to come into society and assert his presence. He did what was necessary to achieve what he desired.

In Cavendish perhaps more than in any of his contemporaries, the traits of temperament and character reinforce the traits required of a scientific researcher. We see this in the value he placed on facts, in the objectivity of all of his dealings, and in the path of truth he followed with such tenacity. The persons who knew Cavendish best were those who knew his work best. What one such person Davy saw as "great" in Cavendish is inseparable from the way of life he chose, one of natural philosophy. In this outcome, it makes no difference whether his "extraordinary singularities" were an expression of an autistic disorder or an eccentric expression of the normal behaviors he grew up around. A choice and act of will were required in either case.

Consistent with his cautious nature, Cavendish was conservative, wary of "fashion." This was evident in his traditional dress, in the entire way he moved through the world. We remind the reader of a few instances of this behavior. In the dissentions of the Royal Society, he supported the old leadership rather than the rebels. His reaction to a new way of dividing instruments was to show that the old way could be made to work better. In electricity, his model was Newton's *Principia*, nearly a century old. He preferred the old theory of heat to the new, and the same was true in optics. He kept his distance from the trend to base natural philosophy on imponderable fluids or the ether. He selected his scientific problems from problems others had studied rather than inventing new ones. He favored improvement in accuracy over discovery. By his way of life, however, he willy-nilly worked for great changes over which he had no control.

The historian Herbert Butterfield described the civilization that emerged from the Scientific Revolution as dissolving all traditions before it, "having eyes for nothing save a future of brave new worlds," a civilization "exhilaratingly new perhaps, but strange as that of Nineveh and Babylon."[153] One of its bearers, Henry Cavendish appeared strange to his contemporaries. That may have had less to do with his eccentricities than with the intensity with which he lived a life of naural philosophy. He helped build a brave, new scientific civilization, though he would not have described his work in any such terms. His contribution was a byproduct of duty of service and a love of natural truth expressed in the exacting language of science.

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[153] Herbert Butterfield (1965, 202).

Appendix I: Family Trees

Cavendish and Grey Family Trees

A biography of Henry Cavendish necessarily takes into account his social position. Both of Cavendish's paternal and maternal grandparents were dukes and duchesses. The family trees in this appendix begin with the grandparents and their siblings, follow with his parents' generation and then his own, and end with the one after his. By definition he was related to them all, though in most cases the relationship was not close, and he probably met only a small fraction of them, but because in eighteenth-century England, "family pride was such that members were usually well aware of distinguished connections,"[154] he would have known about them. Although he associated mainly with persons drawn from another society, one of his own choosing, that of scientific colleagues, he did not abandon the one he was born into, nor could he have. We recognize his aristocratic roots by his style of living, his property, and his will, which left his vast fortune to Cavendishes close to the center of the clan. The dukedom would eventually pass to descendants of his principal heir Lord George Augustus Henry Cavendish, who appears in the last column of the Cavendish family tree. Unless readers of this book are specialists in the history of the period, the names are unfamiliar to them, but they cannot miss the titles that go with them. Dukes, duchesses, and earls are commonplace, as is often great wealth. The family trees reflect the obligation of the head of the family to reproduce himself, taking the form of large families to insure a male heir. They also reflect the fragility of life at the time, even for the most privileged. Five children of the second duke of Devonshire died before their parents. Five children of the first duke, his father, not shown, died before their parents. All five sons of the Duke of Kent died before him, extinguishing the Kent line. Marriages normally, though not always, took place between persons of more or less the same social standing. Not shown on the family charts are illegitimate children, of which the first duke of Devonshire had several, born of unions with an actress and an aspiring actress. The family trees show that many members of the extended Cavendish family did not marry. As a wealthy, single aristocrat, Cavendish was not out of the ordinary. The two family trees in the following pages are nearly complete and, within the limits of their sources, accurate.[155]

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[154] John Cannon (1984, 28).

