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Martin J. S. Rudwick:

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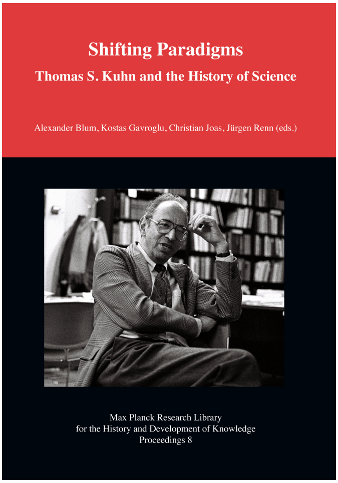

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Chapter 12
Constructive Controversy and the Growth of Knowledge
Martin J. S. Rudwick

I first met Tom Kuhn in 1962, shortly before his *Structure* was published, when I went to the International Congress of the History of Science, held that year at Cornell (and later in Philadelphia). I was then a scientist, and I was visiting the US primarily to do paleontological research at the US National Museum in Washington DC. But I already had strong historical interests. I therefore took the opportunity that the Congress offered, to meet and mix—for the first time—with historians of the sciences *en masse* (five years later I moved professionally into their field). On the first evening I happened to meet Kuhn and a few others, and all the talk was about his forthcoming book. I was immediately excited by what I heard, because Kuhn's ideas about the making of new scientific knowledge resonated with my own—albeit limited—first-hand experience of the practice of scientific research, far more than the abstract and idealized formulations of the philosophers whose work I had read. Kuhn's emphasis on the centrality of social interaction within groups of scientists, rather than the isolated individual minds presupposed by philosophical models, reminded me of Michael Polanyi's *Personal Knowledge* (1958)—which had earlier made a deep impression on me—with its insistence on the irreducibly personal, practical and often tacit character of the human processes of making knowledge, including scientific knowledge.

On the other hand, when later I read the published text of *Structure*, I was less persuaded by Kuhn's dichotomy between normal and revolutionary science, which seemed to be derived too narrowly from his own scientific training as a physicist and his early historical research on the Copernican "revolution." Practicing a very different kind of science, I felt the importance of taking into account the sheer diversity of the plural *sciences*, rather than treating physics as the ideal model for a monolithic "Science." (Many years later, when I was teaching in the Netherlands, one of my Dutch colleagues used to refer to the idea of a singular "Science" as the "*anglophone heresy*", in contrast to the mainstream Continental tradition of recognizing plural *Wissenschaften*, *wetenschappen*, *sciences*, *scienze* etc.) Reflecting on the then current state of my own science—paleozoology in the service of evolutionary biology—and on its earlier history, it seemed to me that "normal science" was often much more dynamic and innovative than Kuhn's model allowed, and "revolutionary science" often much less disruptive and not necessarily leading to incommensurabilities.

Kuhn's original and quite modest concept of paradigms—as concrete pieces of research that act as *exemplars* for fruitful further work—remains, I think, much more useful for understanding the making of scientific knowledge than his later concept of paradigms as alternatives that are radically incompatible and incommensurable. Fruitful exemplars have often emerged from a social process of controversy, entailing both conflict and collaboration, within a limited "*core-set*" of active researchers. When Harry Collins introduced this useful term, he emphasized the small size of core-sets, even within the Big Science of modern physics from which his examples were drawn. I suggested at the time that any core-set had, as its epistemic correlate, a similarly limited "*focal problem*" that had arisen within a wider scientific field. The successive and successful resolutions of specific focal problems by their respective core-sets might then help to describe how, in the history of the sciences, fields of relatively "normal" science have not always been static, constricted or eventually sterile, but often cumulatively fruitful and ultimately transformative, yet without any disruption by radically "revolutionary" change. I still think that this kind of "landscape" of scientific work fits the historical record of the sciences—at least the more observational and classificatory sciences, if not the highly experimental or rigorously mathematized ones—much better than the Kuhnian dichotomy allowed.

