

Chalk to cheese

Progress, power, cooperation and topography: stages towards understanding how science happened

The history of science was at first closely tied to the teaching of the sciences. Its dominant narratives told of advances towards our present enlightened state, mostly focusing on scientific discovery at the hands of great men, with a mainly Marxist minority prioritizing economic, technological and social factors. In their celebrations of the advancement of science, such histories were in line with scientists' own perceptions of the past – unsurprisingly, given that most of their authors had been trained as scientists.

Since then, the discipline has undergone a remarkable series of transformations. In the late 1960s and 70s, the triumphal tales of scientific progress were challenged. Thomas Kuhn, Michel Foucault and others highlighted radical historical discontinuities in scientific theory and practice, and a new breed of “social constructivists” analysed scientific consensus as the products not of disinterested inquiry, but of pursuit of social interests. Appeals to reason and method no longer figured as routes to truth, but as *post facto* propaganda, means of confirming and celebrating scientific doctrines secured by less palatable means. In the 1980s, there appeared another approach, at once visionary and bizarre. According to the “actor-network” theory, historians should approach the issue of scientific consensus formation without prejudice in favour of rational or social factors. They should rather investigate the full range of ways in which “centres of calculation” – laboratories, observatories, museums – have recruited and delegated agents so as to gather information, to authenticate and analyse it, and to distribute their findings in stable form worldwide. Bruno Latour, the leading exponent, courted controversy by insisting not only (reasonably enough) on the central roles of technologies in these processes, but also (less so) on the need to credit machines and instruments with agency and motivation. The 1980s and 90s saw further moves away from concentration on great geniuses and their canonical texts and discoveries. Instead, attempts were made to “recuperate” the persons and things marginalized or obliterated in heroic tales of progress – women, technicians, native informants, routine instruments, buildings, and so forth. In accord with this “decentring” we see ever-increasing attention to local and indigenous styles of inquiry, culminating in the current predilection for “geographies of science”. The focus here is on the conditioning of the practices and contents of the sciences by their national, institutional and geographical locations, and on the ways in which scientific knowledge has been distributed and mediated between sites of inquiry. Like the earlier fashions – social constructivism and actor network theory – this one comes complete with an often infuriating jargon: “topography of

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scientific content”, “biographies of place”, “mediated translocational co-constitution of knowledges”, for example.

Underlying this procession of fads and fashions there has been an accumulation of valu-

able innovations, much in evidence in the volumes under review. Take the issue of anachronism. Up to the 70s, histories of science were, by and large, anachronistic on several scores: in their projection on to past activities of disciplinary categories – for example, “biology”, “geology”, “science” itself – unthinkable at the past times in question; in their focus on discoveries and theories that could be read as anticipating later scientific orthodoxies, rather than on those deemed significant in their own periods; in their uncritical assumption that such crucial terms as “observation” and “experiment” meant in the past what they mean for scientists today; in their attempts to impose on past works modern ideals of consistency and coherence, rather than taking account of past genres and conventions. Historians of science are now routinely far more sensitive on these scores. For example, the contributions to *Histories of Scientific Observation* (hereafter *Observation*) scrutinize the shifting connotations of “observation” and “experiment”; and throughout both *Observation* and *Geographies of Nineteenth-Century*

Science (hereafter *Geographies*) there is careful consideration of the conventions of the often unfamiliar genres of past works.

A second major turn has been from obsession with innovative high theory to close study of what Thomas Kuhn called “normal science”, the routines of everyday pursuit and communication of the sciences. Throughout *Observation* there is meticulous exploration of techniques of observation in relation to practices of collecting, preserving, recording, archiving, tabulating, excerpting and analysing; and the second part of *Geographies*, “Practices and Performances”, is devoted to the minutiae of public presentation, display and defence of science and technology.