[155] The main sources used in developing the family trees are the following. Printed books such as Burke's, Cokayne's, and Debrett's peerages and the *Dictionary of National Biography*. Online geneological resources such as "The Peerage." Wills from the main probate court in England, the Prerogative Court of Canterbury. Memorials: for the Cavendish family, a record of dates of death and ages of members of the family interred by custom in All Saints Church in Derby; for the Grey family, photographs taken inside the Grey Mauseleum in Flitton, Bedfordshire. *Burke's Peerage and Baronetage*, 106th ed. C. Mosley, 2 vols. (Crans, Switzerland: Burke's Peerage, 1999). George Edward Cokayne, *The Complete Peerage of England, Scotland, Ireland, Great Britain and the United Kingdom: Extant, Extinct, or Dormant*, vols. 1–3 (Gloucester: A Sutton, 1982). *Debrett's Peerage and Baronetage* (2008). John Charles Cox and William Henry St. John Hope, *The Chronicle of Collegiate Church or Free Chapel of All Saints Derby* (London, 1881). "The Peerage: A Genealogical Survey of the Peerage of Britain as Well as the Royal Families of Europe," compiled by D. Lundy (http://www.thepeerage.com). English Heritage,

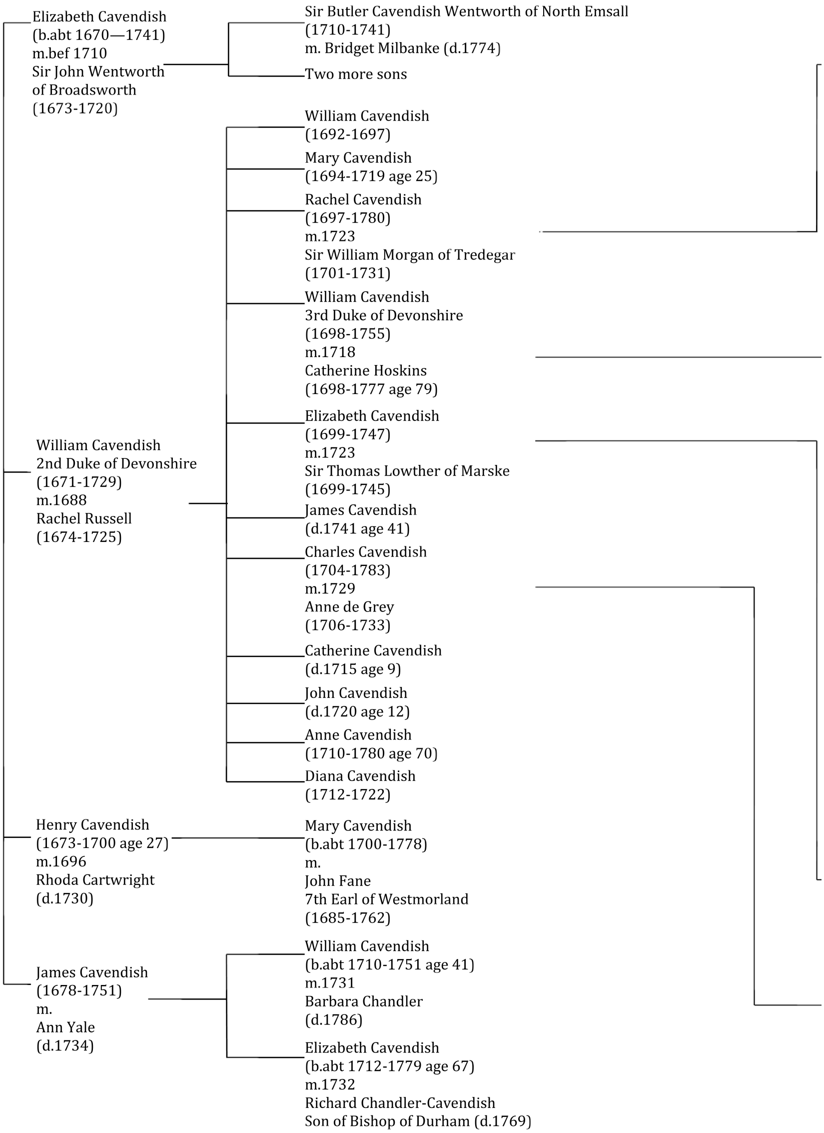

