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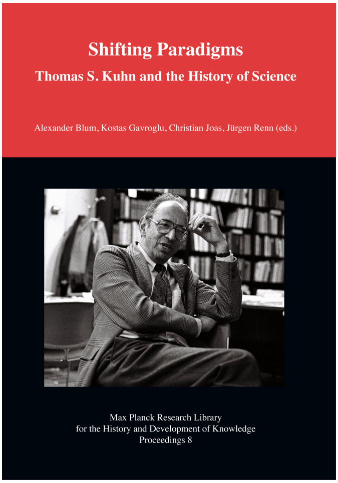
John Pickstone:

From Structures and Tensions in Science to Configurational Histories of the Practices of Knowledge



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Chapter 20
From Structures and Tensions in Science to Configurational Histories of the Practices of Knowledge

John Pickstone

Introduction

I begin this paper with notes in praise of Kuhn, but with a sense of regret that some of his best in insights have barely been followed up—as yet. The Kuhn I praise is not primarily the author of paradigms. When I was a student in London around 1970, they were a part of the Popper-Kuhn-Lakatos-Feyerabend arguments that were then central to history and philosophy of science; but paradigms were only one way of historicizing science, and other guides were becoming available, especially for those of us who came to focus on the history of biology and medicine—notably Canguilhem, Foucault, Toulmin, Collingwood and the German historians of medicine who had emigrated to Baltimore in the 1930s. The Kuhn of the history of quantum physics has remained beyond me; but from its appearance, I valued Kuhn's essay on the tension between mathematical and experimental traditions in the history of the physical sciences (Kuhn 1976). The article seems well cited, but to my knowledge no historians of physical sciences have persistently built upon its suggestive arguments and promising design. Perhaps the closest is Peter Galison's *Image and Logic* which highlights the tensions between experimental and theoretical physics (Galison 1997), and aspects of the work of Andrew Warwick, for example his demonstration of the different traditions into which Maxwell's work was taken up (Warwick 2003).

I appreciate Kuhn's approach because I have tried to work in a similar way, though I did not in fact follow Kuhn. Mostly I took my methods and directions from Foucault and from the history of medicine, but I used Kuhn's article, and my conversations with colleagues, to assure myself that my arguments also applied to the history of physical sciences. To my mind, that article demonstrated some of the key methodological moves which our subject needed—and still does.

What then are the strengths of that article, as a sketch for a general history of science—at least. First and crucially, the demonstration that science is not unitary; that the history of natural knowledge is better modeled through changing relations between contrasting traditions. This simple move provides the historian with a dynamic structure, in tension, over time. Each of the component traditions had its methods and characteristic forms of development; the tensions provide part of the dynamism. This historiographic tactic could have made room for systematic methodological pluralism in science and historiography; its openness should have defused jejune fears of 'grand narratives'. It can provide a strong periodization, but with plenty of room for uneven development; it makes sense of the second scientific revolution, which in my experience is commonly accepted and rarely analyzed. And it opens nice questions both about national styles and the history of mathematics. Thus the costs of its neglect seem to me considerable.

Much of the excellent historiography since the 1970s has presumed a certain unity of science, at least by failing to establish any systematic distinctions between different kinds of science. This seems as true of sociology of science or material cultures studies as it is of Latourian accounts. Studies 'by site' have brought us laboratory, field and museum sciences, but they tend to depict a common set of methods modified in response to by different environments. The literature on field science, for one, seems to show no particular methods, only shared constraints (Kuklick and Kohler 1996), and much the same is true of colonial science (Palladino and Worboys 1993). I once wrote a paper on museological sciences c. 1800 (Pickstone 1994), but the term museological is liable to mislead if intended as a single type, since it is clear that Natural History Museums supported both the natural history for which most of them were founded and the comparative anatomy which some of them developed from the early nineteenth century. Indeed museums were the sites in which several kinds of analytical sciences were built out of various kinds of natural history, and where those interactions and compoundings continued. For clarity, in my view, we do best as historical analysts to give priority to the projects or tradition, not to the sites (Pickstone 2012), and so we come back to Kuhn's paper. As far as I know, Kuhn never explored the physical-science sites in which mathematical and Baconian traditions interacted and produced new kinds of work, but he could have; and as we shall see below, there is now lots of historical work which can help answer the relevant questions.

Note that this museological example follows the same pattern as Kuhn's article in presenting two traditions as interacting and compounding. This is its advantage over another of the recent mechanisms for systematically breaking up the unitary history of science without reifying scientific disciplines over time or descending to the specificities of actors' labels at any particular time. The idea of styles in science was introduced at length by Alistair Crombie (1994) and has been subtly discussed by Ian Hacking; it has much to recommend it, and Kuhn's two traditions appear in Crombie's and Hacking's lists of styles (Hacking 1992, 1996). What is less obvious in their accounts, and in a more recent version (Kwa

2011), is the tensions, the compoundings of styles, and the subtle imbrications which make up much of scientific and technical practice, and to which Kuhn pointed.

