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THE PARADIGM PARADIGM AND RELATED NOTIONS

"There is, in addition, a second reason for doubting that scientists reject paradigms because confronted with anomalies or counterinstances. In developing it my argument will itself foreshadow another of this essay's main theses. The reasons for doubt sketched above were purely factual; they were, that is, themselves counterinstances to a prevalent epistemological theory. As such, if my present point is correct, they can at best help to create a crisis or, more accurately, to reinforce one that is already very much in existence. By themselves they cannot and will not falsify that philosophical theory, for its defenders will do what we have already seen scientists doing when confronted by anomaly. They will devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict. Many of the relevant modifications and qualifications are, in fact, already in the literature. If, therefore, these epistemological counterinstances are to constitute more than a minor irritant, that will be because they help to permit the emergence of a new and different analysis of science within which they are no longer a source of trouble. Furthermore, if a typical pattern, which we shall later observe in scientific revolutions, is applicable here, these anomalies will then no longer seem to be simply facts. From within a new theory of scientific knowledge, they may

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> instead seem very much like tautologies, statements of situations that could not conceivably have been otherwise."[1]

It is central to the historical approach to the philosophy of science, which emerged in the late 1950s and early 1960s, that scientific knowledge is built on some set of accepted principles, theories, or techniques. This thesis involves a radical departure from traditional views of the nature of science. One of the oldest themes of the theory of knowledge, one which goes back at least to Plato's *Meno*, is that the pursuit of knowledge requires the availability of some previous knowledge, and one of the epistemologist's major concerns has always been to clarify the source of that first knowledge and provide its justification. Plato's theory of recollection, Aristotle's doctrine of intuition, the innate ideas of rationalists, the pure impressions of empiricists, and Kant's transcendental idealism are all attempts to provide the foundation on which further knowledge can be constructed. Moreover, all of these attempts to find the basis of knowledge were carried out under the aegis of a single accepted belief—that scientific knowledge must not be founded on accepted beliefs. This belief has controlled the search for an indubitable foundation for knowledge, and the thesis that, if knowledge is achievable, then there must be indubitable first principles, has been shared by sceptic and antisceptic alike. Proponents of a new philosophy of science, however, not only deny that there is any sure foundation for the development of scientific knowledge, but, more radically, they deny that any foundation is required. The resulting foundationless knowledge will, of course, be thoroughly tentative and always subject to revision, but advocates of the new approach do not find this conclusion to be epistemically pernicious; on the contrary, they have taken great pains to document the claim that such revisions have occurred, and to give reasons for expecting that they will recur in the future. This new understanding of the nature of scientific knowledge has brought along with it a new understanding of the aim and structure of the philosophy of science, and my aim in this paper is to review some of the main

[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd edition Chicago, Univ. of Chicago Press, 1970, pp. 77-78.

reasons for holding that science is built on accepted beliefs, and then develop the implications of this view for the philosophy of science.

I. The Role of Background Beliefs in Science

There is a strange tension in logical empiricism between the thesis that scientific claims are, at best, inductively supported hypotheses which are not and could not be indubitably proven, and the thesis that science really does establish truths which are not subject to revision. For example Hempel and Oppenheim[2] and Nagel[3] have held that it is a necessary condition for an explanation that the premises be *true*, a claim which requires one who holds that science does, now, explain, to maintain that at least some scientific theories are true. Nagel, in particular, follows out the logic of this position, holding that some scientific claims must be true since the notion that science has not yet explained anything is absurd, but that we do not know which claims are true since, given the limits of scientific method, no hypothesis can be proven true;[4] the procedures that allow us to establish that some part of current science is true without establishing the truth of any particular claim are not discussed.[5] In recent years historians and historically oriented philosophers of science have been pressing logical empiricists to take the consequences of their own logical analyses much more seriously than they often have, and the response has been, at times, surprising. Consider, for example, the following remark by Feigl:

> As Reichenbach pointed out a long time ago, science progresses by successively "securing" its various knowledge claims. For

[2] Carl Hempel and Paul Oppenheim, "Studies in the Logic of Explanation." In C. Hempel, *Aspects of Scientific Explanation*, New York, Free Press, 1965, pp. 248-249.

[3] Ernest Nagel, *The Structure of Science*, New York, Harcourt, Brace & World, 1961, pp. 42-43.

[4] *Ibid.*

[5] Cf. Harold I. Brown, *Perception, Theory and Commitment: The New Philosophy of Science*, Chicago, Univ. of Chicago Press, 1979, pp. 60-66 for further discussion.

