

# Kuhn and His Critics on Normal and Revolutionary Science

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There is ample evidence that Professor Thomas S. Kuhn's concept of scientific paradigms has been accepted as an important, original, and permanent contribution to the discussion and writing on the logic of scientific change, but nevertheless there is something unsatisfactory about it for philosophers in particular.<sup>1</sup>

One aspect of the problem with Kuhn's concept of scientific paradigms has to do with the writings of Rene Descartes and Francis Bacon. Despite their differences, the warnings of both Descartes and Bacon against the possibly pernicious influence of scientific tradition seem to imply that research must go on as if previous research has settled nothing, as if acquired knowledge might be a mere prejudice, and as if the researcher has liberated himself from any influence of acculturation (professional training in particular). The scientist is supposed to remember constantly that the knowledge he thinks he has already might not really be knowledge; consequently, he always must fear the possibly pernicious influence of his "previous knowledge," and should envy and emulate the condition of the *tabula rasa*. Kuhn's views on the logic of what he calls "normal science" and "mopping up" operations may be disturbing to some of his critics because, in contrast to warnings that border on scientific moralizing, Kuhn's writings seem to commit science to operate for the most part with beliefs and methods (paradigms) that may be merely self-perpetuating and self-reinforcing habits of the scientific establishment at a given time.<sup>2</sup>

A second aspect of the problem is that some critics appear inclined to find in Kuhn's views on the logic of "normal science" some encouragement to the scientific community to embrace and apply authoritarian policies of accreditation and reward or support. These critics have frightening visions of behavioristics in a society effectively closed to the unconventional and possibly

revolutionary scientific thinker,<sup>3</sup> and they generally prefer Sir Karl Popper's analysis of the logic of science, which makes science a model of "open society" because it requires all science to maintain a disestablishmentarian – that is, revolutionary – bias.<sup>4</sup> This differs from the first aspect that has been mentioned because the focus of criticism is not on the accuracy of Kuhn's description of the behavioristics of "normal science," but on the possible authoritarian or political uses of the description if it is accepted as accurate.

A third aspect of the problem is that Kuhn's views on scientific revolution, or the subversion of a dominating paradigm and its replacement by another, appear to overemphasize that significant scientific innovation sometimes has involved some seemingly arbitrary psychological spurt – an incident like Paul's sudden enlightenment and conversion on the road to Damascus, or a kind of Darwinian mutant – in the field of either theoretical or experimental style that had the good luck to capture a substantial scientific following.<sup>5</sup> This is just the opposite of the first aspect of the problem. In that aspect, the logical influence on scientists of commonly received ideas and common training was disturbing. In the present aspect, what seems to disturb Kuhn's critics is Kuhn's recognition of uncommon influences that are more psychological and sociological than logical in nature. Nor do these views of Kuhn on scientific revolution appease those critics who find authoritarian implications in Kuhn's views on the logic of normal science: the former views indeed recognize that the paradigms that drive normal science are sometimes forced to yield to paradigms that are new, but these critics still maintain that Kuhn emphasizes what one of them calls "mob psychology"<sup>6</sup> as a significant instrumentality for change.

Oddly, one result of this debate is that some critics on Sir Karl Popper's side appear ready to say that, if Kuhn is right, then "normal science" – which no one denies is the major part of what is meant by "science" – is hardly science, or, in the words of one of these critics, is "hack science,"<sup>7</sup> while genuine (real, important, interesting) science is "extraordinary science."<sup>8</sup> "Extraordinary science" is Kuhn's name for science that – as we are able to perceive with historical hindsight – has had revolutionary consequences. It is important to understand that "extraordinary science" does not mean simply research science, since the latter covers too much scientific research propelled by a domi-

nant theoretical/conceptual/methodological set, but only research that has revolutionary consequences for the dominant attitude and that functions to replace it with another. It is another name for "revolutionary science," as Kuhn thinks of it, and it includes, for example, Galileo, Kepler, Newton, Harvey, Lavoisier, Dalton, Darwin, and Einstein – but not Ptolemy, Galen, Tycho Brahe, Fabricius, Adams and Leverrier, Priestley, Linnaeus, Malpighi, Faraday, Laplace, Lorentz, Planck, Fermi, or innumerable others.