However, this claim can only be substantiated by assembling many relevant case studies of the dynamics of specific core-sets as they argue over and eventually resolve specific focal problems. This is where historical studies are indispensable, because problems in present-day sciences cannot show us how they may be resolved in the future. Yet current research by historians of the sciences is, in my opinion, giving too little attention to this issue. Any single case study may indeed be necessarily "*micro*" in character (and therefore currently unfashionable), yet cumulatively they ought to be contributing to issues that are as "*macro*" as any in our field.

By coincidence, around the time that I first met Kuhn I was given access to the previously unstudied manuscript papers of George Greenough, a prominent English geologist of the early nineteenth century and the first president of the Geological Society in London, which in turn was the world's first body of its kind. Among a mass of Greenough's otherwise unsorted papers I found a bundle of letters labeled by him as "Great Devonian Controversy." Reading these letters became my serendipitous entry point into an argument that had agitated the community of geologists in nineteenth-century Britain, and eventually much more widely, but which had almost been forgotten by their twentieth-century successors. I spent many years analyzing this highly controversial focal problem, trying to understand how it was eventually resolved—by a complex process of social dynamics within a quite small core-set of historical actors—into a consensus that has endured to the present day. When my book on *The Great Devonian Controversy* (Rudwick 1985) was published it got a lot of attention, some of it highly critical, from philosophers and sociologists as well as historians. But the detailed narrative that substantiated my analysis, and which was made possible by exceptionally rich primary sources, made it a very long book. Probably few readers read it from start to finish, and it has understandably faded from view. Yet, more than a quarter-century after it was published, I think it still has something to offer our current discussions of the making of scientific knowledge, if only as a case study that would be worth testing against others.

The Devonian controversy erupted in the 1830s among leading practitioners of the then quite new science of "*geology*", initially just in Britain but soon in the rest of Europe and eventually as far afield as Russia and North America. Superficially it was concerned simply with the classification and nomenclature of certain major *formations* or sets of strata in relation to others. But it was seen to challenge the dominant exemplar—embodied in the practice of *stratigraphy*—that formations could and should be identified, and hence *correlated* between one region and another, by finding the same fossils in them everywhere. They could then be arranged unambiguously in a unique structural order. Geologists agreed that this pile of rocks corresponded to the temporal order in which they had been deposited: they were a reliable record of the Earth's deep history, from which a reliable record of the history of life could be reconstructed (later, of course, this in turn became major evidence for evolutionary theories). The Devonian controversy arose when this well-established practice of stratigraphy was extended from the relatively easy cases of the younger formations to the more difficult cases of much older and more disturbed rocks. (The magnitude of the Earth's timescale was not at issue among nineteenth-century geologists, all of whom, whether religious or not, agreed that it was inconceivably vast although not yet quantifiable.)

The controversy was triggered when the highly respected English geologist Henry De la Beche reported finding fossils characteristic of the Coal formation, which was of supreme economic importance in the early Industrial Revolution, in the far older strata then recently named Cambrian. This anomaly was so radical and so unexpected—and its potential economic significance so important—that the factual reliability of the report was immediately questioned by other leading geologists, notably Charles Lyell and Roderick Murchison. There ensued some eight years of intense and sometimes acrimonious argument, recorded in field notebooks, in letters (often in turn recording private conversations), in reports of scientific meetings, and in published papers and books. A steadily expanding body of relevant evidence was deployed, with rhetorically effective argumentation on all sides, to support a growing array of diverse interpretations. The perceived balance of plausibility among these candidate solutions shifted repeatedly, as leading geologists changed their positions as a result of hearing persuasive new arguments or personally seeing persuasive new evidence in the field or in museums.

The primary sources—which in their rich density and completeness are possibly unmatched anywhere else in the history of the sciences—make it possible to track all these changes month by month, and at some points even day by day. It is possible to trace how, in real time, a period of bewilderingly diverse interpretations eventually converged into a consensus among the core-set (of about a dozen leading geologists), leaving only a couple of marginal figures holding out as dissidents and disagreeing with each other (see Fig. 12.2). This detailed historical evidence invalidates any claim that the resolution of the problem signaled the "triumph" of one side of the initial argument and the "defeat" of the other (as historical accounts of scientific controversies are usually framed, with the history often being written, of course, by the "winners"). Instead, it shows how the social process of controversy, with all sides deploying the changing empirical evidence to their best advantage, repeatedly forced the actors to modify their positions. Out of this social process a third and eventually successful alternative emerged, which *had not been foreseen by either side at the outset*: it incorporated elements derived from *both* the initial rivals, yet it was no mere compromise (see Fig 12.1). In other words, the consensual solution to the focal problem resulted in the production of genuinely *new* knowledge, which has been incorporated so successfully into the practice of the science that the Devonian controversy has been almost completely forgotten by modern geologists (its consensual product is the defining of a distinctive "Devonian period" in the history of the Earth, during which, for example, both plant and animal life made their first significant appearances on land). Was this new knowledge a social construction or a discovery about the real world? It was, of course, *both*.