> example, the optics of telescopes, microscopes, spectroscopes, interferometers, etc., is indeed presupposed in the testing of astrophysical, biological, etc., hypotheses. But these presuppositions—while, of course, in principle open to (in rare cases actually in need of) revision—are comparatively so much better established than the "farther out" hypotheses that are under scrutiny.[6]

One cannot help but wonder why Feigl brushes off the *logical point* that presupposed results are in principle open to revision in favor of the presumed historical fact that such revisions are rare. Earlier in this essay Feigl engages in a similar shift of viewpoint when he suggests that we should consider low level empirical laws to be at the heart of science since these are much more stable than theories. From a logical point of view, even low level generalizations are uncertain, but Feigl suggests that empirical laws "represent the most precise and the most reliable knowledge mankind has yet attained,"[7] and he seems to be prepared to defend this claim on the basis not of logic, but of history. Of course if Feigl's history turns out to be wrong, and if even empirical laws are subject to important changes—or if there are reasons for believing that, in spite of their lack of certainty, theories do play a fundamental role in science—then the logical point that Feigl sidesteps becomes particularly significant. In the remainder of this section I will review the roles that theories and other background beliefs play in observation, in the recognition of problems, and in providing criteria for what counts as a solution to a problem.

A. *Observation*

In this section I will summarize what I take to be the significant and defensible results that have come out of some twenty years of debate on the "theory-ladenness" of observation. In particular, I will argue that there are three respects in which accepted background beliefs play an essential role in determining what

[6] Herbert Feigl, "Beyond Peaceful Coexistence." In *Minnesota Studies in the Philosophy of Science* V, ed. by R. Stuewer. Minneapolis, Univ. of Minnesota Press, 1970, p. 9.
[7] *Ibid.*, p. 8.

we observe, and we should note at the outset that none of these respects involve any claims about what we might discover through an introspective analysis of our percepts. That is, we do not need to consider whether we perceive unstructured sense-data or if what we perceive is "already infected" with conceptual material in order to show that observation requires background beliefs. Of course, if what we literally perceive is structured by our beliefs, the case will be that much stronger.

Suppose one were given the task of carrying out a detailed examination of the floor of this room, with the aim of noting down every observable feature of that floor, but with no guidance whatsoever as to what sorts of things one ought to look for, and even under a strong injunction to make no hypotheses as to what features are interesting or relevant. I submit that no search of this sort could be completed in finite time nor could its results be presented in a finite list. Any serious exploration of the floor would have to begin with some range of interests and some decisions as to what sorts of things were worth paying attention to. A narcotics agent, for example, would be immediately attracted to the faint traces of cocaine on the floor, while an intelligence agent, who already knows that this room is a regular meeting place for alien spies from Vrandebar, and that Vrandebarians exhale cocaine, would pay no attention to the cocaine on the floor, but might be very interested in the presence of cigarette butts since he knows that nicotine greatly increases the acuity of Vrandebarian hearing. It is not possible for either of these investigators to pay attention to all of the myriad items that they are capable of seeing, and which items they do pay attention to will be determined by what their available background information indicates is significant.

The same point can be made in a rather straightforward manner in the case of scientific observation. The failure of many early anatomists to pay particular attention to the valves in the veins is not evidence of poor eyesight or low intelligence, but of a lack of any reason to pay particular attention to this structure, while their significance leapt to the eye and the mind once the circulation of the blood was understood. The point is even more striking in the case of sophisticated science in which instruments would not be designed and constructed at all unless one already had a fairly good idea of what was being looked for. The search for stellar parallax, for example, that lasted close to 300 years from Copernicus to Bessel would have made no sense unless one had good reasons for believing that the earth moved; indeed in the absence of strong reasons for holding this view, the persistent failure to find stellar parallax could have provided an important refutation of Copernicanism—as had already happened in the past. Similarly, we spend billions of dollars on the construction of ever more powerful particle accelerators because we have reasons for believing that these devices provide a key to making significant observations. Without this belief, not only would the enormous expense be unjustified, but there would be no reason to build these machines. It was not just a lack of technology that kept 19th century physicists out of the accelerator business, rather there was no reason to pursue this sort of technology since they had no reason for wanting to make this sort of observation.

As accepted background beliefs change, new kinds of observations are suggested; in addition, observations that had been of importance under the guidance of previous beliefs lose their importance, and other items that had been available to observation but considered insignificant, move to the center of the stage. Copernicus and Galileo, for example, suggested that heavy objects fall because they are returning to their natural place, but they defined the "natural place" of an object as the larger body from which the object originally came. Galileo suggested that a piece of stone from the earth taken out into space would fall back to the earth and that a piece of stone from the moon would fall to the moon,[8] thus Galileo believed that to predict the trajectory of such an object he would have to know what body it originally came from, a piece of information that we would take to be irrelevant. Similarly, Galen believed that examining the skin of the palm of the hand was a particularly appropriate observation for detecting imbalances in the four fundamental qualities.[9] Contemporary physicians are capable of making quite detailed studies of the palm of the hand, but they lack the

[8] Galileo, *Dialogue Concerning the Two Chief World Systems*, trans. by S. Drake Berkeley, Univ. of California Press, 1967, pp. 33-34, 97.

[9] Owsei Temkin, *Galenism*, Ithaca, Cornell Univ. Press, 1973, p. 19.

ancient physician's reasons for wanting to do so. On the other side, consider a case in which one wishes to determine how much energy is required to double the velocity of a particle in a given time. If one is working in the context of relativity one must know the present velocity of the particle, in classical mechanics that information is irrelevant.