Where once objective and dispassionate inquiry was widely taken for granted as a timeless defining feature of genuine science, there has been a recent upsurge of interest, well represented in these volumes, in the diverse forms of emotional engagement in the sciences. In *Geographies*, Diarmid Finnegan looks at the ways in which “scientific orators” at the Edinburgh Philosophical Institution sought to engage the emotions of their hearers; and Graeme Gooday tells of exhibitions and theatrical performances designed to assuage public fear of electricity. In *Observation*, Otniel Dror gives a fascinating account of the nineteenth-century shift from diagnosis of emotions through the responses of skilled observers to their estimation through “objective” measures of the “visceral state” of the subject; Elizabeth Lunbeck recounts the recruitment of empathy as a “servant of science” in the field of psychoanalysis; and Anne Secord movingly recreates the ways in which vigilance, wonder at the sublime ocean, and awe of the Creator were combined in the study of seaweeds by Dawson Turner, an Evangelical botanist.

Among the first generation of post-war historians of science, there was a standoff between those who took economic and social changes to be the drivers of progress and those of the mainstream who supposed the primary factors to lie in the realm of ideas. On this score matters have changed beyond all recognition. The “hybrid” nature of scientific activities, the mutual entanglement of the social and intellectual, is now generally recognized. To take a couple of examples from *Geographies*, in Simon Naylor’s account of mapping in nineteenth-century Cornwall, cartographic, agricultural and mining enterprises cannot be separated into social context and scientific content, and in David Livingstone’s contribution, political and anthropological pursuits are likewise inextricable. Context now appears in the form of the “sites”, “settings” and “venues” that condition and sustain all aspects of scientific inquiry. *Geographies* is devoted to such locations. For example, Samuel J. M. M. Alberti demonstrates how London museums



12.12.11 Edinburgh

It is small wonder that pandas are so bad at breeding. International diplomacy must take up all their time. The first sight in Edinburgh Zoo of Yang Guang (possibly left) and his partner (or possibly not) Tian Tian has encouraged high hopes of economic cooperation in Scottish hearts. Trade in bamboo shoots is booming. And while nationalist ambitions to abandon the sterling pound of London and join the “mighty Euro”

have been grounded by grim reality this week, the arrival from China of Sweetie (possibly right) and Sunshine has whetted appetites for bigger deals. Some are old enough to look back forty years to when Edward Heath was trading pandas with Beijing and the European economic dream was bright in all but the blackest eyes. Perhaps this time there will at least be the patter of tiny paws to show for everyone’s investment.

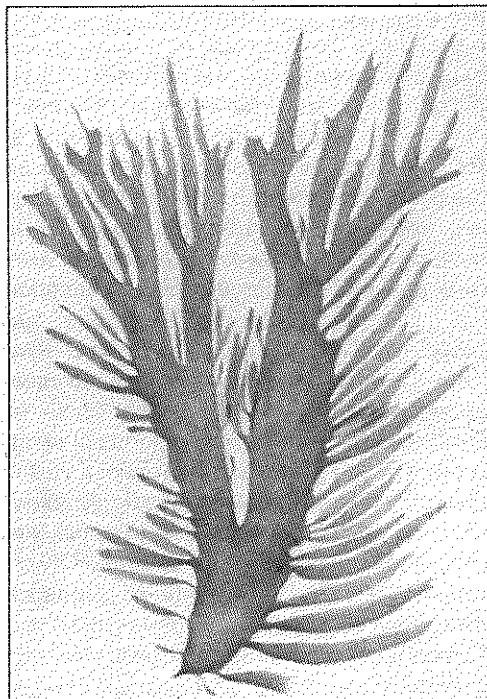
became sites of acknowledged expertise and authority; Donald Opitz shows how a country house and its grounds became a privileged location for research in Mendelian genetics; Sujit Sivasundaram contrasts the highlands and lowlands of Sri Lanka as locations of indigenous knowledge, in part appropriated and in part displaced by colonial science; and Lawrence Dritsas narrates the long-running struggle for credibility and control between the "critical geography" of the British homeland and the field geography of explorers.

