Pergamon

Stud. Hist. Phil. Mod. Phys., Vol. 29, No. 3, 287–294, 1998

Printed in Great Britain
1355-2198/98 $19.00 + 0.00

Introduction: Cultures of Theory

Peter Galison and Andrew Warwick

This is the first in what the editors intend to be an ongoing series of occasional special issues of *Studies in History and Philosophy of Modern Physics* devoted to short collections of papers on specific themes. On this occasion the issue has been put together by two of the journal's editors, Peter Galison and Andrew Warwick, but it is hoped that future special issues will be led by one or more 'guest editors' with specific expertise on the theme in question. The editors would be pleased to receive suggestions for future special issues, especially those combining historical and philosophical themes.

An inaugural special issue of this journal entitled 'Cultures of Theory' may require some explanation. Our readers will readily identify the 'theory' to which we refer as mathematical-physical theory, but might be more puzzled by the reference to its 'cultures'. Of what relevance is an interpretive concept taken from the discipline of anthropology and the broader field of history to historians and philosophers of physics? This is a difficult and complex question to which we cannot do justice in this short introduction, but the following remarks should help to elucidate some of the ways in which the concept is employed by the authors in this collection.

Over the past two decades, a number of historians of science have adopted techniques from anthropology in an attempt to develop more local and more historically contextualised accounts of scientific work. As the late Thomas Kuhn suggested almost fifteen years ago, historians of science should be prepared to approach past knowledge without assuming that it is an obvious stepping stone to the present, in his words they must begin the inquiry 'as the anthropologist approaches an alien culture. They must, that is, be prepared at the start to find that the natives speak a different language and map experience into different categories from those they themselves bring from home' (Kuhn, 1984, p. 246). References by Kuhn and other historians of science to the methods of anthropology are to a large extent, and necessarily, analogical. Historians, unlike

PII: S1355-2198(98)00015-X

ethnographers, cannot literally live and work among the people they study, but they can draw upon the ethnographer's experience of encountering and interpreting contemporary cultures in their own attempts to make sense of the foreignness of the past.

Discarded theories frequently appear incomprehensible, self-contradictory or irrational when measured against present scientific knowledge, but can generally be made to regain their historical integrity when understood in the scientific, intellectual and social context from which they emerged. It is in this sense that the work required to grasp the meaning of past theories has something in common with the effort demanded to reconstruct the beliefs and actions of distant cultures—in both cases the object of inquiry only makes sense when seen in the time, place and concerns of a culture that is not our own.

Understanding science as a cultural activity, then, means learning to identify and to interpret the complicated and particular collection of shared actions, values, signs, beliefs and practices by which groups of scientists make sense of their daily lives and work. It is an approach which also draws upon anthropology in attempting to deal with relatively small communities of practising scientists, and which seeks to understand theories and experiments not as idealised or reified entities, but as part of the daily-lived experience of collections of individuals. This kind of approach has already been widely applied to the history of the experimental sciences, but the literature on the theoretical side is much less well developed.[1] This asymmetry is due, at least in part, to a prejudgement that while experiments can be historicised through the material culture and hands-on work of the laboratory, theories are isolated, ready-made products, which transcend the historical time and place of their making (Warwick, 1992, 1993). Theory and practice are traditionally understood as very different kinds of entity, only the latter being identified with experiment. It is in part towards redressing this imbalance that we have dedicated this volume—towards a contextualised treatment of the manifold practices of theory.[2] In different ways each of the authors in this volume is trying to capture distinctive aspects of particular theoretical communities, from Georgian Cambridge, England, to contemporary chaoticians.

Our questions come neither purely from the 'external' institutional side nor from the purely 'internal' (intellectual history) side, but rather are of the following type. What kind of pedagogy forms the theorists a community wants? How much mathematical rigour is appropriate in theoretical physics within a given community? How do theorists and experimentalists interact, who leads and who follows? Such inquiries necessarily combine the contextualisation of scientific

[1] For some excellent examples of this kind of work, see Buchwald (1995), Collins (1985), Franklin (1986), Galison (1987, 1997), Gooding (1990), Gooding *et al.* (1989), Hacking (1983), Heilbron and Seidel (1989), Hermann *et al.* (1987, 1990), Holton (1978), Licoppe (1996), Pickering (1981), Shapin and Schaffer (1985), Smith and Wise (1989).