Figure 18.1

"The de Grey Mauseleum," (http://www.bedfordshire.gov.uk/CommunityAndLiving/ArchivesAndRecordOffice/CommunityArchives/Flitton/TheDeGreyMausoleumFlitton.aspx). The Cavendish and Grey family trees in this book are improvements of those in Jungnickel and McCormmach (1999).

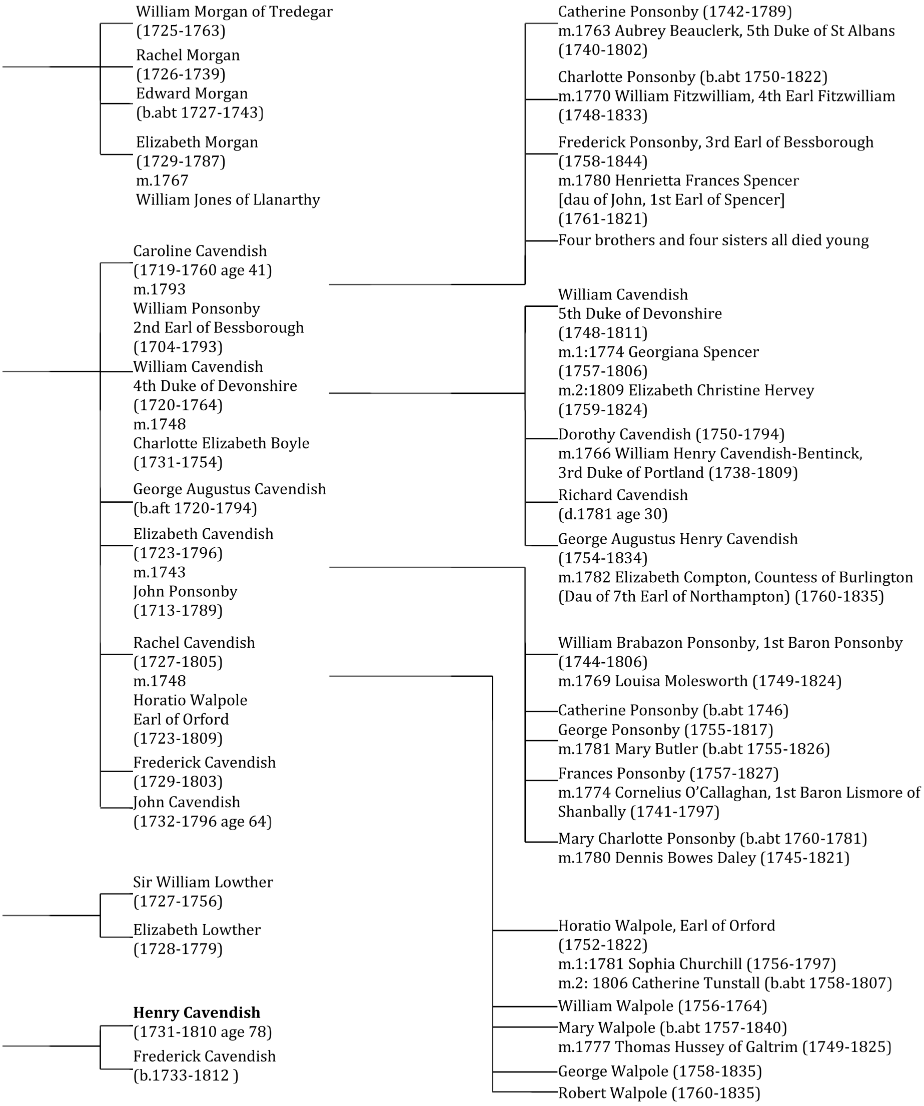
William Morgan of Tredegar (1725-1763)
Rachel Morgan (1726-1739)
Edward Morgan (b.abt 1727-1743)
Elizabeth Morgan (1729-1787) m.1767 William Jones of Llanarthy
Caroline Cavendish (1719-1760 age 41) m.1793 William Ponsonby 2nd Earl of Bessborough (1704-1793)
William Cavendish 4th Duke of Devonshire (1720-1764) m.1748 Charlotte Elizabeth Boyle (1731-1754)
George Augustus Cavendish (b.aft 1720-1794)
Elizabeth Cavendish (1723-1796) m.1743 John Ponsonby (1713-1789)
Rachel Cavendish (1727-1805) m.1748 Horatio Walpole Earl of Orford (1723-1809)
Frederick Cavendish (1729-1803)
John Cavendish (1732-1796 age 64)
Sir William Lowther (1727-1756)
Elizabeth Lowther (1728-1779)
Henry Cavendish (1731-1810 age 78)
Frederick Cavendish (b.1733-1812)
Catherine Ponsonby (1742-1789) m.1763 Aubrey Beauclerk, 5th Duke of St Albans (1740-1802)
Charlotte Ponsonby (b.abt 1750-1822) m.1770 William Fitzwilliam, 4th Earl Fitzwilliam (1748-1833)