This compounding and structural development is allowed for in principle, it is true, but it is not a prominent feature of style histories. It is noteworthy that when Hacking wrote on Foucault and shifts in knowledge c 1800, he connected Foucault's account of the eighteenth-century episteme to Kuhn's notion of immature science and to the Baconian tradition, but he did not discuss the parallel tradition of mathematical analysis. Nor did he follow either Foucault or Kuhn in trying to account for the structures of the sciences and the technologies emergent from about 1800—in medicine, zoology, economics and philology (Hacking 2002). I have tried to draw on both Foucault and Kuhn and to connect them constructively, as I will show below, but first I note other important approaches which seem to me to be limited in similar ways to style analysis.

A recent collection of excellent historical essays on observation shows how seventeenth-century understandings of observation developed, in part, through the interplay of two traditions of collecting: of serial *observationes* on the weather, medicine, planetary positions etc.; and of *experientia* related to crafts. (Daston and Lunbeck 2011). But once the focus moves beyond the seventeenth century these interplays seem absent from the narratives about observation, or at least from the summaries. It is as if observation had become an established practice to be found at various sites and times and in various disciplines; as if it did not thereafter matter how the observations were related to analysis or theory, or to contemporary questions of precision; as if observation was one kind of work, threaded through science, rather than interacting and being compounded with others in complicated, dynamic projects. Much the same might perhaps be said about 'objectivity' when it is analyzed primarily through shifts in moral economies, with little attention to the changing configurations of the complex intellectual and professional projects of which measurements, readings and interpretations were parts (Daston and Galison 2007, and see Porter 2008). In other places, I have tried to show how both these approaches might be enhanced by giving more attention to the changing structures of scientific projects, by histories of tensions, interactions and compoundings (Pickstone 2007, 2012). How then might we use this Kuhnian tactic, not just for the history of science as it existed in 1976, but for the much wider range of topics and concerns which our community of historians now addresses?

In this essay, I first consider the spectrum of present historiography and the expansion of the 'range' since Kuhn. I then show how models of working knowledges in tensions and in compounds may help us analyze the history of science, technology and medicine, as if one complex assemblage, from the early modern formations through to the mid nineteenth century. I further broaden the essay to consider how such models may also be useful for social and cultural sciences and their associated 'real world' practices, such as social action or painting. I conclude with some tentative notes on Romanticism and the reconfigurations of Art / arts and of 'science'. These later sections should to indicate how important understandings of the history of art can fit with and extend my model of the history of STM, especially for its main crux—the deep and complex shifts in the late eighteenth and early nineteenth centuries.

HPS and HSTM

Historians today want much more than Kuhn attempted to provide. We speak to a different present, in a different historiographical context, and we might reasonably ask for analyses that extend across all the sciences and technologies, including social sciences and technologies (or even, perhaps, all the other subjects that German speakers can call *Wissenschaft*). In as much as we are concerned with wide audiences for science (and history), we may also wish to include vernacular or mundane understandings and practices. That breadth would cover a variety well beyond anything Kuhn envisaged when he argued against the unity of science (Pickstone and Worboys 2011).

The Kuhn conference at Berlin helped make evident the great variety of ways in which the history of science has developed since Kuhn wrote. Simplifying, perhaps unfairly and certainly incompletely, I there contrasted two patterns. One was represented by the history and philosophy of science (HPS) I found in London c 1970, which is roughly that from which Kuhn produced his article. The other contains the history of medicine which I began to see in North America in 1971–73, and especially the history of science and technology which I found in Manchester and the Northern Seminar from 1974. It is in this second tradition, of History of Science, Technology and Medicine (HSTM) that I have since worked—hopefully without losing sight of the first.

In brief, HPS centered on philosophical issues, as exemplified in the Popper-Kuhn-Lakatos-Feyerabend sequence; it was 'sociologized' in Edinburgh and Bath, and 'anthropologized' in Paris. The resultant style has been practiced with distinction in Harvard, Cambridge UK and Berlin, and many other places. Which is not to say these places only included that tradition, but that work in these places has generally shared a focus on history of science, rather than technology or medicine, and has retained a strong interest in the first scientific revolution, without neglecting later periods.

