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T. S. KUHN: FROM REVOLUTIONARY TO SOCIAL DEMOCRAT. KUHN AND THE IMAGE OF SCIENCE[*]

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Abstract. T.S. Kuhn's *The Structure of Scientific Revolutions* begins with the observation that our image of science might well undergo a complete transformation if we took a dispassionate look at the actual history of science. The image he has in mind is the one characterized in Chapter I in which the scientific community is pictured as the very paradigm of institutionalized rationality. On this picture the scientist disinterestedly applies his special tool, the scientific method, and each application takes him further on the road to truth. In making this observation Kuhn is not simply looking forward to his own conclusion that between the ideology of science and the realities of scientific practice there falls a vast shadow. Rather he is suggesting that mere reflection on the source of our image of science is likely to prompt the conjecture that the image is gravely distorted. For the vast majority of us acquire our image either through contemporaryscientific textbooks or through popular accounts of science the authors of which in turn derive their image from the standard texts. Such texts are designed to present contemporary scientific beliefs and techniques. In so far as we learn thereby anything about the history of science, it is through cleaned-up versions of past scientific triumphs.

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T. S. Kuhn's *The Structure of Scientific Revolutions* begins with the observation that our image of science might well undergo a complete transformation if we took a dispassionate look at the actual history of science. The image he has in mind is the one characterized in Chapter I in which the scientific community is pictured as the very paradigm of institutionalized rationality. On this picture the scientist disinterestedly applies his special tool, the scientific method, and each application takes him further on the road to truth. In making this observation Kuhn is not simply looking forward to his own conclusion that between the ideology of science and the realities of scientific practice there falls a vast shadow. Rather he is suggesting that mere reflection on the source of our image of science is likely to prompt the conjecture that the image is gravely distorted. For the vast majority of us acquire our image either through contemporary scientific textbooks or through popular accounts of science the

[*] Ch.V from *The Rationality of Science*, Routledge, London, 1981, p. 102-110.

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authors of which in turn derive their image from the standard texts. Such texts are designed to present contemporary scientific beliefs and techniques. In so far as we learn thereby anything about the history of science, it is through cleaned-up versions of past scientific triumphs. We learn nothing of the failures. We glean nothing about the state of science during its barren periods. And our grasp of the struggles that preceded the great moments of science derives more often than not from what the makers of these moments themselves said about the struggle.

If this is the source of one's image of science one ought to worry about its viability, just as one should be worried about one's image of the political process if that image was derived solely from, say, reading the memoirs of Wilson and Brezhnev. As we noted in Chapter I, two sorts of attack have been made on this image. The weak or boring attack is launched by one who accepts both that there is some special method and some ideal mode of applying it but who thinks that the actual practice of the scientific community falls short to a greater or lesser extent from what could be achieved. The strong or exciting attack, on the other hand, is waged by those who deny that there is any such defensible ideal with which actual practice can be compared. The investigations into the history of science, which Kuhn advises, lead him, initially at least, to embrace the exciting attack. However, in response to criticism Kuhn has so modified and altered or re-interpreted the position advanced in the first edition of *The Structure of Scientific Revolutions* that it is no longer clear whether a rationalist is committed to denying anything that Kuhn asserts. To begin with I shall be concerned with the earlier strong Kuhnian position, which deserves to be taken seriously (more seriously than Kuhn himself now appears to take it). For it articulates the most basic challenge to the rationalist perspective, a challenge which has yet to be met in full.

The model of science which Kuhn sees as emerging from a study of the history of science is to be explicated in terms of his notion of a paradigm. In his original essay Kuhn played fast and loose with this notion to the extent that one critic claimed to be able to discern twenty-two different senses in which the term was used[1]. Indeed, several critics have maintained that this free and easy manipulation of the notion nullifies the value of his work. For instance, Shapere writes: "Rather, I have tried to show, such relativism, while it may seem to be suggested by a half-century of deeper study of discarded theories, is a *logical* outgrowth of conceptual confusions, in Kuhn's case owing primarily to the use of a blanket term (i.e. paradigm). For his view is made to appear convincing only by inflating the definition of 'paradigm' until that term becomes so vague and ambiguous that it cannot easily be applied, so mysterious that it cannot help explain, and so misleading that it is a positive hindrance to the understanding of

[1] Masterman in I. Lakatos and A. Musgrave, *Criticism and the Growth of Knowledge*, Cambridge University Press,1970, p. 59-90.

some central aspects of science; and then finally, these excesses must be counter-balanced by qualifications that simply contradict them"[2].

