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The British Society for the History of Science

Review: Kuhn and the Philosophy of Science. Theories of Science
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Source: *The British Journal for the History of Science*, Vol. 12, No. 3 (Nov., 1979), pp. 289-295
Published by: Cambridge University Press on behalf of The British Society for the History of Science
Stable URL: http://www.jstor.org/stable/4026003
Accessed: 15-03-2015 19:31 UTC

ESSAY REVIEWS

◀ KUHN AND THE PHILOSOPHY OF SCIENCE ▶

THEORIES OF SCIENCE

The Essential Tension: Selected Studies in Scientific Tradition and Change. By Thomas S. Kuhn. Chicago & London: University of Chicago Press, 1977. Pp. xxiii+366. $18.50/£12.95.

Although this collection contains very little that is new—only the preface and two of the fourteen essays have not previously appeared in print—it is likely to have an important influence on our understanding of Kuhn's approach to scientific change and on our understanding of scientific change itself.[1] Having so many papers collected in one place should help to prevent those inadequate, one-sided appraisals of Kuhn's ideas that have been so common in the past. And, even though the papers chosen for inclusion tend to be philosophical and programmatic rather than 'straightforwardly historical' (p. x), they reveal enough of the more important elements of Kuhn's thinking to make the task of developing a consistent account of his work as a whole an intriguing and challenging business.

One of the more striking aspects of Kuhn's work is how much of it is shaped by his early experiences as a practising historian of science. What we find above all is the influence of his discovery in 1947 of 'the connected rudiments of an alternative way of reading (historical) texts', according to which in order to understand why, say, Aristotelian physicists said what they did, it is necessary 'to some extent (to learn) to think like one' (p. xi and p. xii). This discovery, which—as Kuhn himself notes (p. xiii)—is a commonplace among historians, was for him nothing short of 'decisive' (p. xii), influencing many of his views about the correct way of practising history of science, the origins of modern science, and the nature of scientific change.

The influence of the 'decisive episode' on Kuhn's view about the practice of history and his own practice is direct and unequivocal. If historians are to avoid distorting the history of science, he tells us their attitude should be 'neither reverence nor contempt, but first a kind of hypothetical sympathy'. (This remark of Bertrand Russell's is quoted twice by Kuhn—on p. 108 and p. 149.) As far as possible, historians should set aside the science that they know; they should study the textbooks and journals of the period they are investigating; they should ask what their subjects' problems were and how these came to be problems for them; they should ask what their subjects thought they had discovered and what they took the bases of these discoveries to be; they should pay particular attention to their subjects' apparent errors since these reveal more of the 'mind at work' than do ideas that modern science retains; etc. (p. 110).

In Kuhn's own case, there can be little doubt that 'the search for best, or best-accessible, readings' has indeed been 'central to [his] historical research' (p. xii). Maxims like those mentioned clearly motivate his attempt in 'Concepts of cause in the development of physics' to provide 'a schematic epitome of the four main stages in the evolution of causal notions in physics' (p. 24); his suggestion in 'Energy conservation as an example of simultaneous discovery' that 'a contemplative immersion in the works of the pioneers [in the development of the idea of energy conservation] and their contemporaries may reveal [among the almost innumerable factors that caused the pioneers to make the discoveries they did] a subgroup of factors which seem more significant than the others' (p. 72); and his attempt in *The Copernican revolution* to 'provide at least preliminary evidence' for the position that 'the techniques developed by historians of ideas can provide a kind of understanding that science will receive in no other way'.[2]

The discovery concerning the way history of science should be done is also the driving force in Kuhn's seemingly tireless campaign against the whig historian, who 'stands on the summit of the twentieth century, and organizes the scheme of history from the point of view of his own day'.[3] Here, what is especially interesting is not so much that Kuhn warns against reading the ideas and categories of the present into the past—we would expect that—but that he develops the demand in a particularly extreme form. In 'Mathematical versus experimental traditions in the development of physical science', for instance, he observes that histories of technical specialities such as the history of electricity are 'often profoundly unhistorical' since they group together phenomena which have only recently (e.g. since the seventeenth century) been seen to constitute a single subject matter (p. 33).