The second formation I think of as rooted in the North of England, first in Leeds (especially under Jerry Ravetz) and then in Manchester (led by Donald

Cardwell), not forgetting aspects of Lancaster (led by Robert Fox) and the exemplary work at Bradford of Jack Morrell (see Ravetz 1971; Cardwell 1994; Fox 1995; Morrell 1972). The North American base was Philadelphia, where through Arnold Thackray the Northern tradition cross-fertilized with American history of technology (Tom Hughes) and the social history of medicine (Charles Rosenberg), including policy issues (Rosemary Stevens). In these places, the main stress was on knowledge at work, taking science and technology (and medicine) together, often for particular localities (not just sites). The chronological center of gravity was the nineteenth and then the twentieth centuries, and the social relations in question were not restricted to those of scientists and patrons; they included professional associations, hospitals, governments and wider publics. This HSTM tradition was important in the development of history of medicine in Britain, including the work at Oxford of Charles Webster who had been associated with Leeds. It was also linked with work that was initially marginal at Cambridge—notably that of Bob Young on history of biology and medicine, and of Martin Rudwick on history of geology (both of whom were formative influences for Roy Porter). As an approach it is now widespread—as the popularity of the acronym HSTM indicates.

That gross simplification may serve as a very rough guide to the social history of the subject in Britain, if we also note that while most of the entrants to these studies in the 1960s and 1970s had been trained in the sciences, entry from history graduates has substantially increased since, perhaps especially into history of medicine. That is one of the reasons for the loosening of the connection with philosophy of science and the tendency of historians of STM to define their work by place and time as well as field—for example, renaissance Italian medicine or British physics in the Cold War—seeing more connections with the wider history of the period than with issues in the theory of science or of STM. This tendency has been accentuated by developments in the sub-discipline of philosophy of science which seem abstruse and unhistorical to historians. A further complication is the growth of 'science studies' in ways which either make little connection with history, or which connect only with the history of late twentieth-century science, producing a 'deep present' rather than a history informed by contrasts over time.

Historians of knowledge know that academic geographies of the kind I have sketched in the last paragraph are always very complex. Happily, our present problems of delineation are ones of riches rather than poverty; but maybe we can hope to benefit by asking about the range of our shared theories of science or of STM. Can we still remain with the traditional HPS issues which have tended to assume a unity of science, to think of technology as the application of science, and to locate the crucial revolution in the seventeenth century? That form of history of science has tended to focus on experiment, and usually on physics; it 'worked outwards' to include chemistry and biology, along with observations and collections etc. But the more we widen out, the more that frame gives out, and the more we may be left with histories which—for all their subtlety to place, time and politics—lack generality. The resultant case histories remain a key part of what we need to speak to the present, but they may lose 'the past in the present' by having no common account of the development of STM.

By my work on Ways of Knowing I have tried to bridge the gap between the HPS where I began in London and the social history of science, technology and medicine we have tried to develop in Manchester—along with colleagues in many other places. To bridge across the sciences, technologies and medicine in a Centre for HSTM, I developed a model—not from the history of physics but from the social history of medicine; not from relatively simple case of knowledge creation in physics laboratories, but from instances of medical practice with defined social relations, including patients; not by thinking of knowledge as esoteric and then 'applied,' but seeing it included and embodied in mundane practices (Pickstone 2000). I have tried to treat ways of working symmetrically with ways of knowing, and have sometimes conjoined them as 'working knowledges' (Pickstone 2007).

I have argued for four such couples, four kinds of working knowledges, both practical and 'theoretical':

1. Understanding philosophical or symbolic 'meanings' and their use in communication;
2. 'Sorting' natural kinds and their use in crafts;
3. Reduction to elements, either mathematical or substantive, and their use in rationalization; and
4. Synthesis from elements, either mathematical or substantive.

Technically speaking, this is a scale of forms (Collingwood 1933). Later forms subsume the earlier, but the earlier also continue, and the relationships are often contested in ways which are context dependent. The compounding ensures that the method is analytical rather than simply taxonomical, a subtlety which seems to have escaped some recent commentators. In any given scientific project which involves more than symbolic meanings, there are relationships to be considered.

The key developments *between* the different classes of working knowledges correspond, as one would hope, to key 'moves' within the history of STM: naturalization, analytical reductions and the move from analysis to synthesis. But all three shifts are seen a reversible, indeed as continuing dynamic relations; hence the complex structures which historians and anthropologists have noted (but rarely analyzed), for example in modern medicine (Mol 2002).

Mapping these kinds and developments over time produces a narrative frame which highlights crucial changes around 1800, from when many forms of substantive analysis were invented. But this is not a model of 'the origin of science'; it is about perpetual shifts in the configurations of working knowledges—and in principle, as I have increasingly realized since the Berlin conference, it seems to work across the whole range of formalized Western knowledges, including social and cultural knowledges and practices.