The first role, then, that background beliefs play in observation is to provide a guide as to what sorts of things one ought to look for or pay attention to, and this is closely related to a second role that they play: having noticed some feature of the world around us we must rely on our background beliefs to tell us what it means. We can provide an initial illustration of this point if we modify our parable slightly and consider a case in which the narcotics agent is exploring the floor looking for cocaine because he has a tip that this drug is being used on campus, while the intelligence agent is engaged in the same set of physical operations, *i.e.*, examining floors looking for cocaine, but in this case because he has a tip that Vrandebarians are meeting on campus.

Shifting to scientific examples we can note, first, that Michelson and Morley[10] did not originally understand their experiment as showing that the velocity of light is independent of its source, or as indicating that there is no ether, but rather as suggesting that the earth drags the ether along with it rather than moving through the ether freely. Indeed, it would have been impossible, in 1887, to provide an intelligible interpretation of this experiment as evidence against the existence of the ether, since the design and interpretation of the interferometer presupposed the wave theory of light, and that theory, as understood at that time, was incoherent without a luminiferous ether. Similarly, Galileo and his opponents could agree on whether a stone fell parallel to a tower or not, but they disagreed about what this showed. The Aristotelians were convinced that this showed that the earth does not move, and Galileo's response was to construct a new physical theory from whose point of view the observation showed nothing one way or the other about the motion of the earth.

[10] Albert Michelson and Edward Morley, "On the Relative Motion of the Earth and the Luminiferous Ether," *American Journal of Science* 34, 1887, pp. 333-345.

There is a third respect in which background beliefs are required for observation, although it will be useful to introduce a distinction in order to describe it more clearly. I will distinguish between 'seeing' and 'observing', confining the former to our direct awareness of items that can be located in a visual field, and using the latter to refer to the process by which we use the things that we see as a source of information about other things that we cannot see. Roughly, and ignoring here any questions about the ontological status of our percepts, there is a clear sense in which we can see colored patches, and in which it is at least arguable that we can see tables and chairs and people, but in which we cannot see electrons or neutrinos. On the other hand, scientists routinely make use of items that can be seen to gain information about things they cannot see, and it is the process of drawing information about the unseen and unseeable from what we can see that counts as observation in modern science.[11] Now it is clear that the move from seeing to observing is wholly dependent on our available background beliefs. In the Reines-Cowan experiment,[12] which physicists take to be the first observation of neutrinos, the only things that the experimenters literally *see* after the experiment has been run is the readout on a counter which records the number of times that gamma rays of specified characteristics have been detected within 9 microseconds of each other, and photographs of oscilloscope images of the gamma pulses. The step from this readout to the conclusion that neutrinos have been observed clearly requires a considerable body of accepted background beliefs. A particularly striking example of this process is provided by cases such as absorption spectra, where an astronomer will note the absence of, say, a helium line in the spectrum of a particular star and take this as an observation of the presence of helium in the outer layers of that star. It takes a substantial body of background information to be able to look at an object and note that something is missing.

[11] Cf. Harold I. Brown, "Observation and the Foundations of Objectivity," *The Monist* 62, 1979, pp. 470-481, and Dudley Shapere, "The Concept of Observation in Science and Philosophy" forthcoming, for further discussion.
[12] F. Reines and C.L. Cowan, "Detection of the Free Neutrino," *Physical Review* 92, 1953, pp. 830-831.

And again, when our background beliefs change, what we can observe on the basis of a given body of percepts also changes. Relativity theory transformed our understanding of those most basic of all observing instruments, measuring rods and clocks, and as a result it is no longer possible to extract information about whether two distant events are simultaneous from the data provided by these instruments, but it is now possible to extract information about spacetime intervals from that data.

B. *Problems*

Background beliefs also play a fundamental role in the generation of scientific problems, for it is often only in contrast to expectations derived from our background beliefs that a particular occurrence or situation will appear problematic. To take e familiar example, the problems with the orbit of Mercury and Uranus that arose during the 19th century were clearly cases in which the problem consisted of a disagreement between observation[13] and theory; if the very same observations had been made without any reference to Newtonian theory the problem that Leverrier and Adams worked on would not have existed. Similarly, one could chart the paths of celestial objects, note that there are a small number of items that travel in annual loops, and be finished—such a path only becomes a problem after one has accepted reasons for believing that celestial objects cannot really move in this sort of path. Moreover, very different problems could have been generated if one began from different background beliefs. On Vrandebar the history of astronomy differs from our own in a rather interesting manner. There, research in astronomy began when Glazorp, an ancient thinker of particularly powerful intellect, concluded that there is a hierarchy of perfection among the objects in the visible universe, and that the more perfect objects have more complex patterns of motion. In the Glazorpian system, Vrandebar itself, which is taken to be stationary at the center of the universe, is the lowest and most imperfect of all objects; Vrandebar's sun and its single satellite,

[13] I will no longer be using the term 'observation' in the restricted sense of the preceding discussion.

which move around Vrandebar in simple circular paths, are considered one step higher in the hierarchy of perfection; the next level is occupied by a small number of objects which move on moderately complex looped annual paths around the center of the universe; and the fixed stars present a special problem. It is clear that the stars are the most distant objects in the universe, and in all other cases greater distance from Vrandebar is correlated with more complex patterns of motion. From these considerations Glazorp concluded that the stars are indeed the most perfect of celestial objects, that they move in exceedingly complex orbits, and that the simple circular motions seen from Vrandebar are mere appearance. Thus he bequeathed to astronomers the problem of saving the appearances by finding the true, complex motions of the stars under the condition that these motions must look circular when seen from Vrandebar.