[2] On treating theory, experiment and instrumentation practices on a symmetrical footing, see Galison (1987, Chapter 5, and 1997, Chapters 1 and 9), Warwick (1992), pp. 631–634.

work with technical content—otherwise the questions themselves become meaningless. Indeed, the kind of analysis envisaged here is not one that attempts to explain scientific work by making it derivative of the social, political and economic interests of the scientists involved (though such factors might figure in the analysis), but one that, where appropriate, directly engages the technical and intellectual issues with which the historical actors were themselves concerned. It is through this kind of immersion that a historian can reasonably claim to be investigating a 'culture' of modern physics, and doing so using categories that the practitioners themselves would recognise.

This use of anthropological technique for historical purposes is not without interpretive problems, however, and requires some qualification. One notable difference between historically distant and relatively recent science (though it is only a difference of degree) is that whereas the former appears, or can easily be made to appear, sufficiently exotic to require recuperation by cultural analysis, the latter can seem so natural and technically self-contained as to be entirely inaccessible to the same techniques. From an anthropological perspective this apparent asymmetry derives not from any essential difference between the cultural situation of past and present science, but from our own familiarity with the culture of the present. We have already noted that a historian engaged in the kind of analysis suggested here will often have to draw upon the very technical skills employed by the physicists themselves, and, in this sense, shares a substantial part of the practitioners' culture. Yet simply to describe a culture of modern physics from what was characterised above as 'the natives' point of view' would be to do little more than to elicit the standard repertoire of practitioners' accounts of the aims, nature and justifications of physics in the 20th century. But, as no lesser figure than Einstein famously remarked: 'If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds' (Einstein, 1973, p. 264).

Here again recent work in anthropology can be of considerable assistance in the analysis of the cultures of modern physics. An important trend in anthropology over recent years has been to apply the methods of ethnography to the very Western society from which they originated. Drawing upon the collective experience gained through numerous encounters with geographically distant cultures, some anthropologists have learned to 'play the stranger' in order to throw aspects of their own previously taken-for-granted culture into unfamiliar relief. Such a distancing might seem at first to sit uncomfortably with the attitude described above. We suggested, it will be recalled, that cultures are better understood by viewing them from the natives' perspective, but have now claimed that understanding a culture requires us necessarily to step back from that perspective. This tension can be made productive, if not entirely relaxed, by reflecting for a moment upon the role of the analyst. Neither the ethnographer nor the cultural historian can provide an absolutely neutral account of a culture; rather, the identification and description of cultural characteristics invariably emerges from a comparison or confrontation between cultures. In other words, we recognise other cultures because they are not like our own, and, for the same reason, are inclined to take our own culture for granted. This is an important point to keep in mind as it implies both that all studies of local cultures will be coloured by the analyst's perspective and that the cultural characteristics identified as significant in a study will not necessarily be deemed important or similarly accounted for by the subjects themselves. The residents of a particular city or participants at a theoretical physics seminar are just as likely to be amused, perplexed, delighted or disgruntled by an ethnographer's account of their activities as are the onlookers at a Balinese cockfight. What is important for the historian, is that the ethnographer's technique of reflexive defamiliarisation, now routinely employed in anthropological writing about contemporary Western culture, is useful in generating a critical cultural perspective on modern science. But while anthropologists tend to stress contrasts across contemporary, geographical space, historians typically emphasise transitions and contrasts across time. As historians and anthropologists have learned from one another, ethnography has become more historically informed, while the cultural history of science has focussed increasingly on the differences and interactions between contemporary, but geographically separated, sites of scientific activity.

All of the essays in this collection draw to some extent upon the anthropological sensibilities and notions of culture discussed above. The first two, by Andrew Warwick and David Kaiser, approach the culture of mathematical physics through scientific pedagogy. Modern theoreticians spend many years mastering the physical principles and mathematical methods that constitute much of their professional expertise, yet this process of enculturation, the techniques it employs and the range of sensibilities it engenders seldom figure in accounts of theoretical work. Theoreticians are, as it were, 'merely' educated at one or other university, little significance being attached to the historical, geographical and intellectual peculiarities of the training they receive. Warwick's essay addresses these issues by tracing the history of some of the most seemingly mundane aspects of the theoretician's craft. Theoretical physicists today take it for granted that those joining their profession have been taught a wide range of advanced technical skills and that the skills in question have been mastered and examined using paper and pen.