Things are not quite as bad as that. For we can discern a way in which the term is used which makes it sufficiently precise to be potentially illuminating. This is what Kuhn now refers to as a disciplinary matrix. If we identify a scientific community in terms of, say, the subject of its investigations, the behaviour of bees or the evolution of the large-scale features of the universe, we should expect to find a considerable number of things held in common by the members of the community. Kuhn's talk of a paradigm is meant to direct our attention to those common factors, reference to which is required in explaining the behaviour of the scientists: "What do its members share that accounts for the relative fulness of their professional communication and relative unanimity of their professional judgments?"[3]

The particular things that Kunn wishes to isolate through the notion of a paradigm include the following[4]:

(i) Shared symbolic generalizations

This is meant to cover the basic theoretical assumptions held in common which are 'deployed without question'[5]. For instance, cosmologists may agree in accepting the field equation of the General Theory of Relativity. This aspect of a paradigm is comparable to Lakatos's notion of the 'hard-core' of a *SRP*. Unhappily, Kuhn goes on to suggest that 'these symbols and expressions formed by compounding them are uninterpreted, still empty of empirical meaning or application'[6]. While there may be problems involved in specifying what such generalizations actually mean, for reasons given in our discussion of instrumentalism, we cannot regard them as ever being uninterpreted.

(ii) Models

Agreement on models may be agreement either that a particular analogy, say, between electric circuits and steady-state hydrodynamical systems, provides a fruitful heuristic to guide research, or that certain connections should be treated as identities (i.e., the identification of heat with molecular motion).

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[2] D. Shapere, „The Structure of Scientific Revolutions', *Philosophical Review* LXXIII, 1964, p. 3-16.
[3] T.S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. Chicago University Press, 1970, p. 182.
[4] See Kuhn, 1970, 1974 and his papers in Lakatos and Musgrave, *op. cit.*1970 and in F. Suppe, *The Structure of Scientific Theories*, Chicago: University of Illinois Press, 1974.
[5] T.S. Kuhn, *The Essential Tension*, Chicago University Press, 1977, p. 297.
[6] *Ibidem*, p. 299.

(iii) Values

Kuhn takes it that the members of the scientific community will agree that theories ought so far as possible to be accurate, consistent, wide in scope, simple and fruitful. While the label 'value' is perhaps unfortunate, it must be agreed that these features are standardly regarded as good-making qualities of theories and that this agreement is important in determining the particular theory choices made by the scientific community.

(iv) Metaphysical principles

A scientific community will agree on certain untestable assumptions which play an important role in determining the direction of research[7]. As an example one might cite a preference for field theories over particle theories. Such principles would have affinities with Lakatos's notion of a positive heuristic on its minimal construal.

(v) Exemplars or concrete problem situations

What Kuhn has in mind is the agreement one finds within a scientific community on what constitutes the nice problems in the field and on what constitutes their solution. Among the ways such agreement is displayed is in the questions set out at the end of chapters in standard texts. It is also meant to include the consensus on what are the significant unsolved problems, as indicated in the research projects set for graduate students and in the agreement as to what constitutes a worthwhile thesis.