The point may be underlined by recalling some familiar examples from the history of terrestial science. Projectile motion was a problem for Aristotle and the medievals because they had a theory which required a force to sustain the projectile's violent motion and they were unable to find that force; the failure to observe phases of Venus and the fact that Venus and Mars showed only minimal variation in brightness were problems for early Copernicans because they were contrary to what the theory entailed, these were not problems for the Ptolemaics; the absence of observable stellar parallax was a problem for 18th and 19th century astronomers because they had accepted a theory which required the existence of this effect; beta decay presented a problem to early 20th century physicists only because they had accepted a variety of conservation principles, and the available data on beta decay clearly contradicted those principles. In each of these cases the familiar problem is generated by a discrepancy between what a particular body of beliefs leads us to expect and what is actually observed. In the absence of any expectations the observed data might not have seemed problematic at all, and in the presence of a different background the data might have raised very different problems than those we are familiar with.

In addition to providing the basis for determining which situations are problematic, background beliefs also provide criteria for determining what counts as an acceptable solution to a problem, and it is often the very theory that is responsible for our viewing a situation as problematic in the first place which tells us how to proceed in trying to solve that problem and how to recognize a solution when we encounter it. The Platonic-Ptolemaic astronomy took the motions of the planets to be problematic exactly because they were noncircular, and demanded the problem be resolved by finding a set of circular motions to save the phenomena. The Leverrier-Adams solution to the problem of Uranus' orbit is the right kind of solution within the framework of Newtonian physics since it postulates another massive body, with its attendant gravitational force, to provide an additional perturbation for the orbit, a type of explanation that would not have been appropriate for Ptolemy or Glazorp. Indeed, not only is this an appropriate solution in Newtonian terms, it is the only kind of solution permitted by that theory (unless one were to discover a pervasive mathematical error), and it is not surprising that when that approach failed in the case of Mercury, the problem remained unsolved until a new theory which permitted new kinds of explanations had been constructed. In all of these cases background beliefs provide a guide as to what types of problem solution are appropriate as well as a basis for deciding if a proposed solution is successful.

II. Background Beliefs in the Philosophy of Science

It seems reasonably clear that one goal of philosophy of science is to understand science, i.e., to answer such questions as: What are the aims of science? What are the relative roles of observation and theory? What counts as an observation? What are the grounds for accepting and rejecting hypotheses? and so forth. As formulated, each of these questions is ambiguous in that it can be read as asking either for a description of how science is actually done or for a set of norms specifying how these things should be done, but for the moment I want to read these questions as purely descriptive. Indeed, it seems clear that the descriptive version of these questions must be dealt with before any intelligible approach can be made to answering the parallel normative questions, for if we were to attempt to determine how science ought to operate without first examining what science is, we might well find ourselves developing norms for magic or automobile racing instead of science.[14] But if we are to ask questions about the nature of science and attempt to answer them by examining science, then all of the points that were made in the previous section about problems and observations apply to the philosophy of science as well: one must have some beliefs about what science is before one can begin in order to know what features to look at, what questions to ask, and what is to count as an answer; philosophers working from different background beliefs are going to look at different features of science, find different aspects problematic, and accept different kinds of solutions to the problems they encounter; and the process of making observations and attempting to solve the problems generated by a particular set of philosophical beliefs may well lead to the abandonment of those beliefs. Thus the demand that science be analyzed from the point of view of classical empiricism plus modern formal logic is best understood as a background belief of the sort that we have come to recognize as fundamental in science itself, and philosophy of science is a discipline that is structurally and epistemically parallel to the sciences. The main difference is that philosophy of science is a *second-order science*, i.e., while first order sciences take various aspects of nature or of human behavior as the data about which they theorize, philosophy of science takes the theories and procedures in these disciplines as its data in attempting to build a theory of science. The revolution in the philosophy of science that has been in progress for the past two decades amounts, then, to an attempt to restructure the field on the basis of a new set of background beliefs.

I will now sketch out some of the approaches and problem areas that I think are central to the development of a new philosophy of science.

A. *Paradigms, etc.*

It was Kant who first clearly saw that there are principles in

[14] I will return to this issue in section E.

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science which have the special status that although they make claims about nature, and although observational situations can arise which, from the point of view of formal logic, contradict those principles, we refuse to interpret the observations as refutations, being prepared, if need be, to impugn the researcher rather than the principle. Since Kant we have learned that such principles are not eternal features of human sensibility or understanding, but rather subject to change as science develops, and that such principles need not be universal and necessary to be legitimate. The major current problem for those who hold that scientific knowledge is founded on accepted beliefs is to build a conceptual framework which will capture this aspect of science. There have been a number of attempts to do this, the most well known being Kuhn's theory of paradigms, and we will begin this phase of our discussion by recalling what Kuhn was up to and reviewing some of the major difficulties with his attempt.