Frederick Ponsonby, 3rd Earl of Bessborough (1758-1844) m.1780 Henrietta Frances Spencer [dau of John, 1st Earl of Spencer] (1761-1821)
Four brothers and four sisters all died young
William Cavendish 5th Duke of Devonshire (1748-1811) m.1:1774 Georgiana Spencer (1757-1806) m.2:1809 Elizabeth Christine Hervey (1759-1824)
Dorothy Cavendish (1750-1794) m.1766 William Henry Cavendish-Bentinck, 3rd Duke of Portland (1738-1809)
Richard Cavendish (d.1781 age 30)
George Augustus Henry Cavendish (1754-1834) m.1782 Elizabeth Compton, Countess of Burlington (Dau of 7th Earl of Northampton) (1760-1835)
William Brabazon Ponsonby, 1st Baron Ponsonby (1744-1806) m.1769 Louisa Molesworth (1749-1824)
Catherine Ponsonby (b.abt 1746)
George Ponsonby (1755-1817) m.1781 Mary Butler (b.abt 1755-1826)
Frances Ponsonby (1757-1827) m.1774 Cornelius O'Callaghan, 1st Baron Lismore of Shanbally (1741-1797)
Mary Charlotte Ponsonby (b.abt 1760-1781) m.1780 Dennis Bowes Daley (1745-1821)
Horatio Walpole, Earl of Orford (1752-1822) m.1:1781 Sophia Churchill (1756-1797) m.2: 1806 Catherine Tunstall (b.abt 1758-1807)
William Walpole (1756-1764)
Mary Walpole (b.abt 1757-1840) m.1777 Thomas Hussey of Galtrim (1749-1825)
George Walpole (1758-1835)
Robert Walpole (1760-1835)