A fourth aspect of criticism directed at Kuhn is that he leans too much toward description and not enough toward prescription. He wants to describe what science is, not declaim on what it ought to be; his attention to science is too historical and not normative enough for his critics. In other words, he may satisfy the psychologist, the sociologist, and the historian, but he does not satisfy the logician/philosopher.<sup>9</sup> One might say that, in the view of the logician/philosopher, the tendency of psychologists, sociologists, and historians is to secularize science; from the perspective of psychologists, sociologists, and historians, however, the tendency of logician/philosophers is to "aetherialize" science.<sup>10</sup>

Certainly, the history of philosophy amply shows that logicians and philosophers, with few exceptions, never have loved the shiftiness of Time, nor have they been able to reconcile themselves to the intractability of Time for transformation into Timelessness. Science is a search for cognitively fruitful norms, but, because any gains it is under the impression of having made are gains in Time's bosom, for the logician/philosopher, no norm that is an outgrowth of actual science has, it seems, credentials that are ever good enough to be believed, that is, trusted in practice. The logician/philosopher may talk about "probability," but he is unhappy about it, as if he would prefer it were it possible to demonstrate that the norms of science had a logically impeccable pedigree. What he does not seem able to appreciate is that, inevitable as trust in a paradigm, no matter what its pedigree, must be in practice, it is just as inevitable that trust itself is tested by Time, so that reminders that the observed gains made in science must not implicitly be trusted, as in normal science they seem to be, amount to hortatory rhetoric. Revolutions in science do not depend on whether Cartesian or Baconian (or Popperian) exhortations are consciously heeded by scientists, but on the undeniable buildup of evidence in normal science itself that some

norm shows signs of failing and needing to be repaired or replaced. In other words, twinges of logical conscience in advance of new experience that inspires them or indicates their applicability are not what make scientific revolutions happen; trust that has been frustrated enough and that normal science itself shows to have been misplaced is what makes controlling paradigms decline and yield their place to new paradigms. So-called revolutionary or “extraordinary” science is a part of normal science and, though historical hindsight for the most part makes it discernible in normal science, “extraordinary” science is not in principle or in fact separable from or competitive with normal science: until a revolution is noticed everything seems normal, and by the time it has been noticed it is essentially over.

Moreover, if an analogy may be taken from the history of geology, scientific change is due more to uniformitarian logical agency than to catastrophic logical (or extralogical) agency, and that includes revolutionary change. Though Lavoisier, for example, is credited for having brought about a revolutionary change with respect to the phlogiston theory, Priestley is only lightly acknowledged in his contribution to Lavoisier’s logic, and that is just too neat. We are supposed to believe that Lavoisier was revolutionary, but that Priestley was not, that Lavoisier overthrew the phlogiston theory, but that Priestley did not – apparently only because Lavoisier finished the job whereas Priestley (with due respect to Mayow) started it, and, moreover, started Lavoisier on it, without realizing that by this move he had doomed the very theory on which he relied for interpreting his relevant work. Kepler, too, began as one might have expected a scientific worker in normal science to begin. He was logically driven by his standards of precise fit to Tycho Brahe’s observations, to realize that the controlling paradigm of astronomical science (the circle) had to be abandoned in favor of the ellipse. But why are we supposed to forget how he began – that is, logically dominated by the conceptual/theoretical attitude of all previous astronomers (heliocentricity was not the “real” revolution)<sup>11</sup> and, moreover, dominated by the observations of Brahe, who was not a Copernican – since, for this episode in astronomy, that is what is required to separate and distinguish normal and revolutionary science in the way in which both Kuhn and Kuhn’s critics seem inclined to do? Yet another example of this irony can be seen in William Harvey’s background. He began as a Galenist, a

position that a pupil of Fabricius could not have avoided.

In this controversy one thing seems to be forgotten, and that is that the only effective revolutions are revolutions that insiders accomplish, that is, members of a community who see one another as peers and who are joined in an uncompleted task. As peers in science must be people with shared knowledge and a shared basic language to express that knowledge (both of which constitute entitlement to common credentials), it is likely that change, small or large, will be an internal thing. Any alternative would be seen as imposition by invading foreigners, the resistance of which would require that individual differences be temporarily forgotten and ranks joined. This seems true for all genuine societies, and the scientific community seems to be no exception.

Thus Kuhn is mainly right about emphasizing the importance of normal science for discerning the logic that moves science along, and his critics – who would emphasize revolutionary, dis-establishmentarian, or extraordinary science – are mainly wrong. But Kuhn is also somewhat wrong in that he himself has encouraged his critics by distinguishing normal and revolutionary science, as if the latter served a different objective, since both the “mopping up” operations characterizing normal science and the role of “novel perspective” characterizing revolutionary science are promoted by the sentiment of the incompleteness of knowledge. This sentiment, then, cannot do anything else but work against the occurrence of scientific complacency, or, in other words, the permanence of any paradigm. The motive without which neither research in normal science nor research in revolutionary science would be comprehensible is residual ignorance – this, and not reminders about the possibly self-serving logical influence of paradigms or traditions of thinking prevailing at any given time, is what guarantees that openness in research which is not arbitrary but rational and which all scientists desire. The words of Isaac Newton seem particularly relevant here, perhaps because they advance an image of science at the same time gigantic (or “extraordinary”) and humble: “If I have seen farther, it is because I have stood on the shoulders of giants.” Although less poetic, perhaps Newton’s words would have been more accurate had they been: “If I have seen farther, it is because I have stood on the shoulders of other scientific workers, great and small, mistaken and unmistaken, and memorable or forgotten.”