This latter notion of a paradigm as a shared example was the genesis of Kuhn's full notion of a paradigm, or as he now prefers to call it 'a disciplinary matrix'[8]. The notion of a shared example derives from his observation that we cannot give a rule specifying necessary and sufficient conditions for the applications of even simple, observational predicates such as '...is a swan'. Often we acquire a grasp of the sense of a predicate through the realization that certain objects constitute paradigm cases of the instantiation of the predicate. We acquire the ability to recognize other objects as being like the paradigm objects in the appropriate respect and apply the predicate to them. Epistemologically the exemplars of the predicates are prior to any rules for their application. For we can certainly apply predicates without being able to articulate the rules governing their application. In fact, it may not be possible, even having acquired the use of a predicate, to specify its sense in this way. This point about the application of

[7] Kuhn, *op. cit.*, 1970, p. 184.
[8] Kuhn in Suppe, *op. cit.*, 1974, p. 482.

predicates is both familiar and not particularly contentious. Kuhn seeks to extend the general idea to more sophisticated predicates than '...is a swan'. For instance, the notions of a successful scientific practice or a significant problem or a successful solution to such a problem are taken to be notions the application of which is grasped through exemplars or paradigms without prior or even post specification of rules giving the necessary and sufficient conditions of, say, solutions being successful. These 'shared examples can serve cognitive functions commonly attributed to shared rules'[9]. Kuhn is right in maintaining that there are no such rules available for these sorts of notion. However, there is a danger in assuming too easily that this is the case. For an endeavour to search for rules may reveal some necessary and some sufficient conditions, and the articulation of these may be of considerable interest even though we cannot produce rules which specify conditions which are jointly necessary and sufficient. His point remains that an explicit grasp of these partial rules is not a precondition of the application of the predicate. In the end it is not clear just how much light this casts on the nature of the scientific enterprise since it has nothing particular to do with the science. As we noted, Kuhn was led to introduce the particular term 'paradigm' in the context of considering the application of predicates. It was, therefore, misleading (as he now acknowledges) to extend that term to cover the other four ingredients noted above. Kuhn also employs the notion of an exemplar in giving his account of the meaning of scientific terms: 'The process of matching exemplars to expressions is initially a way of learning to interpret the expressions.'[10] Through the positive account of the meaning of scientific terms to be given in Chapter VII we shall see that this will not do as the basis of a satisfactory theory of meaning.

The positive and salutary virtue of Kuhn's use of his notion of a paradigm is to remind us that in looking at the scientific enterprise it is important to focus on more than the theories (in the narrow sense of the term) advocated within a given community. The danger in using the notion is that we may be led to view the history of science as a sequence of discrete, clearly demarked, paradigms. The notion is far too vague and imprecise for this. Given his own characterization it simply will not do to say that 'despite occasional ambiguities, the paradigms of a mature scientific community can be determined with relative ease'[11]. Notwithstanding this danger, it remains a useful term. For, in general, it directs our attention to the fact that in understanding the scientific enterprise we must look not only at theories proper but also at a wider range of beliefs, attitudes, procedures and techniques of the scientific community. In particular it reminds us that in explaining the replacement of one theory by another it is essential to look

[9] *Ibidem*, p. 482.
[10] *Ibidem*, p. 517.
[11] Kuhn, *op. cit.*,1970, p. 43.

at this wider nexus and its evolution. However, as noted above, in view of the absence of any associated criterion of individuation we cannot think of the term as identifying any delimited class of particular items. This severely limits its utility as a term of art within the history and sociology of science. It is too vague a term to allow us to ask questions as to why one particular paradigm gives way to another or to seek to devise laws or general theories about paradigms. Kuhn unfortunately writes as if it were a technical term capable of utilization in this way. Interestingly, he himself finds no need to use the term in his recent historical study of the origins of Quantum Mechanics.[12] And, more seriously, as we shall see, Kuhn holds a number of untenable theses about this only vaguely delimited phenomenon of paradigm change.

Revolutions

Kuhn characterizes a period of time during which a particular scientific community shares a paradigm as a period of normal science. During such a period the energies of the members of the community are given over to solving puzzles defined by the paradigm, which is itself based on some significant scientific achievement. Of course, given the vagueness of the notion of a paradigm, we cannot suppose that there are clearly defined periods of normal science. However, it remains true that there are periods in which there is a high degree of agreement, both on theoretical assumptions and on the problems to be solved within the framework provided by those assumptions. During such times the faith in the underlying theory is such that anomalies are not treated as refuting the theory but are treated as puzzles to be solved. In time there may be a growing number of unsolved puzzles and anomalies, as a result of which the community's confidence in its theory is eroded. This crisis of confidence means that the agreement which constitutes the sharing of the paradigm begins to break up and attempts are made to articulate alternative theoretical structures.