Figure 18.2

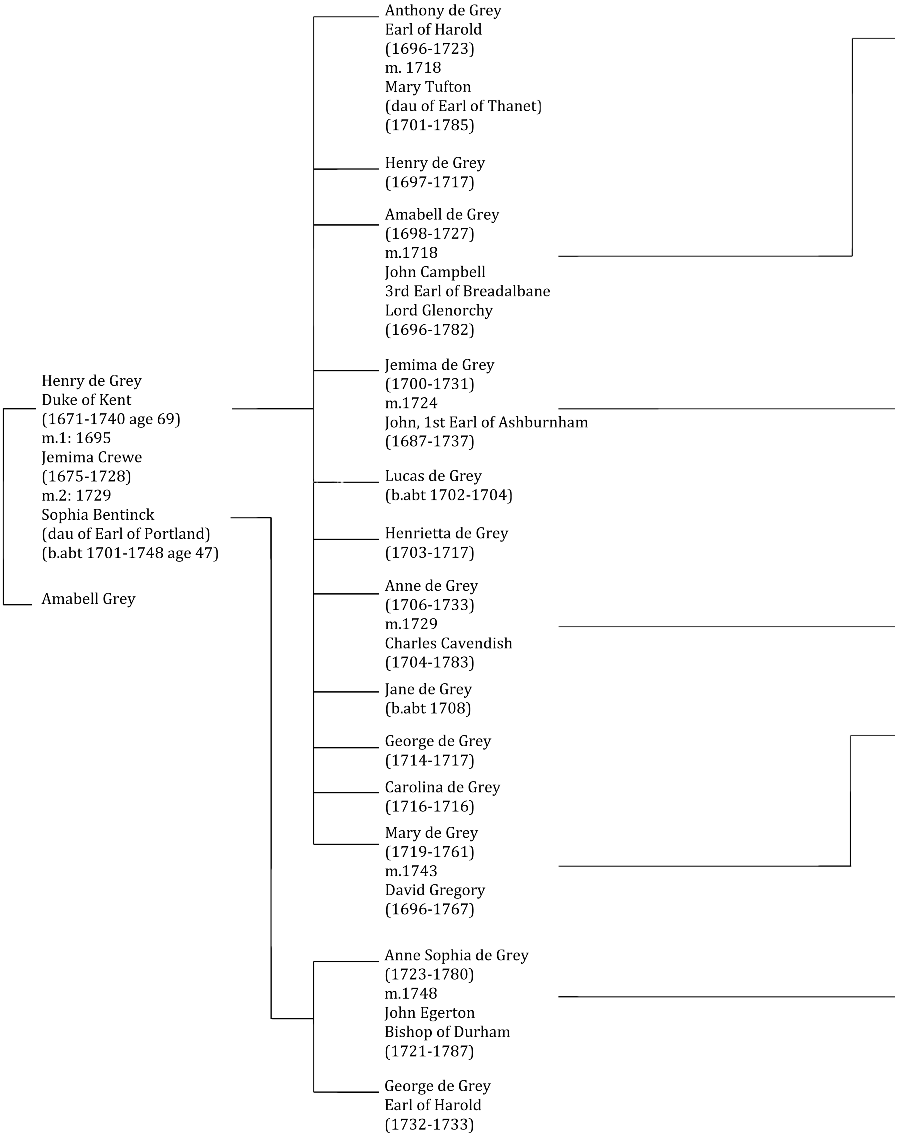
Henry de Grey
Duke of Kent
(1671-1740 age 69)
m.1: 1695
Jemima Crewe
(1675-1728)
m.2: 1729
Sophia Bentinck
(dau of Earl of Portland)
(b.abt 1701-1748 age 47)
Amabell Grey
Anthony de Grey
Earl of Harold
(1696-1723)
m. 1718
Mary Tufton
(dau of Earl of Thanet)
(1701-1785)
Henry de Grey
(1697-1717)
Amabell de Grey
(1698-1727)
m.1718
John Campbell
3rd Earl of Breadalbane
Lord Glenorchy
(1696-1782)
Jemima de Grey
(1700-1731)
m.1724
John, 1st Earl of Ashburnham
(1687-1737)
Lucas de Grey
(b.abt 1702-1704)
Henrietta de Grey
(1703-1717)
Anne de Grey
(1706-1733)
m.1729
Charles Cavendish
(1704-1783)
Jane de Grey
(b.abt 1708)
George de Grey
(1714-1717)
Carolina de Grey
(1716-1716)
Mary de Grey
(1719-1761)
m.1743
David Gregory
(1696-1767)
Anne Sophia de Grey
(1723-1780)
m.1748
John Egerton
Bishop of Durham
(1721-1787)
George de Grey
Earl of Harold
(1732-1733)

