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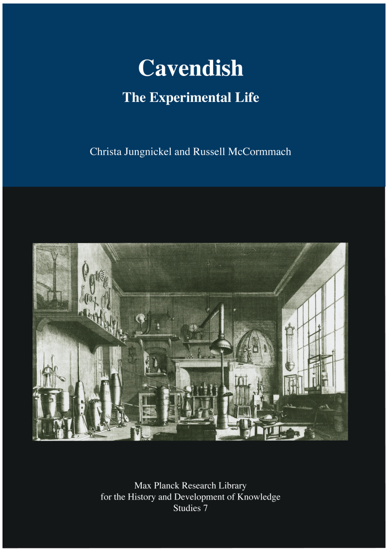
Christa Jungnickel and Russell McCormmach:

Electricity



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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]

[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

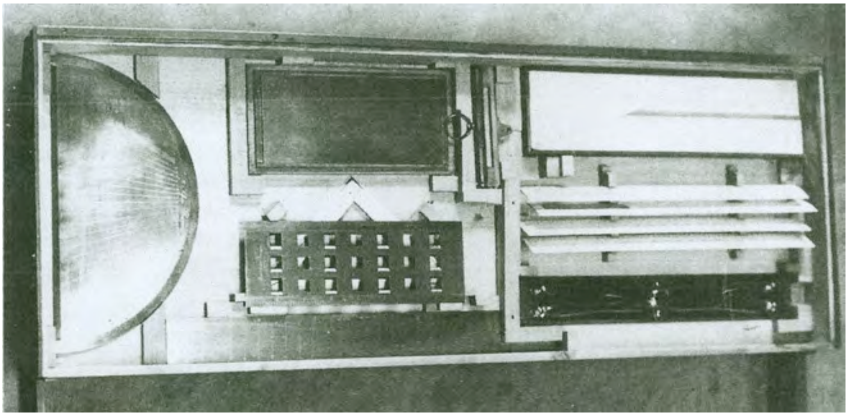

Figure 9.1: Mathematical Instruments. The instrument cases in this and the illustration below are drawers that fit into a cabinet belonging to Henry Cavendish. There are many scales and rulers, a brass globe map projection, an ivory triangle, and more, bearing the names of well-known instrument makers: Jesse Ramsden, Jonathan Sisson, John Morgan, and Fraser, presumably William Fraser. Photograph by the authors.

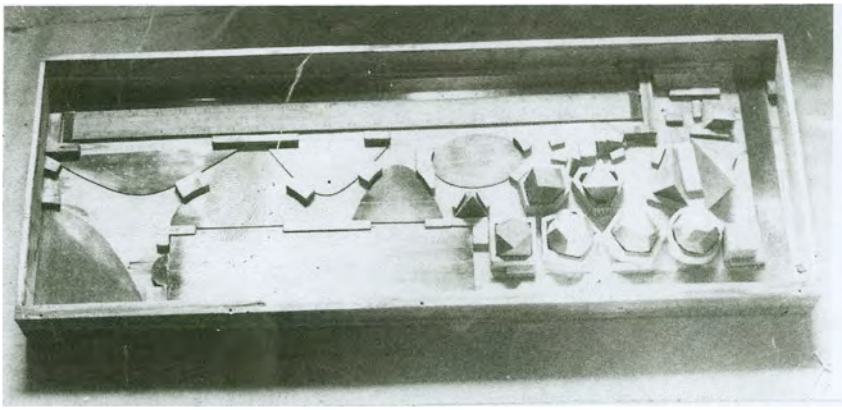

Figure 9.2: Mathematical Instruments. This drawer contains more brass and wood scales and rulers. The regular solids are made of boxwood. Cavendish's scientific papers contain drawings made with these instruments, including drawings from which plates were made for his publications. Photograph by the authors. Chatsworth. Reproduced by permission of the Chatsworth Settlement Trustees.