At this juncture Kuhn introduces the notion of scientific revolutions making an explicit analogy to political revolutions. Kuhn sees a situation as revolutionary in a political sense if an ever-increasing number of persons feel sufficiently estranged from the political process itself to wish to change that process as currently institutionalized. Similarly, a growing set of anomalies generates an awareness of the constraining character of the paradigm and this leads some to articulate a new paradigm to put in place of the old. In the political case Kuhn remarks that there is a difference between the means standardly used in seeking change in non-revolutionary situations and the means used in revolutionary situations. By and large, the individuals in the non-revolutionary situation agree

[12] Kuhn, *op. cit.*, 1978.

on the principles which are to govern decision making. In a revolutionary situation, agreement has broken down and an attempt is made to restructure by force the society in order to create a new framework for decision making. Kuhn is quite explicit in invoking this feature of the analogy. For in times of normal science there is agreement on the problems and agreement on what constitutes a solution. Kuhn thus corrects the simplistic Popperian model of science which tends to represent any experiment as a possible definitive test of a theory. Speaking of the scientist during a period of normality he says: "If it fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than the current theory which is tested."[13]

That there are periods in which experiments are seen in this light is clearly illustrated by noting our attitude to a student who in doing a routine experiment in a laboratory course gets a result at odds with the predicted result. The thought that the fault lies with the theory and not with him or his equipment is not even entertained. In the case of revolutionary science, on the other hand, the proponents of two competing paradigms face each other over the barricades without agreement on the principles governing the choice between paradigms. The views that I have attributed to Kuhn can be construed as plausible, if somewhat obvious, sociological generalizations about the behaviour of the scientific community. As such their evaluation requires an examination of both the history of science and current scientific practice. At this juncture, however, Kuhn articulates a largely philosophical thesis to which historical and sociological factors are largely, if not entirely, irrelevant. At first glance the thesis looks as though it might be just another sociological claim: "As in political revolution, so in paradigm choice – there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and of logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists"[14]. This might be construed as the innocuous claim that propagandizing plays a role in the process of changing allegiances from one paradigm to another, which is something even rationalists can admit. However, Kuhn maintains not just that propagandizing plays a role but that nothing but propagandizing can play a role: "The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before."[15]

[13] Lakatos and Musgrave, *op. cit.*, 1970, p. 5.
[14] Kuhn, *op. cit.*, 1970, p. 94.
[15] *Ibidem*, p. 102.

That is, since the theories embedded in rival paradigms simply cannot be compared, there is no possibility of providing a rational explanation of scientific change. For in the case of incommensurable theories there are no objective theory-neutral principles relative to which the theories can be compared.

Kuhn offers Newtonian and Einsteinian mechanics as a specific example of incommensurability. Indeed, he rejects the standard derivation of Newtonian mechanics as a limiting case of Einsteinian mechanics for velocities low with respect to that of light as spurious on the grounds that the terms in the equations resulting from the derivation differ in meaning from the terms in the Newtonian theory. For in this derivation these terms are defined by reference not to the Newtonian concepts but to the Einsteinian concepts. We noted in Chapter I that the source of this doctrine of meaning variance lies in the positivistic and neo-positivistic holistic conceptions of the meaning of theoretical terms which give rise to the thesis of radical meaning variance, *RMV*. Kuhn does little to argue for this conception and simply assumes it to be correct. One cannot under-estimate the startling character of what we are consequently asked to accept. It means, for example, that there is no logical contradiction between Newton's assertion that simultaneity is not relative and Einstein's assertion that simultaneity is relative. It is ironic that someone who has urged us to take the actual practice of science seriously should be led to this conclusion. For this is certainly not how the scientific community views the Einstein-Newton controversy! In point of fact this is taken to be genuine head-on confrontation that does not represent a mere apparent incompatibility deriving from equivocation in the meanings of the crucial terms. In view, then, of these startling and unpalatable consequences we have every reason to refuse to accept the doctrine until we are presented with forceful arguments on its behalf. Since Kuhn has not offered the arguments for incommensurability due to *RMV*, we will defer further consideration of the case that can be made until Chapter VII. Kuhn's particular contribution has been to draw our attention to the surprising fact that a prima facie attractive theory of meaning leads to the consequences it does.

