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'The Priority of Paradigms' Revisited

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Zusammenfassung

In diesem Beitrag liefere ich eine Interpretation und Verteidigung der These Thomas Kuhns von der Priorität von Paradigmen. Ich behaupte, daß Kuhns Argument für diese These wichtiger, als gewöhnlich angenommen wird, ist, und zwar sowohl für die Klärung seiner Ideen als auch für die Wissenschaftstheorie im allgemeinen. Anerkennt man seine Kritik an der üblichen Auffassung, daß Regeln den Paradigmen vorausgehen, so erscheint vieles von dem, was er über andere Gegenstände sagt, in einem neuen Licht, und viele Schwierigkeiten, die Philosophen (einschließlich Kuhns) bei ihren Versuchen zur Erklärung des Wissenschaftswandels angetroffen haben, erweisen sich als unbegründet.

Thomas Kuhn's discussion of scientific inquiry in *The Structure of Scientific Revolutions* has been roundly criticized since it first appeared in 1962.[1] Its main categories of analysis have been dismissed as vague, unexplanatory and beside the point, and its treatment of scientific change has been pilloried as subjectivistic, relativistic, irrationalistic or worse. Nonetheless, there are themes in the book, that continue to be worth defending. While Kuhn's general argument may well be untenable, some of his central insights have still not been accorded the attention that they deserve. As I hope to show in what follows, his remarks about normal and revolutionary science are less important and less central to his position than his insistence on the primacy of practice.

Kuhn confronts the question of the general character of scientific investigation most squarely in the fifth chapter of *The Structure of Scientific Revolutions,* 'The Priority of Paradigms'. Although his discussion in this chapter is often confusing and far from conclusive, it highlights an issue that is often lost in his later discussions and in criticisms of his work, namely the fundamental role of skill and learning by example in the developement of science. If we are to understand scientific inquiry, we must, Kuhn tells us, pay close attention to the way in which paradigms (understood as concrete scientific achievements) function in scientists' education and their subsequent work in the profession. We must remember that paradigms have priority both in the sense that they serve as the locus of scientific commitment and in the sense that they function as the primary determinants of scientific change.

To appreciate the force of this point, it is important to bear in mind what Kuhn is reacting to. He is not simply attempting to draw attention to the fact that paradigms figure prominently in scientist's education and practice, a point that few would dispute. What he wishes to establish is the far more interesting

[1] T. S. Kuhn, *The Structure of Scientific Revolutions,* University of Chicago Press, Chicago, Second Edition, 1970. All page references in the text are to this work.

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view that scientific research can be and normally is governed directly by paradigms and that accounts of science cast exclusively in terms of conceptual, theoretical, instrumental and methodological 'rules' are bound to fall short.[2] His main complaint is that we put the emphasis in the wrong place when we picture science as an enterprise governed by rules since 'research problems and techniques . . . may relate by resemblance and by modeling to one another part of the scientific corpus' rather than by 'some explicit or even some fully discoverable set of rules and assumptions' (p. 45).

Stated more generally, Kuhn's aim is to get us to think of science as a conglomerate of practices, customs or traditions. In asserting the priority of paradigms, he is challenging the assumption (expressed in old-fashioned jargon) of the priority of Thought to Being and Reason to Action. What he wishes us to see is that scientific practice is not so much governed by rules as it is the main determinant of them. In brief, as I interpret him, Kuhn is confronting what has been (and what continues to be) the dominant rationalist tradition in philosophy of science. For him, the question of how scientists come to adopt their views is far more important than the question of the general character of scientific rationality abstractly understood.[3]

But why think that practice is primary? In Chapter V of *The Structure of Scientific Revolutions,* Kuhn puts forward four different arguments for this conclusion. First, he observes that 'the search for rules [is] both more difficult and less satisfying than the search for paradigms' (p. 43; see also p. 44 and p. 46). Second, he reminds us that the scientist's 'intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications' (p. 46). Third, he maintains that 'rules become important and the characteristic unconcern about them [vanishes only when] paradigms or models are felt to be insecure' (p. 47). And finally, he contends that 'substituting paradigms for rules [makes] the diversity of scientific fields and specialties easier to understand' (p. 49).

None of these observations is likely to convince proponents of the traditional view of science as a rule-governed enterprise however. They can rebut Kuhn's first point by noting that it is far from obvious that an adequate account of scientific paradigms in terms of rules is out of the question,

[2] I should emphasize here that Kuhn broadens the use of the word 'rule' to cover an 'established viewpoint' or 'preconception' (p. 39) and that he explicitly speaks of scientists' 'commitments – conceptual, theoretical, instrumental, and methodological' as 'rules' (p. 42).