Somewhat less obvious is the bearing of Kuhn's discovery on the relationship of the philosophy of science to the history of science. Unlike certain historically-oriented philosophers of science who agree with Lakatos that 'history of science without philosophy of science is blind',[4] Kuhn holds that it is no less anachronistic to read back contemporary philosophy into the history of science than it is to read back contemporary science into it. For him, 'what Lakatos conceives as history is not history at all but history fabricating examples',[5] and 'the living movements in philosophy of science . . . particularly as the field is currently practiced in the English-speaking world . . . [are] more likely to mislead than to illuminate historical research' (p. 11). (Of course, Kuhn does not deny that some knowledge of philosophy may be useful to historians whose subjects are concerned with philosophical or philosophy-related issues (cf. p. 10), nor that the results of the history of science can and should have an important bearing on the philosophy of science (cf. p. 19).)

These points of Kuhn's, at least in general outline, seem to me to be important and well-taken. Rather less convincing is his observation that what he had discovered in the summer of 1947 was the hermeneutic method (p. xiii). It may be true, as Kuhn claims (p. 5) and William Dray and others have argued at length, that the role of general laws is strictly limited in history, but this is not to say that historians practise the hermeneutic method. What the 'decisive episode' shows is that historians should attempt to develop plausible, coherent, undistorted accounts of the available historical data (cf. p. xii); it does not show that the methods they use to understand texts are different from the methods that are used to understand other phenomena.

Another way in which the 'decisive episode' has influenced Kuhn's views can be discerned in his account of the origin of modern science. At the time that he realized that there are alternative ways of reading texts, he also came to the view that during the seventeenth century there had been 'a global sort of change in the way men viewed nature and applied language to it' (p. xiii). Hypothetical sympathy is required to understand pre-seventeenth-century mechanics not because it is primitive, but—in Butterfield's words—because scientists, during the transition to modern science, put on 'a different kind of thinking cap' and picked up 'the opposite end of the stick'.[6]

Almost as soon as he had formulated this position, however, Kuhn seems to have realized that it was in serious need of qualification (cf. p. 35, n. 3). If the transformation of science in the seventeenth century involved a new way of conceptualizing old data rather than the accumulation of new data, we seem forced to treat the experimental work undertaken during this period as unimportant or, following Alexandre Koyré, as a fraud (p. 46). Although the decisive episode suggested that Koyré and Butterfield were right in holding that the Scientific Revolution was a revolution of ideas, Kuhn realized—as he put it himself later on—that 'other vitally important things also happened during the sixteenth and seventeenth centuries' (p. 41).

Kuhn's response to this difficulty is as important as it is simple. It is that we should distinguish the classical sciences (astronomy, harmonics, mathematics, optics, statics and the study of local motion—cf. pp. 36–9) from the Baconian sciences (heat, magnetism, electricity, chemistry, etc.—cf. p. 47), and restrict the 'new thinking cap' idea to the classical sciences. '[If] one thinks of the Scientific Revolution as a revolution of ideas', Kuhn remarks, 'it is the changes in these traditional, quasi-mathematical fields which one must seek to understand' (p. 41). Put otherwise, Kuhn's point is that we can alleviate the tension in Butterfield's work between those chapters in which the 'new thinking cap' idea is to the fore and those in which the issues are treated in an essentially traditional manner by noticing that the former have to do with the classical sciences, and the latter with the Baconian sciences (cf. p. 35, n. 3, and p. 131, n. 2). (This view is developed in Kuhn's work in various places; cf. especially pp. 35–9, 116–18, 136–7, and 213–21).

Notice that this account of the origins of modern science dovetails with the account of science Kuhn develops in *The structure of scientific revolutions*. Here cultural and socio-economic factors are seen as playing an important role in the establishment of new areas of scientific investigation, while sciences that have already achieved a significant body of technical doctrine are seen as being transformed not by the discovery of new data but by the reconceptualization of data already at hand. Although in *The structure of scientific revolutions* Kuhn is concerned primarily with the kind of change that took place in the classical physical sciences in the seventeenth century, and in chemistry in the eighteenth century, it is not his view, as some critics have supposed, that the development of science is immune to external influences (cf. p. xv).

Limiting the Koyré-Butterfield thesis to the classical sciences certainly enhances its plausibility. However, we may still wonder whether Kuhn's approach to these matters is entirely satisfactory. For, on Kuhn's own account of the classical and Baconian sciences, transitions involving new observations and transitions involving 'new concepts' occur in *both* kinds of science; it is not the case that the classical and Baconian traditions are free of, respectively, experimental and conceptual considerations. Experiment occurred in the classical sciences but played a *smaller* role and tended to be *more often* designed to show what was already known than to find out how nature would behave (p. 43); to have *deeper* roots in tradition (p. 45), and to be *more closely* keyed to the theories which had called them forth (pp. 45–6). On the other hand, the Baconian sciences on occasion involved significant conceptual developments; it is, after all, no mean achievement to realize that lightning, the amber effect, and the torpedo (electric eel) should be grouped together as electrical phenomena (cf. pp. 33, 46). Such considerations suggest that it would be a mistake to associate the traditions characterized by Koyré and Butterfield too closely with the transformation of the classical sciences in the seventeenth century.

