An Episode in Geology

The Great Devonian Controversy. The Shaping of Scientific Knowledge Among Gentlemanly Specialists. MARTIN J. S. RUDWICK. University of Chicago Press, Chicago, 1985. xxxiv, 494 pp., illus. $39.95. Science and Its Conceptual Foundations.

This account of a geological debate of the late 1830's and early '40's is surely one of the most important studies in the history of science of recent years, and arguably the best work to date in the history of geology. Its merits are numerous: a lucid account based on remarkably detailed scrutiny of a massive quantity of printed and manuscript sources; an admirable combination of historical narrative and theoretical analysis; a most valuable technique of historical synthesis through the use of diagrams and figures; and a first-rate editorial and production job on the part of the publisher.

The broad theme is the empirical and theoretical changes that led to the establishment of the Devonian System as an accepted subdivision of the stratigraphical column and the social factors that were involved in this process. The chief protagonists were on the one hand Henry De la Beche, first director of the Geological Survey of Great Britain, and on the other the "amateur" geologists Roderick Murchison and Adam Sedgwick, whose main institutional base was (for this debate) the Geological Society of London. The British Association for the Advancement of Science also provided an important forum. The initial locus of attention and contention was the "culm" rocks of Devon, which De la Beche sought to understand by lithological studies and through the analysis of geological structures. Sedgwick, and more particularly Murchison, following William Smith, placed greater reliance on paleontological evidence. From Devon, the debate was extended to encompass rocks in South Wales, Ireland, Belgium, France, and Germany, and eventually as far afield as Russia and America. In the course of the debate, the old Wernerian notion of universal formations of different lithologies had to be relinquished; but the original Smith concept of specific fossil types being unambiguously characteristic of rocks of particular ages was also found wanting, and it became more evident that there could be rocks of significantly different types, with somewhat different fossils, laid down simultaneously in different parts of the world. The whole controversy blew up because of the discovery in Devon of fossil plants of coal-measure appearance in small coal seams in rocks that otherwise resembled the ancient graywackes or transition rocks of Werner. Consequently, the adequacy of plant remains as stratigraphical indicators was one of the chief subjects of dispute.

For De la Beche, a successful outcome of the debate was of paramount importance, for on it depended his situation at the Survey and, on a wider front, the success of the rising profession of geology. For Sedgwick and Murchison, on the other hand, there were committed views as to the mode of geological science that was appropriate—gentlemanly and scholarly, as opposed to bureaucratic and professionalized—and the use of fossils rather than rock types as the basis of stratigraphy. Also, they had their personal theoretical barrows to push, in the form of the Cambrian and Silurian systems respectively, so that for each of them a major concern was the standing, in the light of the outcome of the Devonian controversy, of the system that he had proposed. In the case of Murchison, there seems to have been an overdeveloped personal ego, perhaps a product of his own powerful social situation, that was determined to force all geologists to acquiesce to his theoretical views.

Besides the three main contestants, there were numerous other figures: major geologists who were less intimately concerned with this particular controversy but whose opinion provided judgment on the whole question, and a host of minor figures who were of insufficient stature to determine the final outcome but who often provided important empirical evidence.

On the evidence that Rudwick adduces, both sides of the controversy saw the whole episode in military terms—as a contest in which "territory" might be captured and in which participants might, when pressured, change sides. Each side sought to make the other "submit" to its views. Not surprisingly, when a compromise was eventually achieved both sides had to "give ground" somewhat. De la Beche's field interpretations were at least partly vindicated, but the Wernerian lithological approach to stratigraphy could not be sustained. On the other hand, a simplistic use of fossils as stratigraphical indicators was insufficient; and one had to recog-

"A governmental geologist at work in the field in gentlemanly dress." Geologists "working on foot, wearing outdoor clothing, and being frequently soaked to the skin, . . . could find that their gentlemanly status went unrecognized by their social equals, particularly since . . . they appeared to be engaged in that most menial of occupations, stonebreaking. . . . Yet their relative disguise, and their evident readiness to share the hardships of outdoor life, seem to have enabled them to elicit valuable local information from the lower classes much more easily. . . . Conversely, the wearing of formal attire while 'stonebreaking,' which later became de rigueur among those employed on the Geological Survey in Britain, created a social discrepancy that must have puzzled the idle bystander." [Reproduced from the first *Memoir* of the British Geological Survey (1846) in *The Great Devonian Controversy*]

nize that formations were not worldwide. Eventually, the conception of the Devonian System, actually proposed by William Lonsdale, was seen as a satisfactory theoretical compromise for the stratigraphical situation of the rocks that were the subject of contention. Rudwick brilliantly reduces the various theoretical shifts to a few readily comprehensible diagrams.

The theoretical conclusion, for the philosophically inclined historian, is that scientific knowledge is a social construct, generated by real people (of differing "credibilities") working at specific times and places, with specific social interests shaping their work. Also, the meanings of theoretical terms are found to be "malleable." They gradually undergo change during the course of debate, as each side seeks to press its views; but there is no question of "incommensurability" of theoretical terms from one stage of the debate to the next. For Rudwick does not see this episode in the history of science as "revolutionary" in the Kuhnian sense, so semantic incommensurability need not be expected. Even so, the "great Devonian controversy" was obviously one in which very significant theoretical change occurred.