There is room in this new story for Kuhn's paradigms (most of which seem to be new programs of analysis—mathematical or substantive), but paradigms are not primary. There is a much stronger place for Kuhn's essential tensions, for seeing different 'projects' in tension, and their various compoundings. In the rest of this paper, I want to show how Kuhn's insights can be related to the larger picture which I have tried to develop, for STM and then for wider fields. Maybe this will help explain my work to historians who know Kuhn better than they know history of medicine or biology. I begin with two key absences in Kuhn's essay; one set is easily explained, the other more suggestive.

Working Knowledges: The Early Modern Triad

For the early modern period, Kuhn pointed to the mathematical tradition extending back to the Greeks, and to an empirical or experimental tradition which was more recent, less structured, less esoteric, and closer to crafts and other everyday practices. This pairing now seems well established as two of the three main seventeenth-century traditions involved in the Scientific Revolution—the other being natural philosophy. The mathematical story was foundational for accounts of that revolution; what we may call the Baconian tradition has received much more attention of late, as the history of natural history and then by reference to the wider renaissance category of *historia* (Pomata and Siraisi 2005).

Kuhn discusses natural history only for Baconian 'experimental histories,' but the extension to the rest of what we might call 'extended natural history,' or even 'information sciences' seems relatively unproblematic. In Kuhn's experimental tradition, observations which one might term passive or active (that is, manipulative or experimental) were being collected, examined and sorted, more or less critically, in much the same ways as specimens. Whether the observations concerned minerals or electrical effects, birds or the stars, here makes little difference. Additionally, it is now clear the collecting and indexing of many kinds of text can be thought of in the same way, specifically as *historia literaria*. More generally, the 'sorting' of manuscripts and books might well be seen as the renaissance basis of all the other forms of *historia*; the humanities, here and elsewhere, preceded the natural sciences (Grafton 2011; Blair 2010).

The larger absence is natural philosophy, which Kuhn's essay barely discusses. It certainly could have, since one of the key points in Kuhn's own historiographical journey had been the realization that Aristotle was doing philosophy, not 'science'. In my terms Aristotle was understanding and explaining from a set of first principles which applied to human activities as well as the natural world. The best of recent historiography for or against the scientific revolution does indeed include natural philosophy as the third components (Schuster 1990). This is also very clear in the masterly survey by Floris Cohen, and in his own work which models the interactions of the Archimedean tradition (mathematical reductions), the Baconian (natural histories) and various philosophical traditions—through into the eighteenth century (Cohen 2007, 723).

This triadic model of early-modern knowledges and practices seems indeed to neatly summarize much of the best recent work in HSTM. We are concerned with the relations and interactions of three ways of knowing: the 'natural' branches, as it were, of philosophy, of mathematics and of *historia*. That triadic formulation can take us from physics to medicine and beyond because it relates to three forms of knowledge that seem to be fundamental and persistent: reasoning from first principles, the reduction of phenomena to mathematics, and all the descriptive and classificatory activities which make for 'information'—what Bacon called the sciences of memory. Naturalization and mathematization have long been seen as key aspects of the seventeenth-century scientific revolution; if we see them as continuing and unstable relationships, we can then see how they work in later periods of STM, and how they help comprise subsequent knowledge structures, at many levels. We may also note, here without argument, that each of the early modern genres had its characteristic form of utility—say, rhetoric and self-development, practical mathematics and craft. Thus we can also bridge across from practices *for* knowledge to practices *for* material advantage or improvement. This relation is *not* the application of science (as pictured in the nineteenth century), it is the knowledge *in* the liberal arts, broadly understood, from rhetoric to medicine, from astrology to architecture.

If these few sentences may indicate how Kuhn's suggestions could have been generalized across the range now required for the early modern period, we can then add a key point about the interactions. In the formal hierarchies of knowledge, philosophy once stood above mathematics and *historia*. The latter two were meant to be preparative components of natural philosophy, but mathematicians and naturalists came increasingly to challenge philosophy—through mathematical philosophies or by doctors turning from philosophical to practical knowledges. That much seems common ground among writers on seventeenth-century science, so too the important transformations in the mathematical tradition and natural philosophy, and the powerful extensions of the mathematical and Baconian traditions through the eighteenth century. Less clear is the importance of *interactions* between mathematical and Baconian traditions in the seventeenth. Kuhn downplayed them, arguing that most experiments which impacted on mathematical studies were but ‘thought experiments’; only Newton was allowed as a strong bridge, and his legacy remained two-fold—a mathematical tradition based on the *Principia* and an experimental tradition based on the *Optics*. For Kuhn, modern physics was a later creation: the required bridges were partly a matter of internal developments, partly due to new institutions; and they were created with difficulty. Much was achieved in France between circa 1780 and 1830; but even by the later nineteenth century, compounding was a work in progress (Kuhn 1976).