Kuhn's distinction between the classical and Baconian sciences also figures prominently in his discussion of Merton's thesis that the Puritan ethic was especially congenial to the development of science. Just as the appeal of the Koyré-Butterfield thesis may be enhanced by restricting it to the classical sciences, so, Kuhn argues, the appeal of Merton's thesis is 'vastly larger if it is applied not to the Scientific Revolution as a whole, but rather to the movement which advanced the Baconian sciences' (p. 59). Although it is wrong to think that 'an explanation of the rise of the new experimental philosophy is tantamount to an explanation of scientific development' (p. 136), it is reasonable to think that the investigation and manipulation of nature prompted by Puritanism played an important role in the development of the non-mathematical, Baconian sciences. In response to this line of argument, we can certainly agree that restricting the Merton thesis in the way Kuhn suggests enhances its plausibility. However, many difficulties remain, not least of which is that studies of the development of science in Britain—the centre for Baconian science (cf. p. 58)—have raised serious difficulties for the thesis.[7] Indeed, Kuhn himself remarks on the difficulty of identifying the Puritan 'ethos' (p. 59).

No less important than the influence of the decisive episode on Kuhn's views about the practice of history and the origins of modern science is the influence of this episode on his views about the nature of scientific theorizing and scientific change. Concerning the first of these, what Kuhn learned is that theorizing may be of two distinct kinds: it may conform to tradition and involve the development and application of an already established conceptual scheme, or it may be revolutionary and involve the substitution of one thinking cap for another. In the terminology of 'The essential tension', what Kuhn came to appreciate is that scientific thought may be either convergent (fundamentally conservative) or divergent (fundamentally innovative) (pp. 225–7). Concerning scientific change, what Kuhn learned is that science progresses not only by the accumulation of new observational findings but also by intellectual upheaval. Thus, it is hardly surprising that Kuhn sometimes talks as though conceptual change is a part of the traditional picture of science, and sometimes as though it is not; this is what we would expect given that science is traditionally viewed *both* as an enterprise in which the kind of flexibility and open-mindedness characteristic of revolutionary, divergent thought is at a premium *and* as an enterprise which progresses by accumulating new observations rather than by the replacement of conceptual schemes.

In 'The essential tension', written in 1959, these ideas are developed in an interesting way. The popular stereotype of the scientist as a divergent thinker, Kuhn argues, is incomplete and must be supplemented with 'the other face of [the] same coin', with the image of the basic scientist as a 'firm traditionalist' (pp. 236–7). As Kuhn puts it: innovative, divergent thought and tradition-bound, convergent thought are 'inevitably in conflict'; they give rise to an essential tension at the heart of the enterprise (pp. 226, 227). 'Within the group', Kuhn remarks, 'some individuals may be more traditional, others more iconoclastic . . .; yet education, institutional norms, and the nature of the job to be done will inevitably combine to ensure that group members will, to a greater or lesser extent, be pulled in both directions' (p. 227, n. 2).[8] Here the important point to notice is that Kuhn takes education, the textbook tradition, etc., as inculcating two discordant attitudes concerning correct scientific practice; he does not think of them as producing firm traditionalists, puzzle solvers, convergent thinkers.

Between 1959 and 1961 when the first draft of *The structure of scientific revolutions* was completed and 'The function of dogma in scientific research' was written, Kuhn's thinking concerning these matters underwent a remarkable transformation. The two modes of thought that Kuhn previously viewed as being inevitably in conflict were now seen as being characteristic of two distinct kinds of science. Tradition and dogma give rise to extended periods of convergent research, and, in the normal course of events, scientists are and should be firm traditionalists (puzzle solvers, convergent thinkers) engaged in what is essentially a mopping-up operation. Whereas in 'The essential tension' convergent and divergent research are held to be inevitably in conflict, we are now told that periods of normal (tradition-bound, paradigm-governed) science alternate with periods of extraordinary (revolutionary, paradigm-establishing) science.

