

Testing Paradigms

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To what extent is scientific development rational? How important are nonrational and irrational factors in explaining scientific changes in belief in theories? In the influential book *The Structure of Scientific Revolutions*¹ Thomas Kuhn argues for the thesis that there is almost no testing in "normal science." Here is one of several places in which Kuhn advances it: "The scientific enterprise as a whole does from time to time prove useful, open up new territory, display order, and test long-accepted belief. Nevertheless, *the individual* engaged on a normal research problem is almost never doing any one of these things" (SSR, p. 38). Since most of the history of science consists in what Kuhn calls "normal science," this means that there is almost no paradigm testing in science, period. Thus, Kuhn's argument is an essential part of his case for the further conclusion that rational factors play only a minimal role in widespread changes in belief in and allegiance to various theories. These changes, according to Kuhn, are more comparable to religious conversion or choice of political affiliation than to rational consideration of evidence and argument. I consider these two propositions to be two sides of the same coin. Together they form a thesis about the proper characterization of scientific development. If I wish to emphasize the minimal role of rational factors in depicting Kuhn's thesis, I shall call it "the minimalist doctrine," whereas I shall call it the "religious/political doctrine" if I wish to stress the large role of nonrational and irrational factors and the comparison to religious conversion and choice of political affiliation. It is clear that nonrational and irrational factors have played some role, along with rational ones, in the transfer of belief from one dominant theory to another. To decide whether Kuhn's thesis is *true*, then, would require both some clarification or explicit quantification of *how much* each kind of factor plays and extensive

knowledge of the details of the history of science. But the issue I wish to address here is both less ambitious and (fortunately for me) nonempirical. Has Kuhn proved his "minimalist" and "religious-and-political" doctrines? Here I intend to argue that Kuhn has not proved either conclusion with his two major arguments, which I call the "neutral perception" and "facts-fit-theory" arguments.

Since Kuhn makes his case in terms of features of what he calls "paradigms," my reply requires some attention to the meaning of that term. Kuhn defines it early. He notes that some important scientific works, such as Aristotle's *Physics*, Newton's *Principia*, and Lavoisier's *Chemistry* are paradigms, as they "implicitly ... define[d] the legitimate problem[s] and methods of a research field for succeeding generations and practitioners [and they did so] ... because they shared two essential characteristics: (1) their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing ways of doing science; and (2) they were sufficiently open-ended to leave all sorts of problems for the group of adherents to resolve" (SSR, p. 10, close paraphrase).² Since scientific theories are propounded in the works Kuhn mentions and since these open-ended theories serve as a basis for research, I shall at times use the term "dominant theory" instead of "paradigm" to stress that paradigms dominate or hold sway over the thinking and research of their advocates.

Kuhn probably would object that this characterization glosses over an important aspect of his concept of a paradigm. Indeed, in the "1969 Postscript" to the second edition, Kuhn admits that he uses the term "paradigm" in two different senses: "On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science" (SSR, p. 175). This second sense of "paradigm"

appears to mean roughly “paradigm case,” or paradigm cases that guide scientific research, such as the example of Pavlov’s dog in psychology.

This ambiguity in the term “paradigm” makes it difficult to determine just what Kuhn means when he claims that there is almost no paradigm testing in normal science. If “paradigm” is construed to mean “paradigm case,” then it is not altogether easy to see how there would be any testing of paradigms in normal science. How would you test *an example* designed to illustrate certain propositions? I suppose someone might try to replicate the conditions that (allegedly) obtained in the example, and the possibility of failure to reproduce the results would constitute a test of the example.

But if all that Kuhn means by the thesis that there is almost no paradigm testing in normal science is that scientists generally take for granted the textbook or paradigm cases that are used to illustrate major claims, then his claim is not inconsistent with the contention that the shift of allegiance from one theory to another is largely based on rational considerations, principally on the basis of evidence and good argument. Why? Because a failure to test paradigm *examples* is consistent with there being many tests of the *propositions* that those examples are supposed to illustrate. Thus, this interpretation of Kuhn’s thesis takes the irrationalistic “bite” out of it.

Besides, there is some reason to reject this interpretation. When Kuhn writes of changes of allegiance from one paradigm to another being comparable to religious conversion, after all, he does not mean allegiance to an *example*, but to a dominant *theory* that, in conjunction with values and examples, defines a way of doing science. Hence, even if paradigm examples play a role in Kuhn’s “minimalist” and “religious/political” doctrines, they are secondary to the primary role played by theories.