Figure 18.3

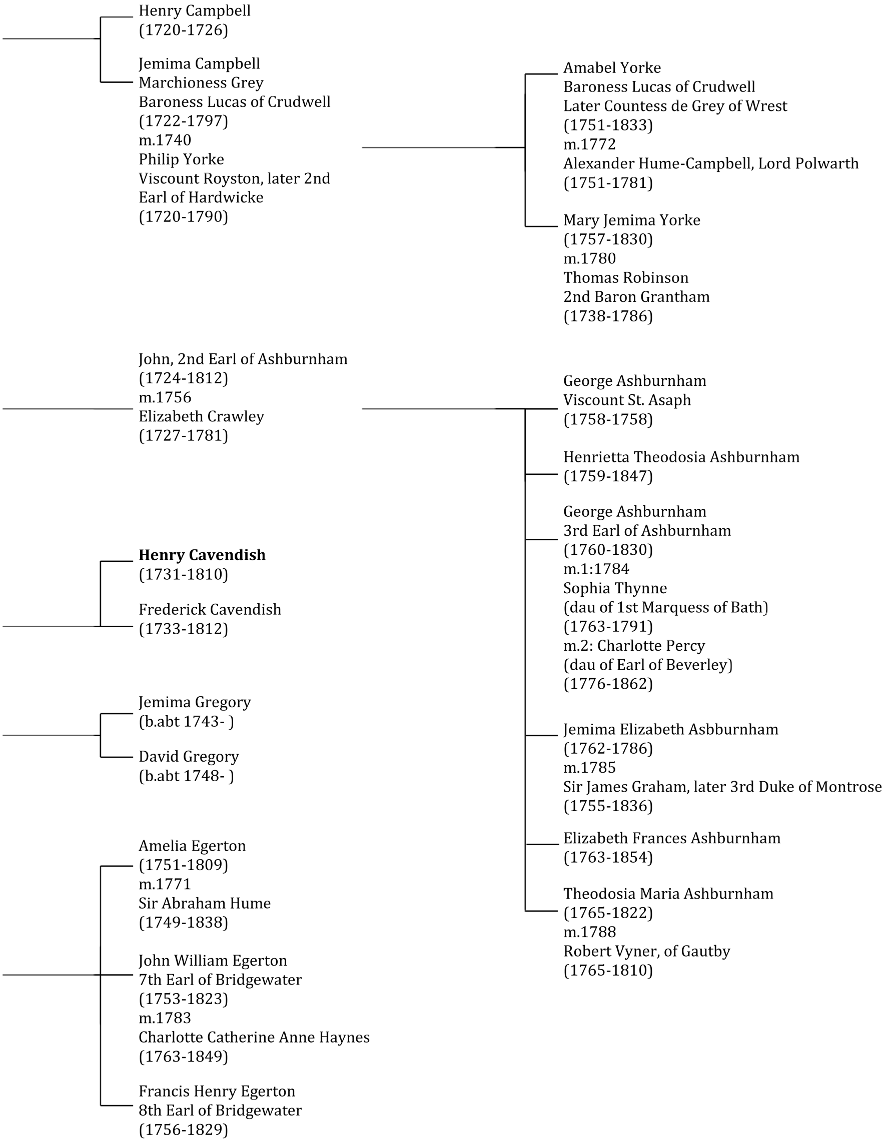
Henry Campbell
(1720-1726)
Jemima Campbell
Marchioness Grey
Baroness Lucas of Crudwell
(1722-1797)
m.1740
Philip Yorke
Viscount Royston, later 2nd
Earl of Hardwicke
(1720-1790)
Amabel Yorke
Baroness Lucas of Crudwell
Later Countess de Grey of Wrest
(1751-1833)
m.1772
Alexander Hume-Campbell, Lord Polwarth
(1751-1781)
Mary Jemima Yorke
(1757-1830)
m.1780
Thomas Robinson
2nd Baron Grantham
(1738-1786)
John, 2nd Earl of Ashburnham
(1724-1812)
m.1756
Elizabeth Crawley
(1727-1781)
George Ashburnham
Viscount St. Asaph
(1758-1758)
Henrietta Theodosia Ashburnham
(1759-1847)
George Ashburnham
3rd Earl of Ashburnham
(1760-1830)
m.1:1784
Sophia Thynne
(dau of 1st Marquess of Bath)
(1763-1791)
m.2: Charlotte Percy
(dau of Earl of Beverley)
(1776-1862)
Henry Cavendish
(1731-1810)
Frederick Cavendish
(1733-1812)
Jemima Gregory
(b.abt 1743-)
David Gregory
(b.abt 1748-)
Jemima Elizabeth Asbburnham
(1762-1786)
m.1785
Sir James Graham, later 3rd Duke of Montrose
(1755-1836)
Elizabeth Frances Ashburnham
(1763-1854)
Theodosia Maria Ashburnham
(1765-1822)
m.1788
Robert Vyner, of Gautby
(1765-1810)
Amelia Egerton
(1751-1809)
m.1771
Sir Abraham Hume
(1749-1838)
John William Egerton
7th Earl of Bridgewater
(1753-1823)
m.1783
Charlotte Catherine Anne Haynes
(1763-1849)
Francis Henry Egerton
8th Earl of Bridgewater
(1756-1829)