[3] Thus, I take Kuhn's position to be similar in spirit to that of the later Wittgenstein. It is, I think, fair to say that Chapter V of *The Structure of Scientific Revolutions* is the most Wittgensteinian of all Kuhn's writings, but it is also true that Kuhn's understanding of Wittgenstein – despite its source (see p. xi) – is open to question. One might, for example, criticize his assumption that Wittgenstein regarded 'family resemblance' as an explanatory notion (see pp. 44–45). (For more on the connection between Kuhn's views and Wittgenstein's, see P. Barker, 'Uncle Ludwig's Book of Science', *Philosophical Topics,* 12, Supplement, 1982, pp. 76–77). Also worth nothing here is that Kuhn's views run parallel to many of those Michael Oakeshott, although – as far as I am aware – Oakeshott was not an influence on Kuhn. See in particular Oakeshott's paper on 'Rational Conduct' in his *Rationalism and Politics,* Methuen, London, 1962, pp. 80–110.

especially given the wide range of 'commitments' that Kuhn himself recognizes under this head.[4] They can argue in opposition to his second point that learning by example is fully compatible with acting according to rules, and they can counter his third point by observing that rules can be elicited in times of crisis only if they have been previously assimilated. Finally, they can observe in response to Kuhn's argument about the diversity of science that this is also explicable on the assumption that scientists acquire an enormous variety of intellectual commitments (or rules) in the course of their education.

From the standpoint of the traditional view, Kuhn is directing his fire at the wrong target. It may well be true that much of what scientists do is based on experience and practice rather than on the conscious application of rules. But this hardly shows that paradigms have priority, it being no part of the traditional approach that the relevant rules are always explicit. What Kuhn needs to establish is a point that he does not even consider, namely that scientific inquiry remains inexplicable when scientists' tacit commitments as well as their overt ones are taken into account. He cannot simply assume that scientific practice is transparent and its rules discernible by direct inspection. For better or worse, it is the rules that underlie scientific practice that matter, not those that lie open to view.[5]

These observations are sound enough but they do not do justice to the general thrust of Kuhn's argument, still less demonstrate that he is wrong to hold that rules are secondary to practice. Although Kuhn is plausibly read as attempting to refute the traditional conception of science, it is unhelpful to read him this way. The more interesting interpretation is that his main object is to emphasize important facts about scientific practice that are regularly overlooked by proponents of the traditional viewpoint and to raise the crucial question of the point of insisting on the primacy of rules. On this interpretation, Kuhn is not so much concerned to refute traditional philosophical thinking about science as to point out that it can be understood without the encumbrance of philosophical theory. In emphasizing the priority of paradigms, he means to draw our attention to the simple but profound fact that science is perfectly capable of taking care of itself.

Certainly, Kuhn goes along way towards establishing that it is gratuitous to assume that rules guide practice. He is surely right to observe that 'normal science can be determined in part by the direct inspection of paradigms' (p. 44) and to stress that the scientist's ability to do successful research can be 'understood without recourse to hypothetical rules of the [scientific] game' (p. 47). Moreover, there can be little doubt that the scientific community has no need of rules as long as it 'accepts without question the particular problem-solutions already achieved' (*ibid.*) and even less that the diversity of scientific

[4] See footnote 2.

[5] Kuhn's position with regard to rules seems especially strange given his endorsement of the view that 'much of the scientist's success depends on "tacit knowledge", i.e., upon knowledge that is acquired through practice and that cannot be articulated explicitly' (p. 44), a view that he takes to have been 'brilliantly developed' by Michael Polanyi. If tacit knowledge may be obtained this way, why not also 'tacit rules'?

specialties can be convincingly explained by referring to scientists' acceptance of such problem-solutions. However flimsy Kuhn's remarks about the priority of paradigms may be when considered as arguments, it is hard to fault his observation that 'one is at liberty to suppose that somewhere along the way the scientist has intuitively abstracted rules of the game for himself, but there is little reason to believe it' (p. 47).