Given *RMV*, the problem of rationally comparing rival theories simply does not arise. Kuhn himself has remarked on this in the context[16] of withdrawing from his earlier more extreme position so as to allow for the possibility of partial communication between the proponents of competing paradigms. Since Kuhn no longer holds that extreme thesis of *RMV*, we have to look to his other reasons for likening the transitions between paradigms to a process of conversion or gestalt shift rather than to a rule-governed investigation which terminates in the grounded judgment that one paradigm is more justified than another. That is, even if we assumed invariance of meanings we would find, according to Kuhn, that in revolutionary periods there is a change in the standards of evaluation.

[16] *Ibidem*, p. 198-9.

Kuhn's account of what it is that changes when such standards change is obscure. At times he talks of paradigm shifts as bringing about 'changes in the standards governing permissible problems, concepts and explanations'[17]. If it were the case that the very criterion of what constituted a good explanation changed radically as one paradigm replaced another, and if we lacked any paradigm neutral standard for evaluating criteria of explanation, we would have a problem. However, Kuhn does nothing to establish such an incommensurability between paradigms which we might call incommensurability due to radical standard variance. Indeed, he does not even show that the conception of what constitutes a good explanation has varied in the history of science. What is cited in justifying the claim that standards of explanation vary supports a quite different thesis. For instance, he says that the transition in the seventeenth century from the conception of gravity as having a mechanical explanation to the conception accepted by the mid-eighteenth century of gravity as being innate (and hence inexplicable) represents a shift in the standards of explanation[18]. But this undoubted transition is a transition in beliefs about what can be explained. There is no reason to think that it represents a change in the very criterion of what counts as a good explanation. It will be argued that there are shifting conceptions of what constitutes an explanation, but that since there are rational considerations relevant to assessing these conceptions we do not have any reason to think that incommensurability due to radical standard variance is a real problem.

Kuhn offers another and more forceful reason for thinking that there may be problems involved in comparing theories across paradigms. Rightly remarking that we cannot choose between theories simply by reference to the number of problems they solve, because no paradigm ever solves all its problems and no two paradigms leave the same problems unsolved, he remarks that paradigm debates "involve the question: which problems is it more significant to have solved? Like the issue of competing standards, that question of values can be answered only in terms of criteria that lie outside of normal science altogether, and it is that recourse to external criteria that most obviously makes paradigm debates revolutionary"[19].

If we formulate this problem of the significance of solved problems at a level of great generality it can look a very real problem. Suppose that theory T_1 solves a problem P_1 but not P_2 and that theory T_2 solves P_2 but not P_1. Let us imagine that the proponents of T_1 think that P_1 is significant and that P_2 is not, and vice versa for the proponents of T_2. What are we to do? We do not have any readily available criteria for assessing the significance of problems. This is not to say that we may not agree on some general considerations relevant to making such

[17] *Ibidem,* p. 104.
[18] *Ibidem*, p. 105.
[19] *Ibidem*, p. 110.

judgments in certain cases. For instance, if one party can point to the fact that the solution of one problem paves the way for further fruitful work, those who hold that the other problem is more significant should be expected to justify their position by showing either that this work is unlikely to be fruitful or that their own solution gives rise to further work which is at least equally fruitful. It may be that in the end we have to say that as things stand there is no reason to think that one problem is more significant than the other. It is none the less highly unlikely that this will generate a total stalemate. For it would be rare indeed if the only relevant factor at stake in the choice between competing theories turned on the question of the significance of their solved problems. Perhaps one theory generates a host of false predictions which the other does not. As an actual problem the problem of the significance of problems is not imposing. For it is unlikely that there will be many cases where the choice will rest entirely on unsettable judgments of significance. If it does in some cases this need not disturb the rationalist. For he ought to hold that sometimes the most rational thing is to suspend both belief and disbelief. If it is simply a difference as to significance, he ought to encourage the development of both theories with the reasonable expectation that some other more tractable difference will emerge.