Figure 18.4

Appendix II: Chronology and Publications

Henry Cavendish’s Chronology and Publications

- Born Sunday, 31 October 1731, in Nice, first child of Lord Charles Cavendish and Lady Anne (de Grey) Cavendish.
- Death of his mother, Lady Anne, on 20 September 1733, at Putteridge. Move of his father, Lord Charles, from Putteridge to Great Marlborough Street, Westminster, in 1738.
- Entered Hackney Academy in 1742.
- Entered St. Peter’s College, or Peterhouse, Cambridge University, as a fellow commoner, on 24 November 1749.
- First publication, in Latin, “Luctus,” in Cambridge University, *Academicae Cantabrigiensis Luctus in Obitum Frederici celsissimi Walliae Principis* (Cambridge, 1751) (Lament on the Death of Most Eminent Frederick, Prince of Wales).
- Left Cambridge without taking a degree, having been in residence until 23 February 1753, nearly the full time required for a degree.
- Probably subscribed to Felice Giardini’s musical Academy in London in 1758 or 1759.
- Proposed 10 November 1757, and elected 31 July 1760, member of the Society of Royal Philosophers (Royal Society Dining Club).
- Proposed 9 January 1760, and elected 16 January 1760, member of the Society of Arts.
- Proposed 31 January 1760, and elected 1 May 1760, Fellow of the Royal Society of London.
- First published research, appearing in a paper by William Heberden, “Some Account of a Salt Found on the Pic of Teneriffe,” *PT* 55 (1765): 57–60; read 7 February 1764.
- Elected 30 November 1765 member of the Council of the Royal Society, the first of many times.
- First published research under his own name, “Three Papers, Containing Experiments on Factitious Air,” *PT* 56 (1766): 141–184; read 29 May, 6 and 13 November 1766. For this work, he was awarded the Copley Medal of the Royal Society.
- “Experiments on Rathbone-Place Water,” *PT* 57 (1767): 92–108; read 19 February 1767.
- “An Attempt to Explain Some of the Principal Phaenomena of Electricity, by Means of an Elastic Fluid,” *PT* 61 (1771): 584–677; read 19 December 1771 and 9 January 1772.
- Proposed 21 January 1773, and elected 25 February 1773, Fellow of the Society of Antiquaries.
- Elected 8 December 1773 trustee of the British Museum.
- “An Account of Some Attempts to Imitate the Effects of the Torpedo by Electricity,” *PT* 66 (1776): 196–225; read 18 January 1775.