Kuhn's remarks in Chapter V of *The Structure of Scientific Revolutions* are especially important since they reveal the weakness of the rationalist (or intellectualist) view of rules as prerequisites for coherent scientific practice. Once we acknowledge the possibility of paradigms directly determining scientific practice 'by resemblance and by modeling', we shall be much less prone to assume that there must be rules underlying well-conducted scientific inquiry. Indeed, given that paradigms may determine scientific research 'without the intervention of discoverable rules', the more plausible assumption would seem to be, as Kuhn observes, that they 'actually do operate in this manner' (p. 46). After all, what can possibly be gained by postulating the existence of rules governing scientific inquiry when we can get by just as well without them?

In particular, Kuhn's discussion alerts us to the precariousness of the conception of science as a rule-governed enterprise. When we defend this view, we are not defending the obvious; we are not merely summarizing scientific practice, still less making an uncontroversial observation about its essential nature. What we are doing is proposing a bold psychological hypothesis about how scientists (and presumably people in general) think. Viewed this way,Kuhn's discussion is especially important in that it brings out the magnitude of the task facing the proponent of the traditional view, it being far from easy to make sense of the idea of a hidden rule, to say nothing of justifying the idea that such rules guide and provide backing for practice.[6]

To put the point another way, what Kuhn shows – perhaps inadvertently – is that the apparent inexorability of the picture of science as a rule-governed enterprise is illusory. In directing our attention to the possibility of paradigms directly determining practice, he makes it difficult for us to fall into the trap of regarding the traditional way of conceiving scientific practice as a straigthforward empirical claim about it. In particular, given Kuhn's observations, it becomes clear that what is presented as the culmination of the analysis – namely the discovery that scientific inquiry is guided by rules – is in fact one of the preconditions of the analysis itself. After Kuhn, it is difficult to avoid the conclusion that the traditional view rests in large measure on non-existent empirical results (or, worse still, shaky metaphysical principles).

[6] Against those who would argue that such considerations have more to do with the philosophy of mind than the philosophy of science, I would argue that it is one of Kuhn's major achievements to have reinstated the link between these two areas of philosophical investigation. All too often in recent years, philosophers of science have proceeded without considering whether their suggestions are psychologically realistic. For the most part, they have simply taken it for granted that the philosphy of mind will fall into place once the philosophy of science has been sorted out.

In my view, Kuhn's main thesis in Chapter V of *The Structure of Scientific Revolutions* is not so much a thesis about priority of pardigms as one about the non-priority of rules. True, Kuhn does frequently treat paradigms as though they were explanatorily prior to rules, and some of his remarks suggest that he takes himself to be advocating a theory on a par with traditional theories based on rules. However, if what I am arguing here is correct, we would do better to think of him as being involved in the quite different project of reining in theoretical speculation and of bringing philosophical reflection down to earth. It is only by taking him to be challenging the need for a general theory of science (as opposed to advocating an alternative theory) that we can make any sense of his insistence on the inadequacy of traditional thinking about science. The point and force of what he says reside entirely in its opposition to the traditional philosophical viewpoint.

The present interpretation of Kuhn's position may be further clarified by noting that he refrains from embracing the common view that practice and skill are inimical to reason and deliberation. Contrary to what is often alleged, Kuhn does not hold that the rationalist conception of scientists following rules ought to be replaced by a nonrationalist conception of scientists pursuing social interests and responding to social pressures. His aim is not to subordinate reason to tradition but rather to remind us that reason is itself a particular (albeit rather special) tradition. His attack on the standard conception of science as a rule-governed enterprise is an attack on the philosophical, not the everyday, conception of reason. We should see him as recalling something that we already know, not as attempting to revise commonsense.

Also notice that Kuhn allows that scientists sometimes learn their trade by assimilating rules rather than by means of examples and practice. Kuhn is indeed on record as holding that 'scientists never learn concepts, laws, and theories in the abstract and by themselves' (p. 46). But he also admits that a student may be aided by 'definitions in his text' (although less so than 'by observing and participating in the application of these concepts to problem-solution') (p. 47). Remarks such as these will inevitably seem odd if we think of Kuhn as attempting to develop an alternative philosophical theory of science. As remarks about how scientists actually proceed, however, they are surely uncontroversial and very much to the point. What Kuhn means to emphasize is not that scientific expertise cannot but be learnt by examples and practice but that this is how it is – as a matter of fact – normally acquired.[7]

The main thing to remember here is that one can question the philosophical doctrine of the centrality of rules without asserting that rules are subservient to paradigms. Although Kuhn's choice of phrase often suggests that he aims to reverse traditional priorities, he is most plausibly interpreted as attempting to cast doubt on the usefulness of the traditional way of conceiving the problem.