How does Kuhn defend the “minimalist” and “religious/political” doctrines? Kuhn writes that in science there is no standard higher than agreement among the scientific community (SSR, p. 94). Why

should we believe that? We ordinarily think that science differs from religious conversion and choice of political affiliation precisely in that agreement among scientists grows out of consideration of *the evidence*. But what is “the evidence?” It is there that Kuhn finds problems. Here is Kuhn’s “neutral perception” argument:

1. If choice between competing paradigms is fundamentally rational – that is, is more than a matter of agreement or persuasion – then paradigm choice must be (rationally) based on the evidence.
2. But if paradigm choice were (rationally) based on the evidence, then there must be an external standard for distinguishing between seeing (paradigm-free perception) and seeing-as (paradigm-based perception).
3. However, perception is inherently paradigm-based. (Kuhn supports this claim with examples, including examples of anomalously-marked cards, inverting goggles, and duck/rabbit.) (SSR, p. 113)
4. Moreover, paradigms determine what to look for, what counts as an explanation, problems to be solved and methods to be used.
5. Thus, there is no external standard for distinguishing seeing from seeing-as [from 3 and 4].
6. Therefore, if choice between competing paradigms is fundamentally rational – is more than a matter of agreement or persuasion – then there must be an external standard for distinguishing seeing from seeing-as [from 1 and 2].
7. Therefore, the choice between competing paradigms is fundamentally not rational – is no more than a matter of agreement or persuasion. In other words, the religious/political thesis is true [from 5 & 6].

This argument is the core idea in Kuhn’s criticisms of both falsi-

fication and probabilistic confirmation theories, the two major kinds of theories according to which scientific development is more rational than Kuhn claims. Kuhn contends that both presuppose paradigm-free perception of the evidence and a neutral language in which to describe it. Only thereby could we reach a rational decision about the relative merits of competing paradigms. But there is no paradigm-free perception or language.

There is something to what Kuhn says. The example of the anomalously-marked cards shows that our perceptions are to a large extent explained by our expectations. And Kuhn thinks that scientists' expectations are a function of which paradigm they endorse. The duck/rabbit example illustrates how data that are (in some sense) "the same" can be interpreted differently. Again, Kuhn thinks that the paradigm one accepts explains why one (arbitrarily) adopts one of a number of possible interpretations.

But has Kuhn proven that the choice between competing paradigms is no more than a matter of agreement or persuasion? I think that the answer is "no." When Kuhn refers to a lack of an external standard for distinguishing seeing from seeing-as, Kuhn means perception and language that are neutral.

1. But Kuhn's claim that there is no such thing as neutral perception or language is true only if he means *absolutely* neutral.

True enough, it is difficult or impossible to imagine how one would describe what one observes in a way that is free from *all* sorts of anticipatory ideas.

2. However, perception and language that are neutral *with respect to the competitors* are all that are needed in order to decide between competing theories on rational grounds. Absolutely neutral perception and language are not necessary.

For example, if we try to judge the relative merits of Ptolemy's and Copernicus's theories, it will not do to describe sunrises as literally cases of the sun rising or as the result of the earth's rotation. However, we could describe those events as "the sun appearing over the horizon."

3. Even if a paradigm determines what to look for and the problems to be solved, that is still consistent with there being competitor-neutral "facts" for which all the competing theories must account.

And failure to account for such facts would be a rational reason for preferring a competitor. Even if we accept Kuhn's premise that paradigms define what to look for and problems to be solved, it does not follow that a dominant theory is insulated against all external criticism. (By "external" I mean either objective shortcomings of a theory or criticism from those who do not accept a dominant theory.) After all, competing theories can (and sometimes do) direct us to look for many of the same things and define similar problems that permit a comparison and contrast. For example, consider what Russell says about the shift from Newtonian mechanics to Einstein's relativity theory: "The reasons for accepting Einstein's law of gravitation rather than Newton's are partly empirical ... Einstein's law of gravitation gives very nearly the same results as Newton's, when applied to the calculation of the orbits of the planets and their satellites. If it did not, it could not be true, since the consequences deduced from Newton's law have been found to be almost exactly verified by observation. When, in 1915, Einstein first published his new law, there was only one empirical fact to which he could point to show that his theory was better than Newton's. This was what is called the motion of the perihelion of Mercury." (Bertrand Russell, *The ABC of Relativity*).

4. In the "Postscript" to his book Kuhn appeals to competitor-neutral perception (which he calls "stimuli").

"The men who experience such communication breakdowns must, however, have some recourse. The stimuli that impinge upon them are the same. So is their general neural apparatus, however differently programmed. Furthermore, except in a small, if all-important, area of experience even their neural programming must be very nearly the same, for they share a history, except the immediate past. As a result, both their everyday and most of their scientific world and language are shared. Given that much in common, they should be able to find out a great deal about how they differ. The techniques required are not, however, either straightforward, or comfortable, or parts of the scientist's normal arsenal" (SSR, p 201). It appears that Kuhn acknowledges that the agreement among scientists is not a matter of mere agreement.

5. Kuhn contradicts himself by noting that there often are some events that can be described in a neutral way; and these are events that militate against one theory and favor its competitor.

Kuhn writes: "But each language community can usually produce from the start a few concrete research results that, though describable in sentences understood in the same way by both groups, cannot yet be accounted for by the other community in its own terms" (SSR, p. 203). Thus, Kuhn admits that the persuasion of scientists to accept one theory and reject another is not usually empty persuasion – that is, persuasion where the evidence is lacking. Thus, changes in allegiance to theories can, by Kuhn's own admission, be a matter of *rational* persuasion.