What prompted this shift in Kuhn's thinking? Internal evidence and Kuhn's own autobiographical remarks suggest that the new view originated in his investigation of the paradigm concept, and that Kuhn took the need for such a device to be established by the fact that 'periods governed by one or another traditional modes of practice must necessarily intervene between revolutions' (p. xvii, cf. also p. 227). Since it is implausible to attempt to account for extended periods of convergent research by 'enumerating the elements about which the members of a given community supposedly agree' (p. xviii), we are forced to invoke considerations having to do with the way scientists are trained, the influence of the textbook tradition and the like. Specifically, during the period under consideration it became Kuhn's view that normal interludes are to be explained by appealing to the idea of a paradigm. 'Once [this] piece of [the] puzzle fell into place', he tells us, 'a draft of [*The structure of scientific revolutions*] emerged rapidly'.[9]

There are two points about the view of *The structure of scientific revolutions* worth noting here. First, one of the many objections which have been urged against the view Kuhn puts forward there is, interestingly, that it fails to recognize that there is an interplay between tradition and innovation in science. As Feyerabend puts the point, we should 'speak of the normal *component* and the philosophical *component* of science and not of the normal *period* and the *period* of revolution'.[10] Second, Kuhn's argument leading to the conclusion that normal science is paradigm-governed can be criticized on the grounds that it fails to recognize the role of non-empirical, conceptual considerations in science. What Kuhn neglects to take into account is that scientists can and do recognize what Buchdahl has called the 'explicative' and 'architectonic' components of theory choice—i.e. considerations having to do with the intelligibility (widely construed) of a theory and its 'rationale'.[11] Since in the normal run of events changes of a conceptual nature are more difficult to sustain than changes which involve articulating and applying concepts already in place, why not think that there is an ever-present tension in science between tradition and innovation and take normal science to be nothing other than a surface effect? In the present context I think it not unfair to say that Kuhn's analysis of the decisive episode, in general so unerring, has failed him in a significant way. For as is well known, Kuhn's thinking from the very start has been dominated by the view that conceptual change, change which involves putting on a new thinking cap, is problematic in ways that change involving new observations is not. This point, incidentally, is underlined in a striking way in Kuhn's remark in the preface of the present volume that the decisive episode 'quickly led (him) to books on Gestalt psychology and related fields' (p. xiii).

In his more recent work, represented here by 'Logic of discovery or psychology of research', 'Second thoughts on paradigms' and 'Objectivity, value judgement, and theory choice', Kuhn has again substantially modified his view. He now distinguishes between exemplars, concrete problem solutions (cf. p. xix), and disciplinary matrices—'most or all of the objects of commitment described in [*The structure of scientific revolutions*] as paradigms, parts of paradigms or paradigmatic' (p. 297); he no longer requires that paradigms (i.e. disciplinary matrices) be 'universal';[12] he allows that what he previously called pre-paradigmatic science may be paradigm-governed;[13] he has softened his views concerning incommensurability to allow for a certain amount of communication between proponents of different disciplinary matrices;[14] and he has begun to lay greater stress on the similarities between his views and traditional views concerning theory choice (cf. p. 321). Moreover, Kuhn has promoted certain themes of *The structure of scientific revolutions* to a more central position in his account. He argues that good scientific theories are characterized by values such as accuracy, consistency, congruence with other theories, scope, simplicity and fruitfulness (pp. 321–2); that scientists regularly differ in their judgements concerning the relative merits of competing theories since, although the values mentioned are shared, they are applied and weighted in different ways (pp. 322–3); and that—provided the relevant group of scientists is sufficiently large—individual differences cancel themselves out, so that what comes to be accepted by the group as a whole is uncontaminated by subjective, non-scientific factors (p. 333). (The last of these claims is reminiscent of the Darwinian theory of scientific progress, rapidly sketched at the end of *The structure of scientific revolutions*, according to which revolutions are resolved by 'the selection by conflict of the fittest way to practice future science'.[15] A particularly clear and concise statement of the new view appears in 'Reflections on my critics' (not included in this volume): 'Group behaviour', Kuhn remarks in this essay, 'will be affected decisively by the shared commitments [i.e. by the kind of values alluded to above], but individual choice will be a function also of personality, education, and prior pattern of professional research'.[16]

As these ideas are introduced piecemeal, it is difficult to obtain a clear picture of Kuhn's present view. I think we may legitimately query some of its aspects: the proposed account of theory choice, for instance, appears to be excessively permissive, and it is unclear whether the account of scientific progress, with its appeal to what looks suspiciously like an invisible hand, has explanatory force.[17] On the other hand, however, the new account rehabilitates the important idea of there being an essential tension at the heart of the enterprise, and recognizes in plain terms that 'scientific knowledge is essentially a *group* product and that neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it' (p. xx; Kuhn's italics). Certainly, we would be ill advised to view Kuhn's recent work in the way some have: as a capitulation to the philosophers, or as a demonstration that the initial project was misconceived.