Actually, Rudwick's model for the processes of scientific change derives chiefly from French analysts of science such as Bourdieu and Latour. Rudwick sees the arena of scientific debate as an "agonistic field" (that is, a field of contest). So in the Devonian controversy it was not empirical data as such that eventually settled the issue. Observations had to be written down or sketched in the field and then brought to the bar of the scientific community—in this case chiefly the leading members of the Geological Society of London—for appraisal and either acceptance or rejection. Geologists had to argue their cases as forcefully as possible, and this involved a deal of toing and froing behind the scenes, to which the voluminous correspondences examined by Rudwick bear ample testimony. The overall process was analogous to the procedures in a law court, as well as being similar to the events of a military campaign. The final court of appeal would, I take it, be the practical success or otherwise of the theoretical schemes proposed.

However, it should be emphasized that Rudwick's strong interest in the social dimension of science does not lead him to embrace a thoroughgoing epistemological relativism, such as is espoused by some sociologists of knowledge. He sees scientific knowledge as "shaped" or "forged" in the heat of scientific debate, but he emphasizes the accumulation of "constraining" (but not "determining") empirical evidence, which leads to the construction of a theoretical picture that is related to the real world. His view is illuminated by the analogy of an emerging picture made up of a pattern of dots, which are added successively as empirical evidence is collected. Initially, false conjectures may be made about the picture, but eventually, with sufficient data ("dots"), consensus will be achieved as to what the picture is. Likewise, interpretative work in science is increasingly constrained by the evidence, but is at no time determined by it. Rival attempts at construing the picture may be compared to the work in the "agonistic field" of the scientific community, in which competing theoretical interpretations are proposed, attacked, and defended.

In all this, however, I think there may be a problem for Rudwick to address. For if, as authors such as N. R. Hanson would have us believe, all observations are themselves theory-laden, then there is an extra epistemological "layer" at which the social determination of knowledge may operate and which the picture analogy does not capture. Thus Rudwick's qualified realism may require further qualification. But this consideration does not seriously undermine the totality of his achievement as historian of the Devonian controversy.

As to the historical narrative, my only objection pertains to the treatment of De la Beche's side of the debate. By Rudwick's account, De la Beche seems generally to have been on the receiving end of the blasts issuing from the muzzles of Sedgwick and (particularly) Murchison (who emerges for me as a singularly unlikable character). Yet De la Beche apparently managed to float to the surface after every encounter. The reader may be left unsure how he contrived to do so.

One can hardly doubt that Rudwick has written the definitive account of the Devonian controversy. But in a sense that is not his main achievement. The important point, as I see it, is that he has provided a splendid model for research in the history of science. In the process, he offers significant support to those who regard scientific knowledge as a social construct, not objective in the full sense of the word. His construal of history will, therefore, undoubtedly please those who admire the work of writers such as Latour. They, however, may feel that Rudwick has not gone quite far enough in his analysis. By contrast, some readers of *Science* may take the view that he has already gone too far in his acknowledgment of the social dimension in the construction of scientific knowledge. It is difficult to satisfy all opinions, but there will be few who feel dissatisfied with the quality of Rudwick's archival researches and his skill in bringing the past vividly before us.

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Instabilities in Plasmas

Unstable Current Systems and Plasma Instabilities in Astrophysics. MUKUL R. KUNDU and GORDON D. HOLMAN, Eds. Reidel, Boston, 1984 (distributor, Kluwer, Hingham, Mass.). xxii, 566 pp., illus. Paper, $29.50. International Astronomical Union Symposium no. 107. From a symposium, College Park, Md., Aug. 1983.

In the past 10 years, there has been a growing recognition that problems in many diverse areas of physics are linked by a need to understand unstable current systems in magnetized plasmas. Large-scale disruptions occur in laboratory plasmas, solar system plasmas, and astrophysical plasmas. These are not only of scientific interest, they are of great practical importance for the thermonuclear fusion program and for global communications, which are seriously disrupted by geomagnetic storms. The present volume contains the proceedings of a meeting that attempted to summarize the current understanding of instabilities in magnetized plasmas and to stimulate interactions among various plasma physics communities. The volume contains a total of 63 technical papers, which cover an extremely wide range of topics, and any plasma physicist should be able to find at least a few papers relevant to his or her own interests.

A strong unifying theme of the volume is the process of magnetic field reconnection, which is the merging of magnetized plasmas containing magnetic fields in different directions. Two major laboratory experiments on magnetic reconnection are reviewed, as are the classic reconnection models of Parker, Sweet, and Petschek. In addition, examples of resistive magnetohydrodynamic simulations and particle simulations are presented, and numerous plasma kinetic instabilities that may play a role in magnetic reconnection are reviewed. The classic reconnection models still provide the