During the half-century since Kuhn's *Structure* was published, the often acrimonious arguments about scientific knowledge *as social construction* could have been avoided, or at least ameliorated, if practitioners of "science studies" (historians, philosophers and sociologists) had considered other epistemic projects that are, like the natural sciences, *both* wholly human and social constructions *and* truth-bearing representations of natural realities. *Maps* and *mapping* were cited occasionally in this context, but I think they still have much to teach us. Anyone who uses maps extensively must be well aware of the sheer diversity of these representations of one-and-the-same reality, which adopt equally diverse sets of socially understood conventions. The world-famous map of London's "Underground" or metro system, for example, is utterly unlike a street map of the same city, or maps that show the major roads, aviation routes, weather conditions or underlying geology of the same region. Yet all these maps may be judged to be accurate (or at least corrigible) representations, which can be used equally successfully for their diverse respective purposes. Add the historical dimension, and maps may also be rightly judged to have been progressively *more* accurate and reliable representations; or, if they differ radically from their counterparts in other historical periods, it may be because their intended purposes were quite different (for example, the early *mappae mundi* centered on Jerusalem, compared to modern world maps). The analogy with the historical construction of scientific explanations should be obvious.

The past half-century has seen a welcome increase in historians' awareness of the value of *visual* sources of all kinds (including maps), compared with their earlier almost exclusive use of textual sources. I was acutely conscious of this when I moved in mid-career from a strongly visual science into historical teaching and research: visual images and diagrams were generally regarded by my new colleagues as optional decoration, not—as in my science—as an indispensable complement to any verbal exposition. A paper I published in *History of Science* in 1976, arguing for the importance of visual sources in historical work, was almost ignored by historians for several years (though it was welcomed by scientists with historical interests), before being cited retrospectively—to my bemusement—as a "pioneer" example of what has since become an active and fashionable field of historical research. Yet in contrast to this new appreciation of visual imagery in primary sources, historians still rarely use visual imagery of their own devising, to explicate their historical interpretations. Back in 1985, reviews of my Devonian book were sharply divided on this issue: the scientists found its interpretative diagrams helpful and illuminating, but most of the historians, sociologists and philosophers said they found them incomprehensible or even repellent. Nothing much has changed since that time: I think "science studies" scholars still deprive themselves of mental tools that many kinds of scientist find valuable or even indispensable (see Figs. 12.1, 12.2).