A paradigm, for Kuhn, is a specific scientific achievement which provides a model for solving problems in a particular field, and which becomes an accepted basis for research by workers in that field. Kuhn introduces the notion of a paradigm by first listing examples of works that he takes to be paradigm cases of paradigms and then explaining that: "they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve."[15] Note particularly that a paradigm must be successful, but not too successful. It must be sufficiently successful to attract scientists, but, since scientists engage in research, it must not be so successful that it eliminates any need for further research in its domain. Note also that a paradigm is in some sense a social entity, i.e., we do not have a paradigm unless we have a *group* of researchers working in terms of a common background.

Turning to the criticisms of Kuhn's thesis, I want to begin by suggesting that one of the most frequently posed objections—that it is unclear whether a piece of work becomes a paradigm because it is cognitively successful and thus attracts a group of

[15] Kuhn, *Structure of Scientific Revolutions*, p. 10.

researchers, or whether it is taken to be cognitively successful because it attracts a group of researchers who take it as a model for their own research—is not a genuine problem at all. It is central to Kuhn's analysis of science that there is no algorithm for determining what constitutes a genuine scientific achievement, and that decisions as to scientific adequacy can only be made by people trained in the relevant scientific field. Given this viewpoint, it is by no means unreasonable for the historian or philosopher to look to the distinguishable research communities in attempting to identify shared beliefs. This does not commit us to holding *a priori* that groups of scientists never misjudge the viability or fruitfulness of a piece of work, but only to the view that the historian's or philosopher's identifications of scientific paradigms are as dubitable as the scientist's decisions as to which lines of work are worth pursuing.[16]

A second difficulty is more pressing: it remains unclear what relations hold between paradigms and theories, and between the members of nested sets of paradigms and theories. The notion of a paradigm is supposed to capture what it is that particular groups of scientists share, and Kuhn acknowledges that scientists would respond by saying that they share theories. Kuhn tells us that he avoids this term because of the connotations it has for traditional philosophy of science, but that, "I shall be glad if the term can ultimately be recaptured for this use."[17] But Kuhn also holds that paradigms include other commitments in addition to theories, e.g., values, and he suggests, along with Polanyi, that there is more to paradigms than can be formulated explicitly.[18] Indeed, there is a bewildering variety of background beliefs at work in any advanced science, and even if we restrict our attention to theories, we find researchers in specific specialties and subspecialties operating under a number of accepted theories. Kuhn repeatedly suggests the need for more detailed study of those paradigms which function for relatively small groups of researchers in the various subspecialties of a field. But if, say, solid state physicists and high energy physicists have their own

[16] I am indebted to Prof. Thomas Nickles for helping me get clear on this point.
[17] Kuhn, *Structure of Scientific Revolutions*, p. 182.
[18] *Ibid.*, p. 44.

paradigms that define their specialties, how do these "smaller" paradigms relate to quantum mechanics, a broader theory that is also paradigmatic for these fields; and how are we to understand the relation between relativity and quantum mechanics: they are not definitive of different fields in the way in which quantum mechanics and natural selection are, and they are not competing paradigms, nor do they seem to be hierarchially related? At the same time, quantum mechanics, but not relativity theory, is paradigmatic for some fields in chemistry, and there are methodological commitments that cut across several disciplines. The demand for mathematical science first emerged in astronomy and eventually spread to physics, chemistry, biology and the social sciences—but it has not become central to *all* of sociology and biology in the way in which it has become central for all of physics. What we need, then, is a better conceptual framework for capturing not only the role that background beliefs play in specific scientific fields, but also the interconnections between different systems of background beliefs.

Third, it seems clear that the fundamental beliefs accepted by groups of practitioners in specific fields are not as monolithic as Kuhn often suggests, i.e., there is no normal science in the sense of periods in which there is no dissent at all from a fundamental theory. Still, there is a genuine feature of science that Kuhn has called our attention to with his discussion of "normal science," for there do seem to be important and productive periods in which the overwhelming majority of scientists in a particular discipline share a rather substantial body of background beliefs that they are not prepared to question. And even though this does not mean that there are *no* dissenters, it is still often possible to draw a distinction between standard science and dissenting views, and scientists themselves tend to distinguish between legitimate opposition and a fringe that need not be taken seriously. To take but one example, in recent years steady-state cosmologists, who rejected conservation of matter and postulated the spontaneous appearance of hydrogen atoms in interstellar space, were taken much more seriously by astronomers, and treated much more cordially than, say, Velikovsky, who postulated interplanetary collisions in historical times. I think a case can be made for the claim that, whatever appears on the surface, Velikovsky's results and his methodology violated deep commitments held by contemporary astronomers that the steady-state cosmologists shared, but one would like to achieve a better understanding of the grounds for these decisions and of the role that they play in the development of science.