Warwick discusses the origins and significance of paper-based learning and examining at Cambridge University in the early 19th century, and argues that the adoption of the mathematician's methods in student training marked an extremely important cultural watershed both for undergraduate studies and for the discipline of mathematical physics. Prior to the 19th century, the culture of undergraduate study was one of reading and oral debate; students were required neither to reproduce their knowledge on paper nor to solve technically demanding mathematical problems. With the rise of written examinations, however, a new model of undergraduate study was gradually installed, based on the problem-solving techniques developed by 18th-century mathematicians. This model effectively bridged the previously wide gap between classical undergraduate studies and the quite separate research activities pursued by some senior members of the university. In order to prepare students to tackle increasingly difficult mathematical problems, tutors began teaching small classes of students of roughly equal ability, devising progressive technical exercises, and drilling students using problem-sets from past examination papers. This new pedagogical economy enabled students to reach previously unimagined levels of technical expertise, and it was upon the paper-based world of mathematical study in the 19th century that the foundations of modern technical training were painstakingly built.

Kaiser's contribution explores the different interpretations of the general theory of relativity that were instantiated and propagated in new physics courses and textbooks during the 1960s and 1970s. From the mid-1920s to the late 1950s, general relativity appeared marginal to the vast majority of practising physicists, and had largely been shunted into a specialised branch of mathematics and mathematical physics. Physics students rarely learned it in courses, even at the graduate level. Following the so-called 'renaissance' of the subject in the early 1960s, however, general relativity became once again an active topic of research and was rapidly reintroduced to undergraduate and graduate studies. But, as Kaiser shows, the new textbooks on general relativity offered more than one account of the theory, each account springing from the technical apparatus most familiar to the author. Kaiser argues that these interpretations can be associated with different sub-cultures of technical practice, and that allegiance to these sub-cultures becomes visible in heated debates over the 'proper' interpretation of the theory.

Jeff Hughes's essay is concerned with the relationship between the very different cultures of theory and experiment at the Cavendish Laboratory in Cambridge during the 1920s and early 1930s. At the beginning of the 20th century there were deep divisions in Cambridge physics between the experimental tradition of the Cavendish and the distinguished school of mathematical physicists associated with the famous Mathematical Tripos. The experimenters, who relied in their work on radioactivity upon relatively simple and highly visual models of the atom, regarded the microphysical speculations of the mathematicians as conceptually fanciful and technically incomprehensible. The new theories of relativity and quantum mechanics were treated with a similar mixture of incomprehension and suspicion by Cavendish experimenters until the mid-1920s, but in the latter years of the decade they began to collaborate with a number of theoreticians working on the new quantum mechanical interpretation of the atom. Hughes shows how and why this new and unlikely alliance between two culturally dissimilar groups occurred and argues that the interaction was a major factor in the emergence of the new discipline of nuclear physics.

The essay by Norton Wise and David Brock explores a more recently self-defining subculture of theoretical physicists, which has identified itself as practising a 'postmodern quantum mechanics'. This incipient discipline is concerned with the behaviour of physical systems in the mesoscopic domain which lies in the transition zone between the quantum microworld and the macroworld of classical physics. Using computer-generated trajectories, physicists working in this field have found fascinating similarities between the quantum mechanical behaviour of particles confined in spaces of mesoscopic dimensions and the chaotic behaviour of some macro-systems. Wise and Brock argue that it is this blend of premodern and modern techniques that the self-styled 'chaoticians' recognise as constitutive both of a new discipline and of a new approach to physics. This approach sees the individualistic behaviour of mesoscopic systems as heralding a new physics which focusses on the peculiarities of diverse physical systems rather than the fundamental and unifying properties of all systems. An approach so at odds with widely held views on the reductionist nature of physics has generated some controversy, and Wise and Brock use exchanges between chaoticians and other physicists to help characterise the culture of postmodern quantum mechanics.

In the final essay in this collection, Peter Galison examines the theoretical culture of wartime Los Alamos, attending in particular to the work of Richard Feynman. Before the war, Feynman's work with his advisor, John Wheeler, had been enormously imaginative, more programmatic in its reformulation of the known theories of electrodynamics and quantum mechanics than oriented towards direct experimentation or quantitative prediction. During the war, however, he was thrust suddenly into a new role, mediating between theorists and experimentalists, between theorists and Oak Ridge engineers, between theorists and human 'computers', and between theory and the designers of a proposed alternative nuclear bomb known as the hydride. This was theory in a novel world, theory charged with the task of reliably, quantitatively, and quickly assessing a myriad of technical alterations: theory face-to-face with work more like patent variations than speculative reformulations of existing theory. Galison argues that, in this war-directed world, Feynman's work took on a more visual, experiment- and rule-oriented character, in short a 'modular-effective' theoretical culture that proudly jettisoned mathematical niceties in the interest of direct prediction and qualitative understanding. By the later 1940s, when Feynman had put quantum electrodynamics into a visual, experimentally-directed and rule-structured form, the theory, especially as it was applied to renormalisation, looked alien enough from prewar mathematical-theoretical culture, that the great Cambridge-trained theorist of the 1930s, Paul Dirac, rejected it in no uncertain terms. Here was a clash of theoretical values, not a dispute over experimental prediction. What for Feynman was a visualisable, calculation-rich and basic understanding of underlying physical processes was for Dirac no more than rule-following, a calculus that awaited true physico-mathematical analysis.