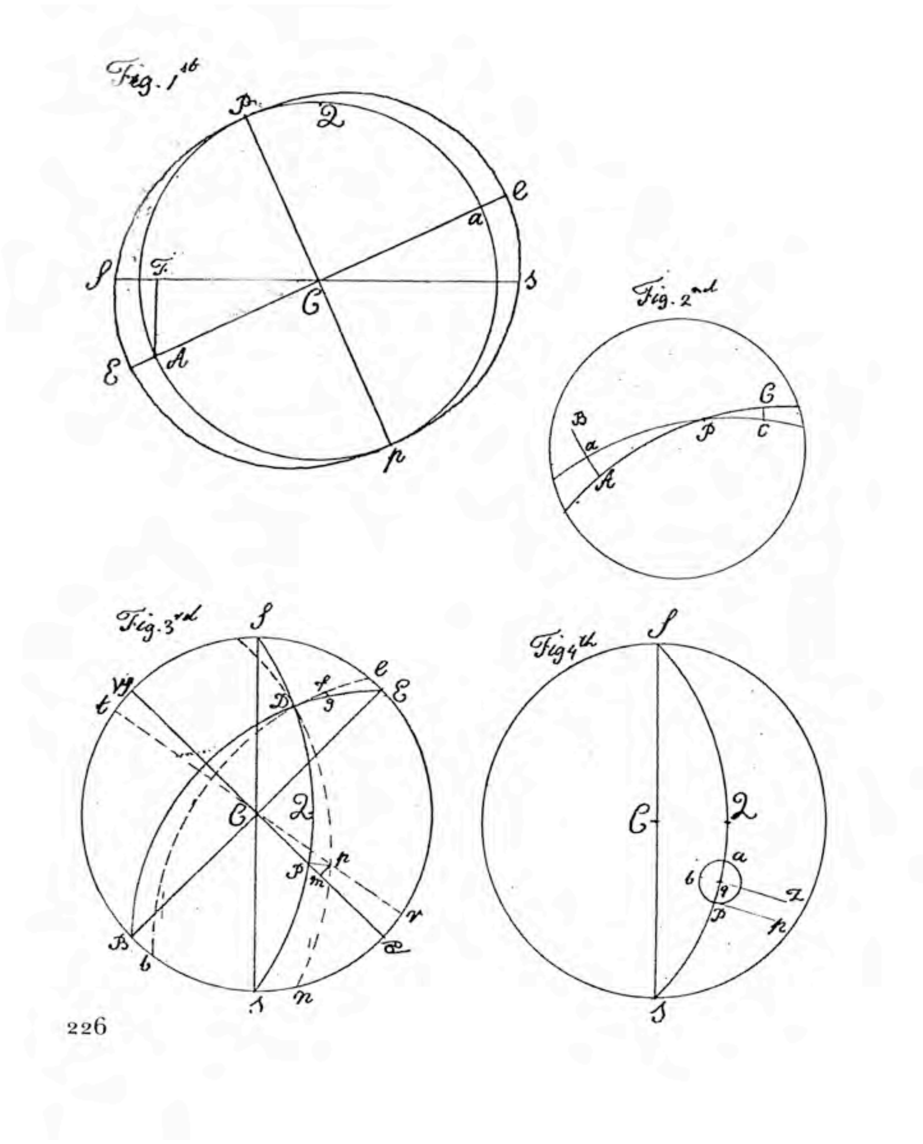

Figure 9.3: Mathematical Drawings. The figures are part of a carefully drafted but unpublished manuscript, "Precession of Equinoxes," Cavendish Mss VIII, 9. Reproduced by permission of the Chatsworth Settlement Trustees.