Then there is the shift away from concentration on intentions and works of great discoverers. The past twenty years have seen growing emphasis on the full range of products of the sciences and on the varied ways in which they have been communicated, received, appropriated, adapted and applied. In *Observation*, Charlotte Bigg looks at the reception and interpretation of Jean Perrin's images of Brownian motion; and Anne Secord, this time writing in *Geographies*, provides wonderfully detailed accounts of ways in which botanical books containing dried specimens (or designed to have specimens or notes added) functioned as incentives and guides for exploration, observation and collecting. This concern with communication, reception and appropriation has generated a recent explosion of works on the exchange of knowledge both within and between cultures. Back in the 1960s and 70s, scientific knowledge was generally seen as having been "diffused" or "disseminated", following the Scientific Revolution of the sixteenth and seventeenth centuries, from Europe to the scientifically benighted regions of the earth. Since then there has been ever more emphasis on the roles of cross-cultural interaction in the generation of scientific knowledge, on the variety of the agents involved in its transmission, and on the countless ways in which knowledge has been adapted, appropriated and customized in transit. In *Geographies*, Sally Kohlstedt and Lawrence Dritsas deal with interactions between metropolitan and peripheral expertise in the generation and transmission of knowledge; in *Observation*, Lorraine Daston describes the ways in which, in the early modern period, observations from remote sites were accumulated and authenticated, and Daniela Bleichmar tells how in eighteenth-century Madrid the Director of the Royal Botanical Garden organized expeditions to the Americas, while in Bogotá a leading colonial naturalist trained a host of native artists to depict plants. Both Daston and Bleichmar are at pains to emphasize the range of media (notebooks, instruction manuals, illustrations, prepared specimens) and mediators (travellers, missionaries, diplomats, artists) involved in the worldwide exchange of natural historical and natural philosophical knowledge.

Last but not least there is a change whose virtue no serious historian would question. The past fifty years have seen a remarkable expansion in the range of sources used by historians of science and the development of ever more critical ways of examining those sources. Where once the narratives were generally built on close, if often anachronistic, readings of a relatively small body of canonical texts, with social and political contexts being filled in from secondary sources, it is now standard practice for historians of science to draw on primary sources relat-

ing to all aspects of the production and reception of works, and to conduct their own researches into the economic, social and political aspects of scientific activity.

Why, over the past half-century or so, has the history of science been so conflicted: so susceptible to fads and fashions, but at the same time making such huge leaps forward in its handling of sources and in its historical placement of the sciences? A properly researched and argued response to this question would be a major undertaking, but here are some suggestions.



Alga (*Ulva lingulata*); from *Drawings of Submerged Algae* by Mary Dawson Turner (1800)

History of science is a young discipline, and the great majority of its practitioners have been not historians, but scientists or ex-scientists. However, it was by no means universally respected by scientists; I well remember as an undergraduate in the mid-60s being told by a Cambridge natural sciences don not to waste time on the lectures in history and philosophy of science, time that would be better spent in the laboratory or on the playing field. With the rise of social constructivism many scientists became actively suspicious of the field, and the antagonism came to a head in the mid-90s in the so-called Science Wars, in which social constructivists, relativists and postmodernists came under fire for ill-informed misrepresentation of the sciences. As for mainstream historians, their lack of interest is evident from the virtual silence about the sciences in such standard historiographical works as E. H. Carr's *What is History?* (1961), G. R. Elton's *The Practice of History* (1967), A. Marwick's *The Nature of History* (1970), J. Tosh's *The Pursuit of History* (1984), and R. J. Evans's *In Defence of History* (1997).

It is, I conjecture, insecurity that has rendered the history of science so liable to inappropriate deployment of models and terminologies emanating from the "soft sciences". From the social and political sciences were derived the vaguely Marxist accounts of scientific knowledge as social construction, such accounts being often spiced up with the vocabulary of relativist philosophy of science. Actor network theory drew on post-colonial anthropological studies of cultural

exchange and on the brand of participant-observer sociology known as "ethnomethodology". A current source of unwanted jargon is cultural geography; another is an ill-considered and condescending form of political correctness according to which, by dumbing down "knowledge" into "local knowledges", justice can supposedly be done to indigenous expertise appropriated and/or marginalized by "Western" science and rationality.

Whence, then, the simultaneous accumulation of virtues? This surely represents the belated involvement of history of science with other more securely established branches of history. In the case of avoidance of anachronism this is, indeed, widely recognized by historians of science. Anachronism had been effectively challenged in the field of political history by Herbert Butterfield in *The Whig Interpretation of History* (1931); and from the 70s, triumphal histories of scientific progress were widely condemned as "Whiggish". In the same period, historians of science took note of the "Cambridge school" of political and intellectual historians (notably J. G. A. Pocock, John Dunn and Quentin Skinner), who advocated respect for past "actors' categories" and avoidance of all descriptions of past intentions and actions in terms the agents themselves could not possibly have comprehended.