Finally, Kuhn seems in many cases, perhaps in all, to have the relation between crisis and the development of new fundamental ideas backwards. In his own discussions of the Copernican revolution Kuhn takes great pains to point out the problems that led Copernicus to propose a new foundation for astronomy. But it is fundamental to Kuhn's own analysis that the existence of problems is not sufficient to generate a crisis, and if we are to take seriously Kuhn's remarks about science as a community phenomenon, then one unhappy scientist does not constitute a crisis. Indeed, there was a crisis in astronomy in the 17th century, but it developed a considerable time after *De Revolutionibus* appeared, perhaps only after Galileo and Kepler had begun to show that the new astronomy might be made to work after all, and it seems clear that Copernicus did not appear in response to a crisis in any sense of this term that Kuhn could accept, but rather that Copernicus played a central role in the creation of a crisis. The same point can be made from Kuhn's own recent study of the development of the quantum theory.[19] Here Kuhn makes it quite clear that, although blackbody radiation was a problem in the late 19th century, nothing approaching a Kuhnian crisis existed when Planck first proposed his new theory; nor did a perceived crisis lead to Einstein's use of quantum discontinuities in his photoelectric theory. If a Kuhnian crisis developed at all, it was the successful deployment of the notion of discontinuity that led to crisis, rather than a crisis leading to the proposal of the new idea.[20]

The same point can be made even more clearly for the recent development of an historical approach to the philosophy of

[19] Thomas S. Kuhn, *Black-Body Theory and the Quantum Discontinuity,* New York, Oxford Univ. Press, 1978.

[20] The notion that pre-revolutionary periods may well be characterized by complacency rather than crisis was first suggested to me by Dr. Raymond Brock. It is discussed at length in Lewis Feuer, *Einstein and the Generations of Science*, New York, Basic Books, 1974.

science, for while the new work in this mode has certainly created a crisis for logical empiricism, there was no perceived crisis that led to the historical approach. Logical empiricism had its standard problems that philosophers worked on, and there is little if any indication in the literature of the early 1950s to suggest that a large number of philosophers of science had begun to have doubts about the efficacy of formal methods or the appropriateness of empiricist epistemology as a basis for the philosophical elucidation of science. Rather, it was the challenge of a few philosophers along with developments in fields external to standard philosophy of science—e.g., the professionalization of the history of science—that led to a number of overlapping new approaches which, in turn, generated a crisis for the tradition. We should also note that if this sequence of events provides an anomaly for Kuhn's model of scientific change, it is just what one would expect on Feyerabend's proliferationist approach, with its emphasis on the role of new ideas in indicating new phenomena that are worth looking at and in generating new problems.

I have gone on at some length about Kuhn because of the central role his work has played in this development, and I will now comment somewhat more briefly on two more recent attempts to build a better conceptual framework which will make sense of the role of accepted beliefs in science.

Consider first Lakatos' notion of a "research program." A research program is made up of (1) a "hard core," i.e., a set of beliefs which characterize that program and which are not open to change as long as one is working within that program; (2) a "positive heuristic," i.e., a set of suggestions as to how we are to apply the hard core beliefs in developing specific theories; (3) a "negative heuristic," which is a general methodological injunction not to disrupt the hard core of a program, but always to modify one or more auxiliary assumptions when a theory in the program fails an observational test. Within a research program there will be a sequence of theories which are, presumably, progressively better attempts at dealing with phenomena in the program's domain.[21]

[21] Imre Lakatos, "Falsification and the Methodology of Scientific Research

Now Lakatos' work does involve one significant advance over Kuhn's in that it explicitly focuses attention on the hierarchial relation between theories and research programs, and thus provides a tool for analyzing the ways in which theoretical commitments at one level can change within a framework provided by commitments at another level, commitments which are not simultaneously under reconsideration. But although Lakatos offers us an approach to this aspect of science, I do not think that his bilevel schema of research programs and theories is nearly adequate for the task, since it is not clear that two levels are sufficient to capture the range of background beliefs operative in science. Such beliefs occur in the design and interpretation of our instruments, including our own sense organs; in the acceptance of specific theories in limited domains, e.g., the Bardeen-Cooper-Schrieffer theory of superconductivity; in the commitment to much more general theories such as special relativity; in the role of such wide ranging principles as the demand that theories be Lorentz invariant; in the acceptance of general methodological guides, e.g., the decision to seek mathematical theories or conservation principles; in the decision to make use of particular mathematical techniques such as non-Euclidean geometries or the tensor calculus; and even in the acceptance of presumably metaphysical assumptions such as atomism. I am not at all convinced that this list is exhaustive, but it is important to realize that the solution to a given scientific problem may lead to alterations at any point in this hierarchy, and in this context any clear distinction between theories and research programs seems to vanish.