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

[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

[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

[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

[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

[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

[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]

[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

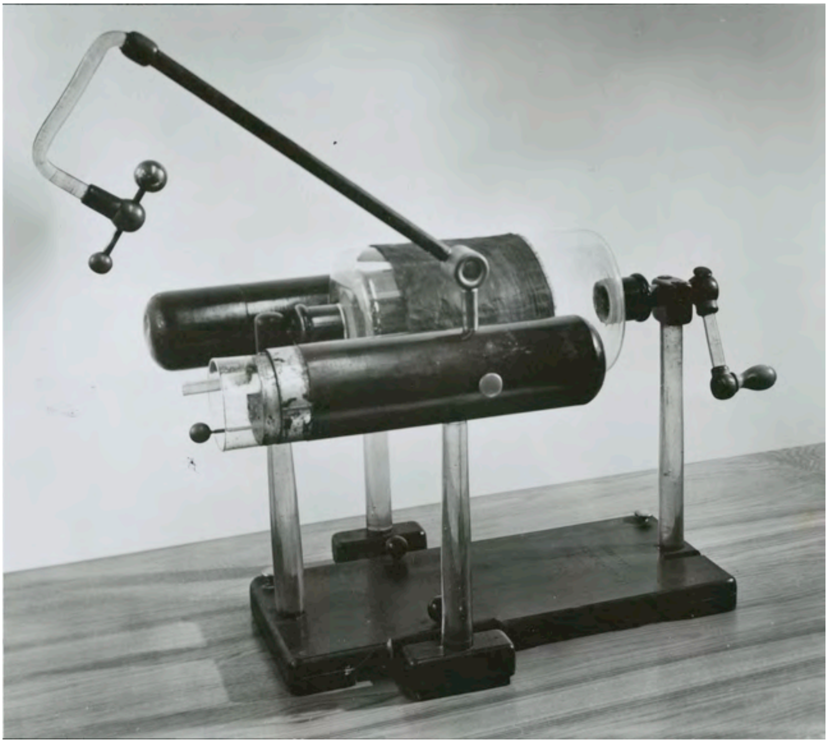

Figure 9.4: Electrical Machine. Made by Edward Nairne, stamped at the base "Nairne's/Patent/ Medico-Electrical/Machine," this instrument belonging to Henry Cavendish was presented to the Cavendish Laboratory at Cambridge by the duke of Devonshire around 1928. Its main parts are a glass cylinder with 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.

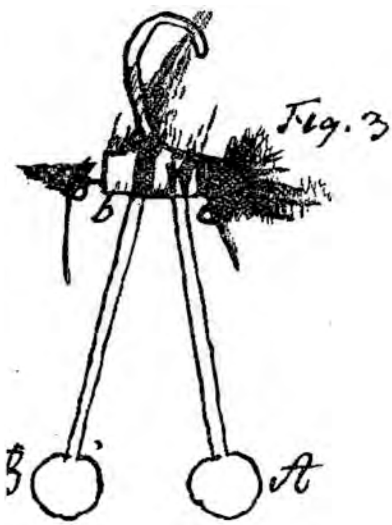

Figure 9.6: Cork-Ball Electrometer. This is the electrometer Cavendish used in his later experiments. It is made of two wheaten straws eleven inches long with cork balls at the bottom, each a third of an inch in diameter. At the top the straws are supported by steel pins on which they turn. The pins 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 +

[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]