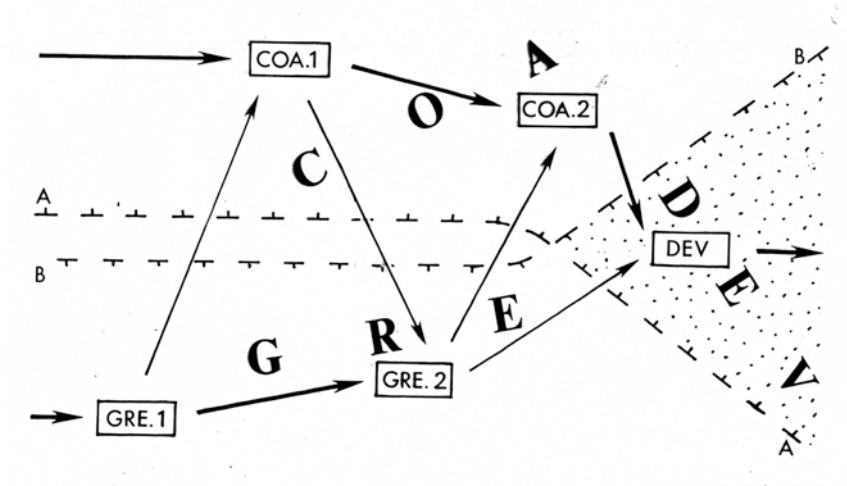

Figure 12.1: A schematic summary of the structure of the Devonian controversy. Historical time (in the 1830s and 1840s) flows from left to right. The vertical dimension represents the relative *theoretical distance* between five major interpretations of the developing body of empirical evidence. They are situated in three *interpretative domains*, classed as *GRE*, *COA* and *DEV*. Thick arrows are lines of *interpretative development*; thin arrows represent *interpretative pressure* from one interpretation on another. Thus when *COA. 1* challenged the pre-existing *GRE.1* it was maximally distant from it; but each later conceded, under pressure from the other, modification into *GRE.2* and *COA.2*, which reduced the distance between them. Later still, under further pressure from *GRE.2*, *COA.2* transmuted dramatically into *DEV*, a new class of interpretation unanticipated on either of the previous alternatives. The *GRE* and *COA* domains had been separated by non-negotiable and incompatible claims that formed the *interpretative boundaries* A and B; but the new interpretation *DEV* resolved their incompatibility ("the battle lines filtered silently through each other, until they faced outward, leaving at their rear a domain defended by them *both*"). The empirical success of *DEV* then expanded rapidly, as represented by the expansion of the stippled *DEV* domain and the consequent marginalizing of the earlier rival domains. This diagram (reproduced from (Rudwick 1985), Fig. 15.2) was an attempt to conceive the basic argumentative structure of the controversy, stripped—temporarily—of all the contingencies of the historical actors who proposed these interpretations.

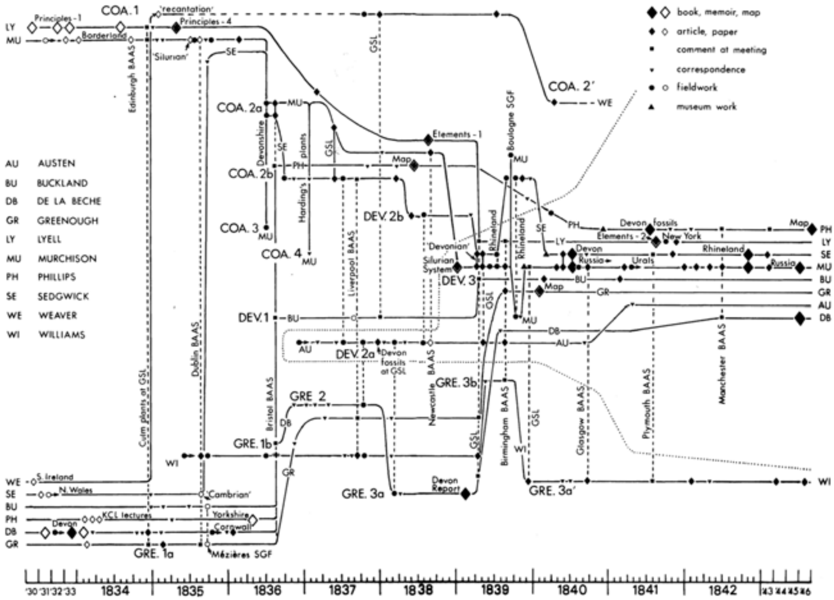

Figure 12.2: A schematic summary of the Devonian controversy, showing the theoretical trajectories of ten major historical actors, plotted against a quantified timescale of months and years from 1834 through 1842 (and, on a condensed scale, for some earlier and later years), marking the main points of documentary evidence for each trajectory and some of the scientific meetings at which the Devonian problem was discussed. The vertical dimension represents the interpretative distance separating the trajectories. They traverse many alternative interpretations within the main classes of *GRE*, *COA* and *DEV*; some variants were held only briefly, others were more stable through time. The diagram illustrates how an initial near-consensus around *GRE.1a*, with a dissident minority arguing for *COA. 1*, was succeeded by a middle period of great confusion, shifting commitments and rapid change, until the proposal of *DEV.3* (which had tentative forerunners from *DEV.1* onwards) led rapidly to a consensus, leaving only two dissidents out on the margins. This diagram (reproduced from (Rudwick 1985), Fig. 15.5) was an attempt to depict the dynamics of the controversy in terms of its core-set of leading historical actors.