- "An Account of the Meteorological Instruments Used at the Royal Society's House," *PT* 66 (1776): 375–401; read 14 March 1776.
- Acquired a country house 34 Church Row, Hampstead, appearing in the rate books from 3 January 1782 through 17 September 1785.
- "An Account of a New Eudiometer," *PT* 73 (1783): 106–135; read 16 January 1783.
- Death of his father, Lord Charles, on 28 April 1783.
- "Observations on Mr. Hutchins's Experiments for Determining the Degree of Cold at Which Quicksilver Freezes," *PT* 73 (1783): 303–328; read 1 May 1783.
- "Experiments on Air," *PT* 74 (1784): 119–169; read 15 January 1784.
- "Answer to Mr. Kirwan's Remarks upon the Experiments on Air," *PT* 74 (1784): 170–177; read 4 March 1784.
- Bought a new townhouse 11 Bedford Square on 21 May 1784.
- "Experiments on Air," *PT* 75 (1785): 372–384; read 2 June 1785.
- Bought a new country house on Clapham Common on 18 June 1785.
- "An Account of Experiments Made by Mr. John McNab, at Henley House, Hudson's Bay, Relating to Freezing Mixtures," *PT* 76 (1786): 241–272; read 23 February 1786.
- "An Account of Experiments Made by Mr. John McNab, at Albany Fort, Hudson's Bay, Relative to the Freezing of Nitrous and Vitriolic Acids," *PT* 78 (1788): 166–181; read 28 February 1788.
- "On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitrous Acid, by the Electric Spark," *PT* 78 (1788): 261–276; read 17 April 1788.
- "On the Height of the Luminous Arch Which Was Seen on Feb. 23, 1784," *PT* 80 (1790): 101–5; read 25 February 1790.
- "On the Civil Year of the Hindoos, and Its Divisions; with an Account of Three Hindoo Almanacs Belonging to Charles Wilkins," *PT* 82 (1792): 383–399; read 21 June 1792.
- "Extract of a Letter from Henry Cavendish, Esq. to Mr. Mendoza y Rios, January, 1795," *PT* 87 (1797): 119–122; read 22 December 1796.
- "Experiments to Determine the Density of the Earth," *PT* 88 (1798): 469–526; read 21 June 1798.
- Became a proprietor of the Royal Institution on 10 February 1800, elected manager on 1 May 1800.
- Elected Foreign Associate of the Institute of France in 1803.
- "On an Improvement in the Manner of Dividing Astronomical Instruments," *PT* 99 (1809): 221–45; read 18 May 1809.
- Died 24 February 1810 at Clapham Common.

List of Abbreviations

BL	British Library, Manuscript Department
Cavendish Mss	Cavendish Scientific Manuscripts, Devonshire Mss Collection, Chatsworth
Devon. Coll.	Devonshire Mss Collection, Chatsworth
DNB	*Dictionary of National Biography*
DSB	*Dictionary of Scientific Biography*
DTC	Dawson Turner Collection
JB	Journal Book of the Royal Society
BM(NH)	Natural History Museum, Botany Library
PT	Philosophical Transactions of the Royal Society of London

Archives

American Philosophical Society
Bank of England
Bedfordshire Record Office
Birmingham Assay Office
Birmingham University Library
Bristol Record Office
British Library, Department of Manuscripts
Cambridge University Library
Camden Archive
Cumbrian County Record Office
Devonshire Mss Collection, Chatsworth
Fitzwilliam Museum, Cambridge
Gloucester Record Office
Greater London Record Office
Historical Society of Pennsylvania
Holborn Public Library
Lambeth Archive
Lancashire Record Office
National Archives of Canada
Natural History Museum, Department of Botany
Public Record Office, Chancery Lane
Royal Astronomical Society
Royal Botanic Garden, Kew
Royal Greenwich Observatory
Royal Institution of Great Britain
Royal Society of Arts
Royal Society of London
Sheffield Central Library
Society of Antiquaries of London
University of Wales, Swansea Library
Wellcome Historical Medical Library
Westminster Archive
Whitby Literary and Philosophical Society
Yale University Library

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Subject Index

Name Index