[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

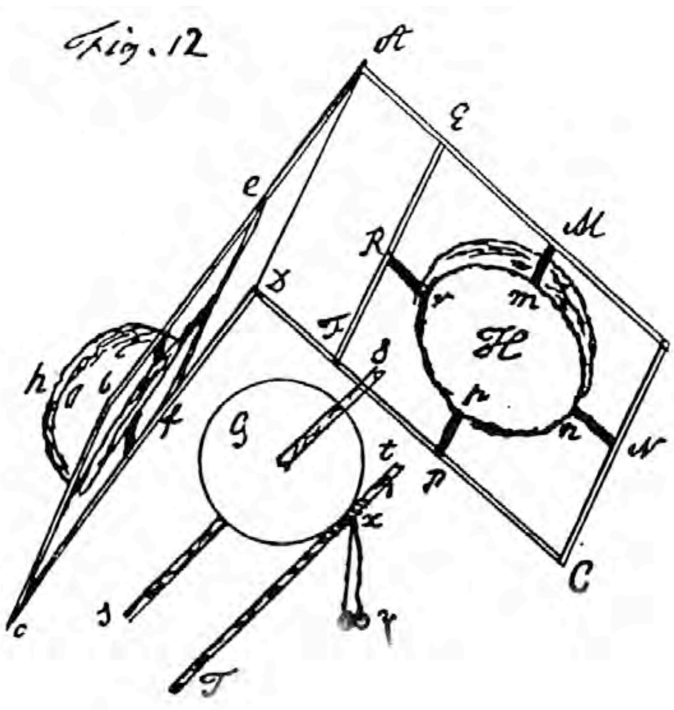

Figure 9.7: Apparatus for Determining the Electric Force. With this apparatus Cavendish demonstrated the distance dependency of the law of electric force. Upon closing, a hinged wooden frame brings together two hemispherical shells around but not touching an inner globe. The globe 12.1 inches in diameter 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.

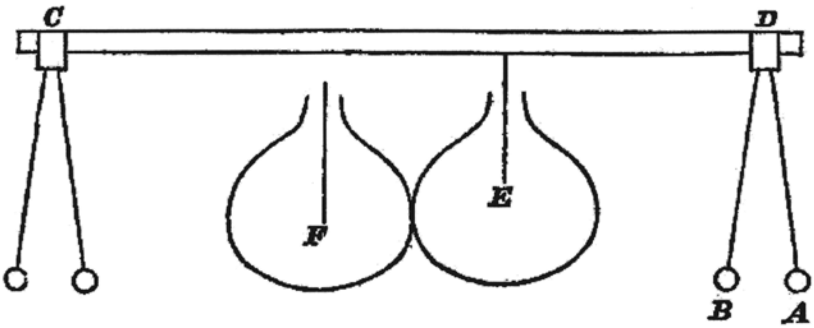

Figure 9.8: Apparatus for Determining the Electric Force. CD is a conducting rod. Electrometers suspended at C and D are similar except that the cork balls A and B can be made heavier by inserting weights. The experiment, which is described in the text, demonstrates that electric force between charged 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.

[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.

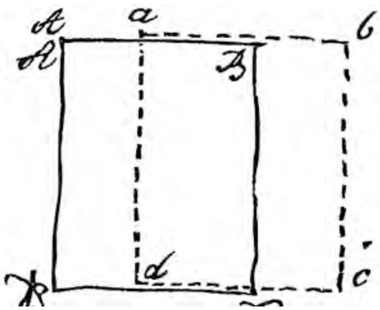

Figure 9.9: Trial Plate. Two flat tin plates, ABCD and abcd, slide over one another, increasing or decreasing the total size and with it the total charge. Cavendish (1879b, 1151–1216).