Other historians have been more positive about the achievements of the seventeenth century in compounding mathematical and Baconian traditions, while recognizing the strong contrary tendencies throughout much of the eighteenth (Cohen 2007; Chalmers 2012). I am not competent to properly judge this issue, but perhaps our learning more about the interaction circa 1800 may help us assess the extent to which those compoundings may have been realized, albeit insecurely, in the seventeenth century.

What Happened circa 1800 in STM?

Kuhn depicted modern physical disciplines as formed by the convergence of mathematical and experimental traditions. The former typically calculated the movements of matter, drawing for confirmation on common observations and concepts. The latter dealt with the (usually qualitative) interactions of things that had secondary qualities, so to speak, like colors or heat or static electricity. The art of producing new sciences was to find something that could be quantified, so the interactions could be mathematized (Heilbron 1980, 1993). One then had a science of light, say, which was more than ‘geometrical,’ or a science of heat. The physicists of the mid-nineteenth century then found ways of reducing these different sciences to a smaller number of wider ones. Chemistry was said to fit this model because chemical reactions, if not very mathematical, could at least be quantified.

Kuhn did not stop to worry about how one might characterize these new physical sciences beyond the linkage of mathematics and experiments. Maybe they were too familiar. Chemists took their elements for granted; historians of physics knew that heat and light were once elements—but that seemed a temporary glitch; and no one was asking how these new physical sciences were related to the biological and social, or even to technologies. Because Kuhn’s history carried us through to classical physics, we did not have to continue with historical characterizations. That may be a general problem with stories that have familiar endings: the strange precursors are studied but the familiar is taken for granted, and the connection with our present is assumed.

But step back now and look at the whole range of knowledge and practices around 1800, including the contemporary revolutions in industry, medicine, education, philosophy and the arts. Look at crystallography, stratigraphy, histology, comparative anatomy, comparative engineering, philology or political economy—and ask again what Kuhn's model can tell us. Return if you will, to the question of *one* or *many* sciences, and wonder when and why you could vote for one. Maybe because eighteenth-century natural philosophers often saw mathematical physics as *the* model, whilst natural history was only fit for refined play, 'merely professional' information—or imperial expeditions. Maybe because the Gentlemen of English Science c 1830 still saw the traditional mathematical sciences as outranking the novelties of chemistry and kindred recent formations (Morrell and Thackray 1981). Maybe because later nineteenth-century physics had common theories (for example, energy) even more extensive than the aspirations of the old rational mechanics. But Kuhn rejected all these unities, insisting on the crudities of experimental philosophy, and the independent births of different physical sciences. The more widely one looks, the more compelling is his model. In as much as science had unity, it was constructed from the disparate. Unity may have been a directing principle for some investigators, intellectually or politically, but it was not constitutive in the creation of new physical sciences, let alone the rest of the sciences and technologies.

So what, if anything, did these disparate sciences have in common? Mathematics, or even quantitation, will not stretch to stratigraphy, nor does it have much purchase for early studies of affinities or for organic chemistry; and it hardly appeared in the new biological, medical and social sciences. So maybe physics is not a good guide here; in some ways it was a residual subject—covering mathematical physics and what was left of experimental physical sciences when chemistry attained a new definition? One notes moreover the different histories of light, heat, electrostatics, magnetism and current electricity.

My own search began at the opposite end of the scale—with the distinguishing features of the new Paris medicine, especially the analysis of the human body into tissues. I knew from the work of Randall Albury that these had been seen as analogous with chemical elements, and that Cuvier's zoology and Magendie's experimental physiology also worked in terms of interactions between elementary organs or tissues (Albury 1972). From Foucault's accounts of the new sciences—of philology and political economy, as well as zoology—it was clear that they too involved new ideas of systems, with interactive parts, and perhaps developing over time (Foucault 1970). Comparative anatomy often proved a useful reference for these sciences, and for studies of machines or architecture.

Chemistry, however, with its pragmatic definition of elements and its compositional understanding of all other 'chemicals,' was undoubtedly the key reference for many other sciences. Their 'elements' too were no longer prescribed by natural philosophies but instead were unearthed pragmatically; so each new science was constituted by its elements—be they single or multiple, passive or active, related by structures or interactive, etc. Mathematization, quantitation, or manipulative experiments might be desiderata, but they were not always possible: stratigraphy was about structures, and so were comparative anatomy, histology and crystallography; in these subjects you could dissect to observe, but you could not interrogate interactions (Pickstone 2000, 2007).