The basic problem is not new. It is the problem that always has divided thinkers of a rationalist persuasion from thinkers of an empirical persuasion, and moreover, has divided thinkers of an empirical persuasion who are rationalists at heart (Hume) from thinkers of an empirical persuasion who are pragmatists (Peirce). It is the problem of submitting to the rubric of science decisions that are less than rational but are not irrational – the logic of scientific activity, on account of its inevitable immersion in Time, cannot be expected to be characterized by complete rationality. Meyerson, although dealing with a different matter, adumbrated this problem when he argued that the logic of science is never strictly rational and is also hardly ever irrational, but “reasonable” in the sense in which *practical* men use the word.<sup>12</sup> “Les resultats” settle the question of justified belief, but they always settle it contextually and temporally because the alternatives are paralysis, on the one hand, if strictly rational demands are required to be met, and, on the other hand, solipsistic defiance of the logical influence of past learning and of the sentiment of residual ignorance if rational demands are not respected at all. These are the alternatives – which threaten to embed authoritarianism and irrationality in science – that Sir Karl Popper and other critics of Kuhn’s thesis fear, and it is easy to see that Kuhn’s picture of science, both normal and revolutionary, does not imply comfort for either of them. On the contrary, they are alternatives to be feared if scientists, distributively and communally considered, really were to try to satisfy the standards of rational justification of most logician/philosophers. The standards cannot be ignored but they cannot be met fully either, at least not in the changing and surprising world about which scientists must be rational.

## Notes

1. The basic relevant work of Professor Kuhn is *The Structure of Scientific Revolutions* in the revised and enlarged edition (Chicago: Chicago University Press, 1970). The best collection of pertinent critical philosophical papers is *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1970), hereafter CGK.

2. Notice the following comparisons of Kuhn with Karl Popper on Science: "It is because Kuhn – at last – has noticed this central fact about all real science . . . , namely that it is normally a habit-governed, puzzle-solving activity, not a fundamentally upheaving or falsifying activity (not, in other words, a *philosophical* activity), that actual scientists are now increasingly reading Kuhn instead of Popper: to such an extent, indeed, that . . . 'paradigm' and not 'hypothesis' is now the 'O.K. word'" (Margaret Masterman, "The Nature of a Paradigm," in CGK, p. 60). "Belief may be a regrettably unavoidable biological weakness to be kept under the control of criticism: but *commitment* is for Popper an outright crime" (Imre Lakatos, "Methodology of Scientific Research Programmes," CGK, p. 92). Also, on the *tabula rasa* see Lakatos, CGK, p. 99, and on the rigidity of conceptual frameworks (paradigms) according to H. Poincaré, see Lakatos, CGK, p. 105.
3. For example, John Watkins, in "Against 'Normal Science'" CGK, p. 27, proposes to rebut "Kuhn's view of scientific normalcy as a society of closed minds."
4. "I remember suggesting to him (Kuhn) in 1961 that he should bring out and discuss in his book the clash between his view of the scientific community as an essentially closed society . . . and Popper's view that the scientific community ought to be, and to a considerable degree is, an open society in which no theory, however dominant and successful, no 'paradigm,' to use Kuhn's term, is ever sacred" (John Watkins, CGK, p. 26). And, again, in CGK, p. 28: "Thus we have the following: the condition which Kuhn regards as the normal and proper condition of science is a condition which, if it actually obtained, Popper would regard as *unscientific* . . . Popper has suggested that the motto of science should be: *Revolution In Permanence!* For Kuhn, it seems, a more appropriate maxim would be: *Not nostrums but normalcy!*" Finally, see Paul Feyerabend, "Consolations For The Specialist," CGK, pp. 199–200; and Karl Popper, "Normal Science and Its Danger," CGK, pp. 52–53.
5. J. Watkins, CGK, pp. 34–35; I. Lakatos, CGK, p. 178; S. Toulmin, "Distinction Between Normal and Revolutionary Science," CGK, p. 43.
6. I. Lakatos, CGK, p. 178.
7. J. Watkins, CGK, pp. 27, 31; K. Popper, CGK, pp. 52–53.
8. J. Watkins, CGK, p. 27; P. Feyerabend, CGK, p. 209.
9. P. Feyerabend, CGK, pp. 198–202.
10. M. Masterman, CGK, pp. 61, 67.
11. Cf. J. LaLumia, "Saving The Phenomena And Scientific Change," *Diogenes* 83 (1973): 128–130.
12. Cf. E. Meyerson, "De l'analyse des produits de la pensee," *Essais*, p. 103; also, *Du cheminement de la pensée*, p. 714.