This leads me to a more general reflection on our relationship to scientists. Scientists' historical views are not always limited to the level of "Let me tell you an anecdote." In some sciences—my own field of the Earth sciences is certainly one – there are many scientists doing serious historical work in an institutional structure parallel to ours, but largely separate from it. Back in 1994 the Geological Society of America sponsored a successful conference in San Diego, which was designed to lessen this divide by bringing together geologists interested in history and historians interested in geology. There was much talk of these groups as being, respectively, "insiders' and "outsiders": the geologists considered that they had "inside" knowledge of the science, whereas the historians could only observe it from the "outside." But I pointed out that these labels could equally well be reversed: it was the historians who knew the "feel" of past periods from the inside, thanks to the virtual time-travel made possible by lengthy immersion in the historical sources and their wider context, whereas the scientists often lacked that inside knowledge. (I felt able to make this point without offending either group, because I was one of the handful of participants who could claim to belong to both!) How then should we historians of the sciences interact with scientists? Some of us will continue to use scientists as valuable primary sources for the recent history of their sciences. But should we not also attend to their evaluation, as "insider" participants, of the dynamics of their own research, and all the other issues that engage us as "outsider" analysts? In my opinion the current trend among historians of the sciences, to seek ever-closer relations with "mainstream" historians, is not an unmixed benefit, if it leads us to neglect our links with working scientists.

On the positive side of this relationship, much excellent historical work, since *Structure* was published, has explored the role of new instrumentation and other material "tools" that have interacted with "ideas" in the making of scientific knowledge. However, to complement this, I think more attention needs to be given to the role of material *objects* such as natural specimens, and not only those such as *Drosophila* that have been used as materials for experiments. The conceptual dominance of physics in science studies of all kinds (including historical studies) has led, in my opinion, to an over-emphasis on what can be confirmed by replication in experiments. This needs to be balanced by recognizing the powerful role that natural objects—even *unique* objects—have played in sciences in which experimentation is subordinate or even negligible. For example, the chance discovery (by coincidence, shortly after Darwin's *Origin* was published) of a fossil *Archaeopteryx*, apparently intermediate in its anatomy between reptiles and birds, was used at the time as persuasive evidence for macroevolution. But this initially unique specimen (from Solnhofen in Bavaria) would have been immensely important in strengthening the case for an evolutionary history of life, even if it had never been "replicated"—as in fact it was later—by the discovery of further specimens of the same strange extinct organism.

Finally, the "experimental turn" in the historiography of the sciences, since *Structure* was published, has yielded valuable insights into the problematic nature of experimentation, not least as revealed by attempts to "*re-stage*" classic experiments. However, such studies of what has been done in laboratories need to be complemented by studies of the scientific practices located in two other major—but relatively neglected—sites of scientific knowledge-making, namely the *field* and the *museum*. In my own work on the history of the Earth sciences, I have found it immensely valuable to "*re-tread*" historic fieldwork, visiting classic sites and sights (specific quarries, mountains, volcanoes, etc.): not to discover what was "really" the case—as presentist-minded scientists might claim to be doing—but to try to see the historical actors' evidence "*through their eyes*" and thereby understand their reasoning and argumentation. In the same way I have studied in museums the *particular* specimens (of minerals, rocks, fossils, etc.) that historical actors described and argued about, again to try to understand how they handled the specific evidence that was available to them. In both these kinds of historical study, natural objects (large and small) are treated as primary sources. I think that much more work could be done along such lines, for all the natural-history sciences, provided that the "seeing" is as analytical (and not merely celebratory) as in our studies of conventional textual sources.

I have used my own experience of working in the history of the Earth sciences, in the half-century since Tom Kuhn's *Structure* was published (and since I first met him), as a small example of the immensely fruitful influence that his work has had on our human understanding of the making of new and reliable natural knowledge. I think it will be no surprise if that influence continues in some form through the next half-century.

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