Kuhn does say that arguments that are stated in a vocabulary that

both groups use in the same way "are not ... *usually* decisive, at least not until a very late stage in the evolution of the opposing views" (SSR, p. 203).

6. But this is the admission that, even when the descriptions are not decisive, they can advance the rational evaluation of competing theories.
7. Furthermore, advocates of the thesis that scientific consensus grows largely out of rational consideration of evidence are not committed to the belief that neutral descriptions of events are decisive at any particular point.
8. Therefore, Kuhn's argument does not prove the religious/political thesis.

Kuhn says that there is almost no dominant-theory testing in "normal science." What further reasons does he give? Here is what I call his "facts-fit-theory" argument:

1. One task of normal science is to make theory fit fact better, a task that might seem to be testing.
2. But the motivation is to solve a puzzle that is assured of a solution (p. 80).
3. If things do not turn out the way the theory predicts, normal scientists blame themselves, rather than the theory (p. 80).
4. If the theory cannot be blamed, if the theory could not be wrong, then the theory is not tested.
5. Therefore, there is almost no dominant-theory testing in "normal science."

My reply is:

1. If Kuhn meant that scientists *always* treat paradigms as

immune from falsification, then that would contradict what he says elsewhere. He would have no way to explain dissatisfaction with an existing paradigm.

2. If Kuhn did mean that, then his claim would contradict scientific practice. Scientists do not treat dominant theories as *immune* to falsification.
3. If Kuhn replied that when dominant theories are treated as fallible, that is not "normal science," then Kuhn would be treating the proposition, "There is almost no theory testing in normal science" as if it were necessarily true by definition, rather than a contingent and empirical proposition.
4. That option (#3) is not open to Kuhn.
5. Thus, Kuhn does not mean that scientists always treat paradigms as immune from falsification, but that they are treated as relatively resistant to falsification.

Does treating dominant theories as relatively resistant to falsification show that science is non-rational or irrational?

1. The more evidence there is for a theory, the more reasonable it is, when faced with disconfirming evidence, to blame one or more of the auxiliary assumptions or to use one or more *ad hoc* hypotheses.
2. The same is true the less evidence there is against a theory.
3. Thus, treating a theory as relatively resistant from falsification can be *rational*.

I have an account of scientific activity to offer as an alternative. I consider it to be an alternative to Kuhn's account because I believe that the social and psychological factors that play a role in the development of science often are not opposed to rational consideration of evidence. I do not have any controlled empirical support for my

claims; so I should perhaps remain silent.

So called "normal science" (as well as "extraordinary science") is recorded in journals. There are less-than-noble social and psychological motives that explain why scientists try to get their work published: salary increases, prestige, grant money, and power. Despite these extrascientific or extrarational motives, as a group scientists still manage to obtain rationally defensible results. How? A typical scientific article includes not only the data and the conclusions reached, but a description of how the data were gathered (the methods used). This is so that other scientists, if they wish, can try to replicate *or* confute the results. Scientists are thus motivated not to rush into print, for they are running the risk of having an embarrassing blunder pointed out in print, a blunder that could undermine the all-too-human goal of getting a raise, more prestige, etc. On the other hand, they do not want to wait too long, for they do not want someone else to upstage them. Scientists can enhance their reputations by pointing out that some ideas are questionable or false. Thus the extrarational motives help to explain rational activity.

Every time a scientist shows that a claim is questionable or false, evidence has been gathered that *could* militate against the claim. And if so, then that claim has been *tested*. And this is to concentrate only on those cases where the evidence is unfavorable. Consider all those cases where the evidence *could* have been unfavorable, but turns out to support the claim. There, too, claims are *tested*. I think that the two types of cases I just described are commonplace, although I do not have empirical findings to support my claim.

Kuhn would doubtlessly retort that this testing is not *of* a paradigm, since it consists of tests of relatively unimportant claims that are *within* a dominant theory. The most fundamental propositions are not often tested.

My reply is that since a paradigm is a dominant theory, even tests of secondary propositions within that theory are paradigm tests. Besides, there is good reason why the most fundamental propositions

are not often tested, namely, that there is so much evidence to recommend them at the time. That is why a theory becomes paradigmatic in the first place. Therefore, even a lack of explicit testing of the most fundamental propositions that comprise scientific theories can be explained in rational terms. Thus, even if there were little testing of the most fundamental propositions of dominant theories, the explanation could still be that this is a consequence of the *rationality* of scientific development. We are not obliged to accept Kuhn's claims that nonrational and irrational factors, *rather than* rational ones, (mostly) produce widespread changes in belief in dominant theories. Kuhn has not proven the minimalist or religious/political doctrines.³

Notes

1. Thomas Kuhn, *The Structure of Scientific Revolutions* (second edition), Chicago: University of Chicago Press, 1970. Hereafter: SSR.
2. Kuhn adds that achievements that share these two characteristics are what he calls "paradigms."
3. Kuhn presupposes, of course, that the choice of religious or political affiliation largely involves nonrational and irrational factors, a presupposition I have not challenged here because I wanted to concentrate on what he says about *science*, rather than religion or politics.