So should one say that in these underprivileged fields one was left with mere observations? I think not. The distinctions between qualitative and quantitative, or between observation and experimental, do not exhaust the issues; indeed, in my view, their application may do grave damage to the historiography of this period, and others. Consider observations of nebulae and of planetary positions. The former, through the eighteenth century, may be classed as natural history; the second might also be so, for example if you were looking for particular astrological conjunctions. But if you are testing a mathematical analysis of planetary motions, or a synthetic model you have created therefrom, then you have reasons for particular kinds of accuracy. You are relating theories, however humble, to observations, and vice versa; that diadic relation *is* the project (Pickstone 2009).

If Cuvier had taught you how to do comparative anatomy, then you knew that parts of the body were related to each other by functions, and that different kinds of animal could be recognized by their different fundamental plans. In this scheme you could even make predictions about bits yet to be found (Rudwick 2005). You could radically alter classifications because you had understood deeper levels. Observation, thereby, was no longer mere description or classification, and perhaps objectivity was a moral demand in a new structural way. Perhaps it arose, at least in part, from the new need to test deeper understandings by means of surfaces, and *vice versa*.

John Herschel was clear about analysis in the different early nineteenth-century sciences: dissect phenomena to the limit, to their elements, and you have a new science (Herschel 1830, 93). William Whewell was even clearer: you have, in some sense, to know the 'idea' of the science to use it—and to clarify the idea. J. S. Mill was less clear and much less historical in assuming the terms to be obvious; but Comte knew that each of the sciences had its own basis, even though they were related to each other. Laudan (1968) remains a useful guide to these debates. Whewell still hankered for the old primacy of mathematical sciences (Yeo

2003); Comte did not; but they both knew that they were dealing with sciences that were new creations, from the mid-eighteenth century at the earliest.

I have called these new sciences analytical because, like chemistry, they dealt in elements peculiar to each particular science. The older and more general knowledges of natural history, mixed mathematics and natural philosophy continued alongside, and in some ways within, the new disciplines. One should not underestimate the number of new materials or phenomena discovered in the nineteenth century, or the continuing importance of detailed description and classification; natural history thrived as never before. But in the eyes of the new professionals, classifying was now to be based on subject-specific elements, which also helped explain structures and processes, and which might, or not, be subject to calculation. This move might also displace the general natural philosophies, for to outline the elements and understand their relations was to explore the 'philosophy of chemistry,' or whatever. Each of the new analytical sciences had its 'philosophy,' but they no longer needed to add up to one natural philosophy.

In this radically new structure of knowledge, more and more sciences were created, in parallel or through hybridizations. Different sciences could be pulled together by finding deeper elements; and substantive analysis could give rise to substantive synthesis, as had previously been the case in mathematical subjects. The combinatorial possibilities here are enormous, and so is the historiographical power: with simple working knowledges as elements, we can analyze very complicated situations (Pickstone 2007, 2011). The historiographical keys, as Kuhn knew, are disunity, differences, tensions, interactions, compoundings and configurations. But we can now push the argument much further than he did.

We might, for example, consider knowledge and action from the side of action, beginning with crafts where demystification of the process may be the equivalent of naturalization, and similarly unstable. Now as then, if actions are truly important then so is the self-preparation of the actors, and the energy that maybe needed for the requisite 'distancing'. As Collingwood outlined rather well, we have our own forms of magic—even for constructive technologies, to say nothing of the selling of the products (Collingwood 1936). As for analysis in technical practices, the best historians of technology know that much of industrialization involved the articulation of learned crafts with the working out of technical relationships, for example, about the duty of engines—rather than the appliance of science (Wegenroth 2003). Such relationships may prove to be contributory to sciences as Cardwell argued for doctrines of energy in the industrial revolution (Cardwell 1989). The exponents of analytical sciences liked to present technologies as products of their sciences; better, I think, to see analytical work as related to empirical specificities *and* to wider meanings—both in natural knowledge and in technical action.

But why should these kinds of reciprocal relations between knowledge and action be peculiar to work on the worlds of nature, or material technologies? Perhaps models of this kind are useful not just for nineteenth-century histories of natural sciences and technics, but also for social technics and social sciences, and indeed for the practices and analyses of the humanities and culture. A few notes may make clear the historiographical possibilities.

Looking Wider: Social and Human Sciences

Following Foucault, and without going much beyond the *historia*: analysis relations already outlined, we can note that several new social and human sciences not only variously extended traditions of natural history but followed the new biological sciences in their forms of analysis. I will take three examples here: history of art, mainly in France; social science and related practices, mainly in England; and philological analyses and practices, mainly in Germany. At the end of this essay, I will return to the complex question of how best to understand the practices which were called Art, rather than merely arts.