More recently, Laudan has attempted to overcome some of these defects by introducing the notion of a "research tradition." Research traditions, like research programs and at least some of the wider paradigms, are broad sets of beliefs within which specific theories are developed, tested, modified, and rejected. Research traditions guide theory development by providing an ontology and a set of methodological principles for some domain, but they do not provide specific testable claims, a task which is reserved for theories. In addition, research traditions are much

Programmes." In *Criticism and the Growth of Knowledge*, ed. by I. Lakatos and A. Musgrave, Cambridge, Cambridge Univ. Press, 1970, pp. 91-195.

longer lived than theories, sometimes enduring for millennia while ~~theories~~ come and go.[22]

Again, there are problems, and one in particular concerns us here: just how one distinguishes the theories from the research traditions never becomes fully clear. Research traditions are supposed to be something above and beyond specific theories, but it also seems that research traditions cannot exist without theories, for the main function of a research tradition is to guide the development of theories. Laudan insists that research traditions also undergo change and that while a given tradition at a particular time includes some inviolable beliefs, which beliefs are to be included in this set will vary.[23] This opens up the possibility that two snapshots of what is supposed to be a single research tradition taken at two sufficiently distant points in time may have nothing at all in common. Is there, for example, a single atomistic research tradition from Leucippus to quantum chromodynamics? Further, some high level principles cut across what would seem to be several different research traditions. Laudan (rightly) emphasizes that what he is attempting to capture under the notion of a research tradition occurs in every intellectual discipline, not just in science. But if we are to include empiricism in philosophy, voluntarism in theology, and mechanism in physiology[24] under this rubric, shouldn't we also include what might be called "mathematicism" first in astronomy and then in science generally?

We have, then, one more attempt to deal with our central problem, and one which, I think, offers some further progress in that it recognizes the possibility of continuous change at all levels in our belief hierarchy, as well as emphasizing fundamental structural similarities between science and other human intellectual endeavours. Where it founders is in its failure to come to grips with the full range and complexity of background beliefs in science, and with the ways in which these background beliefs cut across the various disciplines.

[22] Larry Laudan, *Progress and its Problems*, Berkeley, Univ. of California Press, 1977, pp. 78-81.
[23] *Ibid.*, p. 99.
[24] *Ibid.*, p. 78.

B. *Knowledge as Social*

One of the most controversial theses of some advocates of a new philosophy of science is a concern for the interpersonal interactions among scientists. Once again, this concern flows directly out of abandoning the notion that decisions to accept or reject a particular observation or hypothesis are made in accordance with a rigorous method. If there are no clear-cut rules as to what is acceptable or unacceptable, the decision must be made by the scientists involved in the relevant discipline, and, the argument goes, to understand how these decisions are made, we must study the interactions between scientists which go into the decision making process. Now there is an obvious rejoinder here, i.e., that not every interaction that scientists may engage in in the process of accepting or rejecting a theory will be epistemically relevant. As examples such as the Lysenko affair illustrate, a view may come to be widely accepted for all sorts of reasons, but that does not guarantee that it has been accepted on scientifically legitimate grounds, and at the very least, a philosophy of science ought to be able to distinguish cognitively legitimate grounds for accepting a theory from those which are not legitimate. Now I agree completely with this claim, but I also think that we must reflect a bit to understand its full implications. For in order to make the decision as to what is epistemically relevant we must have a theory of knowledge, and when we are in the process of rejecting a particular theory of knowledge and attempting to build an alternative, we cannot be expected to accept uncritically the old theory's story as to which aspects of the scientific decision process are epistemically relevant and which are not. Thus while I do not want to hold that anything and everything that a scientist does in attempting to get his colleagues to accept a thesis is, *ipso facto*, scientifically legitimate, I do want to insist that we cannot presume that the lines will be drawn in the same way by a new theory of knowledge as they were drawn by the old. In particular, we must reject the view, implicit in much traditional philosophy of science, that interpersonal interactions among scientists are never epistemically essential.

A very different viewpoint follows from the rejection of the

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idea that there is a method which produces scientific results. Rather, as I have argued elsewhere,[25] rationality requires that the individual develop the skill of making decisions in just those situations in which no effective procedure is available, and this means that there are situations in which equally rational and equally competent individuals can deliberate over the same body of information and come to different decisions. But while it is true that disagreements over the acceptability of theories and the interpretation of observations is characteristic of actual scientific research, it is also true that substantial areas of agreement emerge out of the ensuing debates, and that in an overwhelming majority of cases these agreements are arrived at without the use of pressure tactics and without having to wait for the proponents of the older view to die off—even though there is no algorithm that can be applied to settle such disputes. Now the thesis I am proposing here is that the interpersonal interactions involved in the debating process are an essential part of the procedure by which scientific views get accepted and rejected, that science as we know it would not exist without this social aspect, that an adequate epistemology of science must include an understanding of how rational agreements emerge out of rational disagreements, and that this understanding can only be arrived at through a study of the social aspect of science.