[7] Here it is useful to distinguish between what are sometimes called rules of technique (e.g., rules concerning laboratory procedure) and the kind of rule that is traditionally held to underlie scientific practice. Nobody, least of all Kuhn, would deny that rules of technique can be prior to practice. Problems arise only when we take their existence to lend support to the idea of general rules of scientific inquiry.

His object is not to show that Being is prior to Thought and Action prior to Reason, but rather to get us to think of Thought as being a species of Being and Reason a species of Action. Instead of treating either conception as being subservient to the other, he holds that they stand and fall together. In short, as I read Kuhn, his primary aim is to cast doubt on traditional distinctions and to expose the theoretical poverty of the terms in which they are cast.[8]

We can, moreover, trace many of the more important themes of *The Structure of Scientific Revolutions* back to the views that Kuhn develops in Chapter V. Clearly, his treatment of scientific crisis in terms of institutional erosion coheres well with the thesis of the priority of paradigms, as does his picture of the establishment of new paradigms as resting on the 'increasing shift in the distributions of professional allegiances' (p. 158). For to the extent that custom and tradition play a role in scientific inquiry, it is to be expected that established scientific fields break down gradually and that new ones are established only with considerable difficulty (see Chapters VI, VII and VIII). Furthermore, it is reasonable to see Kuhn's views about the 'invisibility of revolutions' and the tendency of scientists to rewrite scientific history of deriving in large measure from his views about the nature of scientific expertise and its acquisition (see Chapter XI).

What is more problematic is the relationship of Kuhn's thesis of the priority of paradigms to his conception of science as being 'normal' or 'revolutionary'. In *The Structure of Scientific Revolutions,* Kuhn often views normal science as a matter of investigation governed by fixed rules and revolutionary science as involving the replacement of one set of rules (or conceptual scheme or world-view) by another. Especially in the second half of the book, he tends to focus on considerations having to do less with practice than with the conceptual, theoretical, methodological and instrumental rules that in Chapter V he treats as being subordinate to paradigms. His earlier insistence on skill and learning by example all too often falls by the wayside, and we are left with the familiar view of science as an enterprise governed by rules, albeit by different sets of rules at different times.[9]

Part of the problem here stems from the ambiguity of the notion of a paradigm. As Kuhn himself later recognized, the account of science provided in *The Structure of Scientific Revolutions* is unsatisfactory because it runs

[8] In this regard, Kuhn differs from Oakeshott. Although they mount similar arguments against the traditional view concerning the relationship of practice to rules, Kuhn rejects the terms of this view while Oakeshott (for the most part) accepts it and hence (mistakenly) concludes that practice has primacy in an exceptionally strong sense.

[9] Ironically, Kuhn comes close to advocating the type of position defended by many positivists when he reverts to treating science as a rule-governed enterprise. Compare in particular, what Kuhn says when he emphasizes rules with the position that Rudolf Carnap defends in his 'Empiricism, Semantics, and Ontology' (reprinted in his *Meaning and Necessity,* University of Chicago Press, Chicago 1956, pp. 205–221). One might certainly be forgiven for thinking that Kuhn's distinction between normal and extraordinary scientific inquiry recapitulates Carnap's distinction between inquiry devoted to answering 'internal questions' and inquiry devoted to answering 'external' ones. Moreover, it should be noted that Kuhn's treatment of paradigm replacement is (in some parts of *The Structure of Scientific Revolutions*) strikingly similiar to Carnap's treatment of the selection of what he calls 'linguistic frameworks'.

together the idea of a paradigm as an exemplary achievement and the idea of it as a disciplinary matrix.[10] However, this ambiguity can be eliminated without compromising the crucial lessons of Chapter V. For it can be argued that scientists belong to the same normal scientific tradition just in the event that they agree in their practice (compare p. 44). And revolutionary change can be regarded as a matter of the development of new research areas (and correlatively the development of new skills and way of learning). On this view, paradigms precede rules (and practice precedes theory) in both normal and extraordinary scientific inquiry.