The turn, by historians of science from obsession with the actions and meanings of great authors to concern with response, transmission and appropriation was long preceded by the shift within literary history towards close engagement with the reception of works, a shift in which the writings of Hans-Robert Jauss and Wolfgang Iser played major roles. From literary history there also derived a new attention to the genres, styles and rhetorical devices of scientific texts, a notable early contribution being Brian Vickers's *Francis Bacon and Renaissance Prose* (1968). As for "decentering", the move from the privileging of great innovators and metropolitan elites to the reconstruction of local and popular attitudes, inspiration came from many sources: notably the "history from below" developed in the 50s and 60s by Asa Briggs, Eric Hobsbawm, E. P. Thompson, Peter Laslett and others, and the *Annales* school and its offshoot, the *histoire des mentalités* represented by books such as Carlo Ginzburg's *The Cheese and the Worms* (1976) and Alain Corbin's *The Foul and the Fragrant* (1982). An invaluable lesson learnt from *histoire des mentalités* is the capacity of mistakes, misunderstandings and mishaps to reveal past attitudes and assumptions that remained tacit under more clement circumstances. (A nice example is in Andrew Mendelsohn's piece in *Observation*, where he analyses failures by eighteenth-century savants to extract generalizations from inventories of observations). In the "new historicism" and "new cultural history" of the 80s and 90s, thorough historical contextualization was combined with respect for the multiplicity of past viewpoints, and scrupulous avoidance of anything that might seem to pass judgement by present-day standards. As for the recognition of "hybridity" in the history of science, the mainstream historical precedents are too many to bear enumeration: throughout the twentieth century in such diverse fields as political history, history of art, social history and historical anthropology

we see growing recognition of the inseparability of the economic, social, political and cultural domains. Finally, critical engagement with a wide range of primary sources has long lain at the very heart of the identity of history as a discipline.

As it becomes more critical in its engagements with the soft sciences and philosophy, and merges at last into mainstream history, the history of science is flourishing as never before. It does, however, face major problems. With the move away from triumphal histories, there has been a marked tendency towards ever more tightly focused microhistorical studies of the practices of the sciences at particular times and places. As several historians of science have recently insisted, re-engagement with large historical questions is surely a worthy goal; but it is far from clear what form such "Big Histories" of the sciences should take. Then there is the classic "problem of universals", raised in acute form by recent studies of past roles in the sciences of emotions and moral stances. How far can we suppose that these can be identified across times and cultures? Can we filter out a core of universal "feelings", as opposed to culturally laden emotions and commitments? If not, how can historians of science hope to get to grips with past "emotional economies" (to use a currently fashionable expression)?

Tied in with the problem of universals is the problem of integrating interpretive with explanatory history of science. As is very much in evidence in *Observation* and *Geographies*, current historians of science tend to adopt what anthropologists call the "emic" approach, devoted to conveying past beliefs and actions in the agents' own terms. But explanation of developments in the sciences is surely a legitimate aim. Indeed, freed of some of the philosophical and ideological preconceptions that blighted mid-twentieth-century histories of the sciences, the time has surely come to return to the daunting issue of the causes of major changes in the sciences, including those that constituted, by our lights, progress. Now such explanations surely demand the "etic" approach, the application to past agents of economic, social, psychological and anthropological categories and theories far removed from those available to the agents themselves. How can the emic and etic approaches be combined so as to deliver histories of the sciences that are at once historically sensitive in their reconstructions of past actions and beliefs, and effective in providing at least partial explanations of them? If etic explanatory models are used, what is their status? Consider, for example, the frameworks mentioned above for analysis of the geographical movement of scientific knowledge: the diffusion model, the actor network model, and the currently fashionable mediated circulation model. Are we to think of these as mutually exclusive theories, analogous to rival theories in the natural sciences? Or should they be conceived (as seems to me much more plausible) as alternative "ideal types" to which patterns of knowledge distribution may approximate more or less closely at different times and places?

These are formidable problems. But historians of science should welcome them with open arms. Since the 1960s the subject has been beset by problems of its own making. Now at last it is facing up to real problems. It has come of age.