Historical and theoretical studies of Art were based on collections, often newly used for teaching. Describing and cataloguing were key activities, often with an imperial dimension. Discoveries in Egypt, or the extension of the Art canon to include gothic painting were empirical excitements that we too easily take for granted. But there were also new forms of analysis which paralleled biological approaches. Viollet le Duc's morphology of Gothic buildings in France and Hippolyte Taine's environmentalist explanations of art by social environments both owed much to the earlier work of Geoffroy Saint-Hilaire and Georges Cuvier at the Paris Museum of Natural History—which both these art-historians knew well. Intriguingly, their work on art history was also connected with attempts to destabilize the old genre structure in the Paris Ecole des Beaux Arts, and hence with new understandings of contemporary art (Walsh 2002).

As a second test, we can check the structure of the 'social sciences' in the mid nineteenth-century. The relation of French sociology to biological analysis was explicit and pervasive, at least in the form pioneered by Comte and extended by Durkheim. That is well known because histories of social sciences have often been disciplinary, in the academic sense, and have focused on sociological analysis. But if we want to grasp the scope and variety of nineteenth-century social action and knowledge, then Lawrence Goldman's account of the Social Science Association in Britain is a fuller guide. The Association's *Proceedings* certainly included much which could be seen as the social equivalents of natural history, and some of the studies extended to quantitative data which could be analyzed mathematically (using the new statistics). Lots of the material was 'practical,' for much of the public interest was aroused by reformers and philanthropists pursuing what might well have been called social arts, rather than social sciences. At the level of 'theory,' some British 'social scientists' were influenced by the reductionist analyses of the Ricardian economists, while others preferred the more Germanic, historical and inductive work of Whewell and his friends. Though relatively few, excepting the Positivists, were interested in *creating* systematic analyses of society, many referred to Herbert Spencer's analytical account of human society over time. This was heavily based on the idea of 'division of labour,' first developed in eighteenth-century political economy, and then in nineteenth-century comparative anatomy and physiology (Goldman 2005).

Comparative anatomy was also a reference point for the new philology (Amsterdamska 1987; Leerson 2012; Karstens 2012). Here the new developments, though based on British and French discoveries, were institutionalized mainly in German universities, where the pattern of development similar to that of the natural science, and comparable in range and volume. Indeed it can be argued that, just as Renaissance *historia* of texts seems to have led to the *historia* of nature, so the collections and University seminars of the German analytical philologists preceded the laboratories and research schools of the chemists, anatomo-physiologists and experimental physicists.

But in all these sciences—natural, social and human—the context for both 'historia' and analyses in German universities was not just collections: it was the will to research, and in some cases the placing of new subjects in the philosophical faculties rather than the professional faculties. Germany showed how new disciplines could flourish, not in the professional schools which had helped create them in France, but in universities with a research ethic—a new system of intellectual production which bears comparison with industrial capitalism. As academic histories tell us—for chemistry, physiology and also for fields such as philology—subjects were to be built as new disciplines, not as preparatory to professional training; all achieved 'autonomy,' more or less.

So runs the usual history of science, but again we need to be cautious, and look for continuing traditions including natural history and crafts, especially in relation with the liberal professions. Schools of medicine and law, and the teaching of languages had practical, normative goals; they were still in large measure vehicles of what had been called liberal arts—practices laced with knowledge and developed reflexively. They depended on histories of cases and professional crafts. To reduce these relations and practices to the building and subsequent application of analytical disciplines is bad history and was probably bad pedagogy—for humanities as for medicine or engineering.

Romanticism and the Creation of Art and Science

Kuhn's account of what happened to knowledge circa 1800, and mine too, rely heavily on France. If we stick with the natural sciences and technics, or even if we include the social sciences and the humanities closest to biology, then we might, to a first approximation, exclude much that was specifically German and focus on natural histories and crafts, analytical disciplines and practices—in a narrative which runs easily to our present. But if we want to account for the specificities of German forms of analysis, for new philosophies, and especially for the creation of the modern relationship between Science and Art, then we are forced to focus, albeit here briefly, on German developments. These may now lie outside the ken of most historians of STM, but they are central to histories of Art and of those parts of the humanities which depend on 'understanding'.

To begin to explain these features we must look, of course, to the formative roles of the new German philosophy—the Kantian Revolution, Idealism and Romanticism. Three points, at least, are critical in relation to the working knowledges model. Firstly the importance of new type of analysis which relied not on reduction to different elements but on tracing complex structures back to their basic or early forms—a method that was crucial for various kinds of biology as well as many kinds of historical studies (Cunningham and Jardine 1990). Secondly, the explorations of what came to be called 'subjectivity' and the possibility of analytical introspective psychology—in addition to extensions of experimental physiology. This connected with a new account of how texts (and maybe practices) were to be analyzed as systems of meaning—an account that was eventually called *verstehen* and made to distinguish *Geisteswissenschaften* from *Naturwissenschaften* (Smith 1997). Thirdly, the creation of a new view of fine art—not as a *techne* of objective representation which generalized according to classical rules, but as a result of inspiration through which artists recorded their individual and subjective responses to particularities (Shiner 2001; Abrams 1953). These three innovations were closely connected: a pervasive interest in 'ways of seeing' bound romantic art with German natural sciences and with new practices in the humanities.