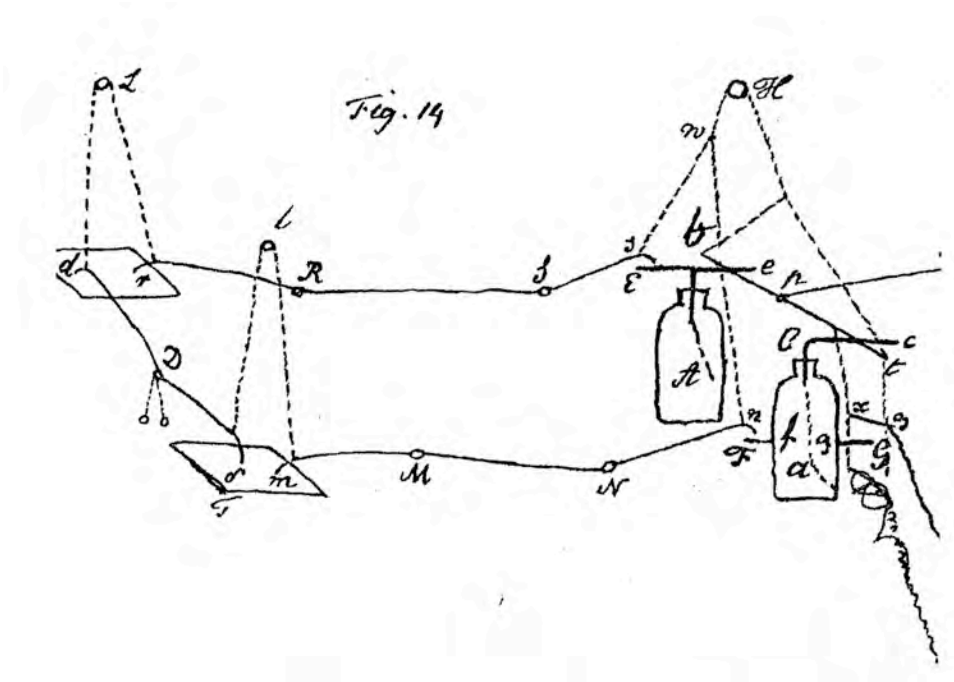

Figure 9.10: Apparatus for Determining Charges of Bodies. T is a trial plate. B is a body to be measured. It will be replaced by a second body b. The charges of the two bodies are compared, as explained in the text. A and a are Leiden jars, and D is an electrometer. Cavendish (1879b, 116–117).

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

[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

[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]

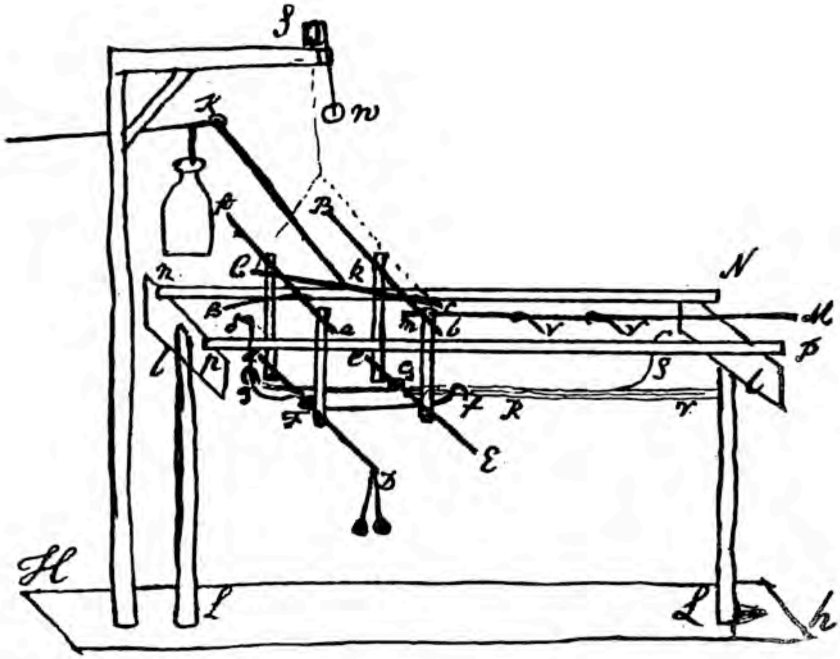

Figure 9.11: Apparatus for Determining Charges of Coated Plates. Standing on the floor, this seemingly rickety contrivance of wood and glass sticks, wires, and Leiden jar is actually portable and is described by Cavendish as compact. Two plates coated on both sides in the manner of a Leiden jar are electrified 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.

[80]Cavendish (1879b, 114). Stephenson (1938, 56).