Contrary to what Kuhn's opponents frequently allege, he cannot be criticized for assuming that science is both fundamentally tradition-bound and capable of revolutionary change. At first sight, his strong emphasis on the closed character of scientific traditions certainly does appear to be antithetical to the emergence of genuinely novel ideas (as Kuhn himself recognizes – see pp 66–67). However, this difficulty evaporates once the traditional view about the relation of thought to action is set aside, this view being all that prevents us from recognizing that tradition, far from precluding novelty, may be the very thing that brings it about. It is not by chance that the scientist's education and subsequent work in the profession are informed by a concern with problem-solving as deeply as they are. The requirement of originality is, after all, itself a fundamental scientific tradition.[11]

Finally, I should note that when Kuhn is interpreted in the way that I am suggesting, many of the criticisms that are commonly levelled against him no longer apply. There is undoubtedly some basis for the critic's puzzlement concerning Kuhn's claim to have found it difficult to apply the distinction between discovery and justification to the actual situations; and one can also understand their tendency to disparage his contention that nothing that he says runs counter to traditional views about the rationality, objectivity and progress of science.[12] Nonetheless, there is much to be said on Kuhn's behalf. Given his emphasis on the central role of paradigms in scientific research, both the question of separability of discovery and justification and the problem of the rationality of science appear in an entirely new light.

[10] See, e.g., the postscript to the second edition of *The Structure of Scientific Revolutions,* pp. 181–182.

[11] I would also argue that similar points can be made concerning Kuhn's analogy between scientific and political revolutions (see pp. 92–93), his comparison of revolutionary transformations with gestalt swiches (see p. 111), and his acceptance of 'the decision of the scientific group' as the criterion of progress (p. 170). In my opinion, these views are best considered from the standpoint adumbrated in Chapter V of *The Structure of Scientific Revolutions.* This way, we can retain the bulk of what Kuhn says without attributing to him obviously fallacious ideas.

[12] Compare Kuhn's remark that his 'attempts to apply [traditional distinctions concerning discovery and justification], even *grosso modo,* to actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic' (p. 9) and his report of being surprised when it was suggested to him that 'his biggest problem now [i.e., after the publication of *The Structure of Scientific Revolutions*] is showing in what sense science can be empirical' (T. S Kuhn, 'Reflection on My Critics' in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge,* Cambridge University Press, Cambridge, 1970, p. 263).

With regard to the first of these issues, note that Kuhn's concern with the genesis of scientific theories does not by itself show that he fails do distinguish properly between discovery and justification nor that he fails to appreciate the difference between epistemology and psychology. In stressing the importance of practice, Kuhn does not – as we have seen – mean to suggest that thought and intelligence play no role in science, his aim being to direct our attention to the actual ways in which scientists make and justify their discoveries. On his view, as I understand it, justification, like discovery, is something that individuals do, not a relationship between abstract propositional contents. We should not see his rejection of the standard assimilation of discovery to psychology and justification to epistemology as being in any way sinsister or confused. What he wishes to point out is simply that discovery and justification should be thought of as being simultaneously psychological and epistemological processes[13].

As for the rationality of science, Kuhn can – given the present interpretation – both acknowledge its importance and provide a plausible account of what it involves. For he can insist that the rationality of the enterprise resides in the central role that reason and criticism play in its development. Here it is important not to slip back into thinking of traditions as being unreflective, unreasoning and blind, and to remember that they can be supplemented, modified and even replaced in an organized and rational fashion. Once we reject the traditional division between Thought and Reason on the one side and Being and Action of the other, the picture of the one as intruding on the other no longer stands. Indeed, to insist that all talk about practice is psychologistic is simply to reassert the very rationalistic conception of rationality that Kuhn is attempting to circumvent.

I conclude, then, that the priority of paradigms deserves to figure prominently in the analysis of *the Structure of Scientific Revolutions*. Contrary to what is often supposed, there is still much to be learned from Kuhn's discussion of scientific research; we cannot afford to set it aside as being merely of historical interest. In particular, if we ignore what Kuhn says about the relationship of paradigms and rules, we shall miss the radical shift in the philosophy of science that he is advocating[14]. He is right to imply that one of the results of his discussion is 'a decisive transformation in the image of science by which we are now possessed' (p. 1).

[13] We should thus not be surprised that Kuhn sometimes speaks of the discovery of hypothesis as constituting part or even all of its justification. I discuss this general issue in some detail in 'The Process of Discovery', *Philosophy of Science*, 52, 1985, pp. 207–220.

[14] Compare the work of critics of Kuhn's views concerning rationality such as Imre Lakatos and Larry Laudan. I would argue that their attempts to improve on Kuhn's views are vitiated by their uncritical acceptance of the traditional picture of science as a rule-governed enterprise and their lack of interest in the issues that Kuhn raises under the rubric of the priority of paradigms.

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