Taken together, we hope that the essays of this volume help open a space in which theory can be discussed both in substance and in context. Not: theory-as-theorems in opposition to experiment-as-practice. Rather: theoretical practice studied with all the care and attention to detail that the best historians have devoted to experimental practice. The rewards of such inquiry focussed (following Einstein) on the deeds of theoreticians can be great, not only for the study of physical theories in this special issue of *Studies in History and Philosophy of Modern Physics*, but for our understanding of theory as it has developed in the wider sphere of the physical sciences.

Early versions of the papers published in this volume were presented at a session at the History of Science Society Meeting (Atlanta) in 1996 and at a one-day meeting at Imperial College (London) in 1997. The History of Science Society session benefited from the helpful comments of Naomi Oreskes. The London meeting was generously supported by the British Academy, the Centre for Philosophy of Social and Natural Sciences (London School of Economics) and the Centre for History of Science, Technology and Medicine (Imperial College). The editors would like warmly to thank the institutions mentioned above for their generous financial support, and also the participants at the meetings for their many helpful comments and criticisms.

References

Buchwald, J. Z. (1995) *Scientific Practice*: *Theories and Stories of doing Physics* (Chicago: The University of Chicago Press).

Collins, H. M. (1985) *Changing Order*: *Replication and Induction in Scientific Practice* (London: Sage).

Einstein, A. (1973) *Ideas and Opinions*, edited by Carl Seelig (London: Souvenir Press).

Franklin, A. (1986) *The Neglect of Experiment* (Cambridge: Cambridge University Press).

Galison, P. (1987) *How Experiments End* (Chicago: The University of Chicago Press).

Galison P. (1997) *Image and Logic*: *A Material Culture of Microphysics* (Chicago: The University of Chicago Press).

Gooding, D. (1990) *Experiment and the Making of Meaning*: *Human Agency in Scientific Observation and Experiment* (Dordrecht: Kluwer).

Gooding, D., Schaffer, S. and Pinch, T. (1989) *The Uses of Experiment* (Cambridge: Cambridge University Press).

Hacking, I. (1983) *Representing and Intervening* (Cambridge: Cambridge University Press).

Heilbron, J. L. and Seidel, R. W. (1989) *Lawrence and his Laboratory* (Berkeley: University of California Press).

Hermann, A., Krige, J., Mersits, U. and Pestre, D. (1987) *History of CERN*: *Vol. 1*: *Launching the European Organization for Nuclear Research*, with a contribution by L. Belloni (Amsterdam: North-Holland).

Hermann, A., Krige, J., Mersits, U. and Pestre, D. (1990) *History of CERN*: *Vol. 2*: *Building and Running the Laboratory*, *1954–1965*, with a contribution by L. Weiss (Amsterdam: North-Holland).

Holton, G. (1978) *The Scientific Imagination* (Cambridge: Cambridge University Press).

Kuhn, T. S. (1984) 'Revisiting Planck', *Historical Studies in the Physical Sciences* **14,** 231–252.

Licoppe, C. (1996) *La formation de la pratique scientifique* (Paris: Découverte).

Pickering, A. (1981) 'The Hunting of the Quark', *Isis*, **72,** 216–236.

Shapin, S. and Schaffer, S. (1985) *Leviathan and the Air-Pump*: *Hobbes*, *Boyle and the Experimental Life* (Princeton: Princeton University Press).

Smith, C. and Wise, M. N. (1989) *Energy and Empire* (Cambridge: Cambridge University Press).

Warwick, A. C. (1992) 'Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905–1911', Part I 'The Uses of Theory', *Studies in History and Philosophy of Science* **23,** 625–656.

Warwick, A. C. (1993) 'Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905–1911', Part II 'Comparing Traditions in Cambridge Physics', *Studies in History and Philosophy of Science* **24,** 1–25.