The contrast between German approaches and French (and most British) analysis is very striking; indeed, for most of the century 1760–1860, Western Europe constituted a remarkable 'natural experiment' in the historical sociology of knowledge. While decompositional analysis came to dominate French science and French natural philosophy tended to be marginalized, German created a system of intellectual productivity around a novel reinvention of natural philosophy, based on formative ideas rather than the association of sensations. In many ways that experiment proved temporary for natural sciences, but for the humanities and

Art it remains foundational. The texts by J. T. Merz, through a century old, remain a key starting point for these developments in science, and in philosophy, not least for his account of nineteenth-century scientific analyses (Merz 1904–1912, esp. Volumes 1 and 2 on Scientific Thought).

We have come a long way from Kuhn and physics, and the end of a long article is not the place for deep histories of art-science relations. But if we are asking what happened to structures of knowledge circa 1800 we must at least note that the creation of Science in its modern English language sense seems to have been importantly co-constitutive with the contemporary creation of Art. Within the old triad of natural knowledges and practices, a range of parallel analytical sciences took prominence; 'science' and 'scientists' were invented to reify that new formation and assert its pre-eminence over such older arts as were said to require knowledge rather than inspiration. Over about the same period, as Kristeller argued around 1950 (Kristeller 1990) and Shiner has shown though social history, the various fine arts became reified as Art, underpinned by aesthetics and divorced from the other old arts—which came to be seen as 'applied art' (Shiner 2001). Thus the old range of arts were variously subjected to Science and / or Art, which came to be seen as parallel terms.

Attention shifted from the work of art to the artist, from the making of an object to the responsiveness of the creator, and to mental capacities which seemed to precede the making. In such ways the artist became equivalent to the new scientist rather than the craftsman. The new view of Art as primarily about the expression of personal response now stood opposed to a vision of Science as a federation of analytical disciplines and thus the sum of objective knowledge. As Abrams (1953) showed collaterally, as it were, poetry's significant 'other' in 1750 had been natural history; by the early nineteenth century, it was chemistry.

These new emphases were associated with the increasing self-consciousness and self-promotions of *scientists* (a new word for a new role) and *artists* (a new newly delimited class). Their parallel elevations came at the expense of the old arts and of natural history. Some of these were divorced from and subordinated to fine art, as lacking prestige and intellectual interest. But they were also undermined by analytical division of labour and mechanization—and by the supposition that the knowledge they contained was but the application of general principles understood by scientists.

But to write thus, of course, is to overwrite continuing traditions and essential tensions. The views of new scientist and artists were contested, and not just between the new twins, as it were. Naturalists and craftspeople, physicians, engineers, architects, radical art critics, social reformers and teachers all continued to stress knowledge from cases and from practice. They still do; and historians need better ways of saying so—by focusing on the complexities of working knowledges.

Conclusion

The history of working knowledges is not one of successions, but of tensions and contested cumulations. That is true within the traditions of natural sciences and technology, including the arts and Art. Few methods disappear from either kind of work, and technics always involve much more than is contained in formal analytical accounts. If that holds for natural sciences and technologies, it is probably true also for the knowledges and practices we call social or cultural.

We know, partly from Kuhn, that tensions between traditions are crucial motors for change. We know from countless historical case studies that the patterns of tensions change over time; that is the very stuff of our histories, and it is obvious that similar tensions occur in many different fields. I have tried to suggest an analytical and historical framework which will allow us to work across the whole range of western disciplines and practices and learn from comparisons. That move will not remove the need for detailed histories of particular cases, any more than they are removed from medicine by new forms of biomedical analysis. But it may make the work of historians less repetitive, more challenging and more collectively creative. The wider the span, the more likely these outcomes; which is why I have here extended the discussion to include social and cultural sciences, and Art as well as arts.

But as always, we must note that by widening the frame we bring new issues to the fore, and that these issues should inform the whole picture, not just the new bits. In suggesting that models of working knowledges developed for natural sciences and technologies may be useful for other fields, I want also to suggest that such long-standing goals of the humanities as moral reflexion and self-development should be part of the wider discussion—including our studies of natural sciences and technologies.

Kuhn's *Structure* was an academic and publishing success in part because it allowed social scientists to focus on their specific paradigms, thus winning scientificity at the expense of wider considerations. Many approaches to history now offer similar deals. But part of the greater challenge to historians, I would say, is to put back the wider considerations and to show them at work in and through our wider histories of knowledge.

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