It would be a mistake to think that there is only one satisfactory way of organizing Kuhn's ideas; his views are sufficiently subtle and complex to admit of alternative, even competing, interpretations. The point is that by tracing Kuhn's thought to the decisive episode we can obtain an indication of how some of the strands of his work are related to one another, and a better understanding of why he holds some of the views that he does. This kind of approach, moreover, conforms tolerably closely to what Kuhn requires of the history of ideas.

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NOTES

[1] The collection comprises 'The relations between the history and the philosophy of science' (1968, revised 1976), pp. 3–20; 'Concepts of cause in the development of physics' (1971), pp. 21–30; 'Mathematical versus experimental traditions in the development of physical science' (1976), pp. 31–65; 'Energy conservation as an example of simultaneous discovery' (1959), pp. 66–104; 'The history of science' (1968), pp. 105–26; 'The relations between history and the history of science' (1971), pp. 127–61; 'The historical structure of scientific discovery' (1962), pp. 165–77; 'The function of measurement in modern physical science' (1961), pp. 178–224; 'The essential tension: tradition and innovation in scientific research' (1959), pp. 225–39; 'A function for thought experiments' (1964), pp. 240–65; 'Logic of discovery or psychology of research' (1970), pp. 266–92; 'Second thoughts on paradigms' (1974), pp. 293–319; 'Objectivity, value judgement, and theory choice' (1973), pp. 320–39; and 'Comment on the relations of science and art' (1969), pp. 340–52. Only the first and the penultimate essays have not previously appeared in print.

[2] T. S. Kuhn, *The Copernican revolution*, Cambridge, Mass., 1957, p. viii.

[3] H. Butterfield, *The whig interpretation of history*, London, 1931, p. 13.

[4] I. Lakatos, 'History of science and its rational reconstructions', *Boston studies in the philosophy of science*, 1971, *8*, 91.

[5] T. S. Kuhn, 'Notes on Lakatos', ibid., p. 143.

[6] H. Butterfield, *The origins of modern science*, 2nd edn., London, 1957, pp. 1, 7.

[7] Cf., for example, D. Kemsley, 'Religious influences in the rise of modern science', *Annals of science*, 1968, *24*, 199–226.

[8] The idea that there is an essential tension in science between tradition and innovation is not stated unequivocally in 'The essential tension'. This is not particularly surprising, the essay being prepared within a month or so of Kuhn's recognition of the utility of the notion of a paradigm (cf. p. xix). In addition, it should also be noted that the idea does not, as one might expect, only appear in Kuhn's work prior to *The structure of scientific revolutions:* it can also be discerned in 'The function of dogma in scientific research', which was written after the first draft of the book was completed (cf. A. C. Crombie (ed.), *Scientific change*, London, 1961, p. 368). In *The structure of scientific revolutions*, 2nd edn., Chicago, 1970, Kuhn refers to 'the essential tension implicit in science' (p. 79), but views it as a characteristic of extraordinary science, not of science in general.

[9] T. S. Kuhn, *The structure of scientific revolutions*, op. cit. (8), p. xiii.

[10] P. K. Feyerabend, 'Consolations for the specialist', in I. Lakatos and A. Musgrave (eds.), *Criticism and the growth of knowledge*, Cambridge, 1970, p. 212. Feyerabend's italics.

[11] G. Buchdahl, 'History of science and criteria of choice', *Minnesota studies in the philosophy of science*, 1970, *5*, section I.

[12] T. S. Kuhn, 'Postscript—1969', in *The structure of scientific revolutions*, op. cit. (8), pp. 178, 181.

[13] Ibid., p. 179.

[14] Ibid., pp. 200–4.

[15] Ibid., p. 172.

[16] T. S. Kuhn, 'Reflections on my critics', in Lakatos and Musgrave (eds.), op. cit. (10), p. 241.

[17] For further discussion and criticism along these lines see D. Shapere, 'The paradigm concept', *Science*, 1971, *172*, 708.

◀ KUHN AND THE HISTORY OF IDEAS ▶

The Essential Tension: Selected Studies in Scientific Tradition and Change. By Thomas S. Kuhn. Chicago & London: University of Chicago Press, 1977. Pp. xxiii+366. $18.50/£12.95.

Like any collection of essays written over a period of time, this volume is repetitive. But repetition has its merits in highlighting the author's own assessment of what he considers to be important. The notion of scientific community has this status for Kuhn, and its salience is closely connected