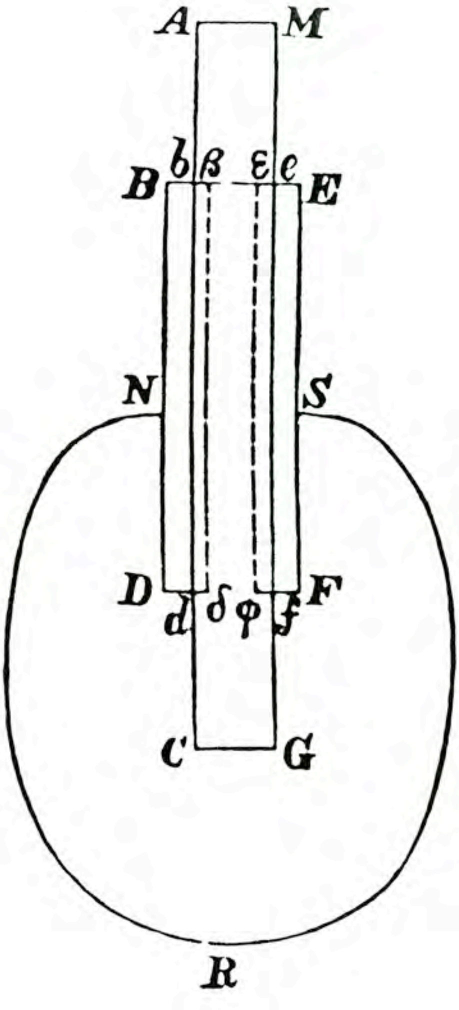

Figure 9.12: Leiden Jar. Cavendish analyzed the phenomena of the Leiden jar, or condenser, using this diagram. ACGM stands for a plate of glass seen edgeways, on either side of which are plates of conducting matter, such as metal foil. The dotted lines indicate the possible penetration of the electric 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.

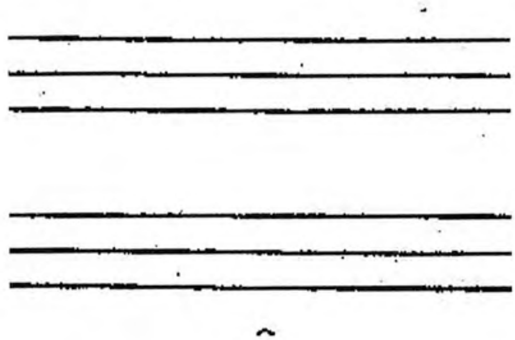

Figure 9.13: Electrical Structure of Glass. Cavendish found that the charges of glass plates coated with foil, which are simple Leiden jars, were eight times what they should have been by the theory. “There is a way of accounting for it,” which he gave two versions of. Cross-section of the glass is shown. 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

[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

[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

[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.

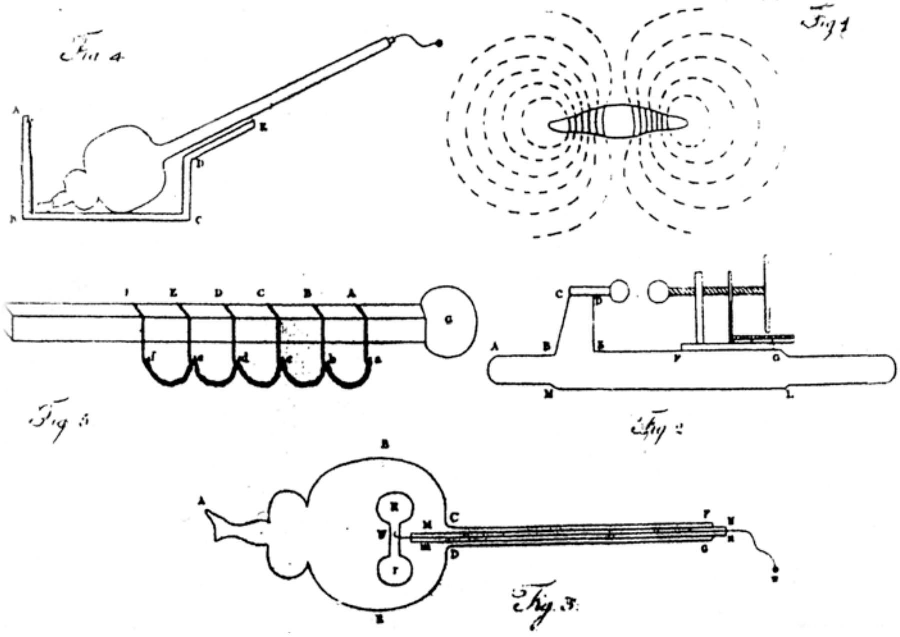

Figure 9.14: Artificial Electric Fish. In Figure 1, the solid line is the outline of an electric fish, or "torpedo," immersed in water. The dotted lines are the direction of flow of the electric fluid. When a person places his hands on the top and bottom of the fish or even only in water in the vicinity 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

[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-

[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

[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

[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

[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,

[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

[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.