C. *Discovery*

We turn now to another area in which recent work has suggested that the lines between what is and is not philosophically significant may have to be drawn differently than they have previously been drawn. For, rather than viewing scientific discovery as an intrinsically nonrational process that has no relevance to epistemology, it is now being suggested not only that there may be rational elements in the discovery process, but that perhaps we should take the case of a scientist struggling to solve a problem as a paradigm case of rational behavior. However, as a result of this shift in attitude, just what we are

[25] Harold I. Brown, "On Being Rational," *American Philosophical Quarterly 15*, 1978, pp. 241-248.

to understand by the term 'discovery' has undergone a transformation, with two immediate effects: a) the rather clear usage of this term among logical empiricists no longer seems even moderately appropriate; and b) what is to count as a discovery has become a significant problem for both the history and philosophy of science.

When logical empiricists distinguish the context of justification from the context of discovery they make it abundantly clear that in talking about scientific discovery they mean the process by which an idea occurs to a scientist, independently of any grounds for accepting or rejecting that idea.[26] Now this is not the way historians of science, or scientists themselves, understand the notion of discovery, for they do not label an idea a "discovery" unless there are strong reasons for accepting that idea; thus discovery cannot be sharply distinguished from justification if we use the term 'discovery' in a way that is at all relevant to capturing the dynamics of science.

Turning now to (b), I want to consider what would seem to be a relatively easy kind of discovery to understand, the identification of specific classes of physical objects; for recent reconsiderations of the interactions between observation and theory have made it much less clear just what we are to count as a discovery. Kuhn, for example, notes that while Priestly seems to have been the first person to have isolated moderately pure oxygen, he misidentified it and misunderstood its role in combustion. Lavoisier got its role in combustion right, but believed that oxygen is the acidifying principle. Which of these, if either, are we to credit with the discovery of oxygen? Similarly, Malphigi was among the first people to see the red corpuscles of the blood, but he thought that they were globules of fat; are we to credit him with discovering red corpuscles? Herschel is generally credited with the discovery of Uranus, even though he thought it was a comet, and many astronomers had noticed it before him and thought it was a star. It is unclear why we should speak of Herschel and not one of his predecessors, or perhaps Lexell who first identified it as a planet, as the discoverer. If it is

[26] Cf. Brown, *Perception, Theory and Commitment*, pp. 129-131 for further discussion.

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enough to see an object to have discovered it, then the first person who saw a comet, but thought it was a god, discovered comets. On the other hand, if correct identification is necessary for discovery, we should hesitate to attribute a discovery until the final word is in on the entities and laws in the relevant domain. The moral of these examples is that, as Kuhn points out, "we need a new vocabulary and concepts for analyzing events like the discovery of oxygen."[27] In other words, just as scientists become "philosophical" about their concepts in periods of fundamental theory change, so philosophers must become metaphilosophical about their concepts in periods when philosophical theory is undergoing fundamental change.

D. *Theory Change*

Perhaps the most debated issue in recent philosophy of science is the grounds for theory change, with views running from the thesis that criteria for accepting or rejecting theories are universal, *a priori* principles that are completely free of the vagaries of the development of science, to the claim that there are no independent grounds for theory change since each (sufficiently powerful) theory includes its own criteria for theory acceptance or rejection, and thus there is no basis for comparing theories at all. On this issue, I do think we have reached a point from which some clarity can be achieved if we can agree first that a rational choice between competing theories does require that there be some common touchstone against which theories can be compared, and second that the rationality of this process does not require that the touchstone be either *a priori* or eternally acceptable—it is quite sufficient that the basis for comparing two theories be relevant to both those theories.[28] Thus it is now a standard expectation that physical theories be formulated mathematically and make quantitative predictions, and the fact that a theory is not so formulated provides a good reason for not taking it seriously, but this criterion did not apply in Galileo's disputes in which the very role of mathematics in

[27] Kuhn, *Structure of Scientific Revolutions*, p. 55.
[28] Cf. Harold I. Brown, "For a Modest Historicism," *The Monist 60*, 1977, pp. 540-555 for further discussion.

physics was a matter of debate. Still, Galileo and his opponents did agree that observations of stellar parallax or of how a stone dropped from the mast of a moving ship were relevant; and while they disagreed on the relevance of the fact that a ball dropped from a tower falls parallel to the tower, there are two rather important points to notice here. First, the disputants had no trouble at all in agreeing that the ball does fall parallel to the tower, and second that Galileo had no difficulty in understanding why the Aristotelians thought that this proved that the earth does not move, and spent a great deal of effort trying to show why they were mistaken.

E. *Norms and Descriptions*

We turn finally to the vexed question of whether philosophy of science is to aim solely at providing a descriptive theory of science, or whether it is prescriptive as well. Logical empiricists took as one of their primary aims the prescriptive task of attempting to establish canons of rational decision procedures in science, and the foundation of their claim to do so lay in the use of logic as a major tool, for logic is itself a normative, not a descriptive, discipline. But we must be careful in understanding just how logic provides the basis for a theory of how science ought to operate, for it is only within a very restricted range that logic finds its normative function. In particular, we must remember that logic does not provide a set of norms that purport to tell us how to make discoveries or solve problems or how one ought to think, but only a canon for judging the validity of linguistically formulated arguments. Further, the philosopher's claim that he has a basis for judging the validity of the scientist's argumentation requires an established, noncontroversial body of logic to which he can appeal, and if we are inclined to grant this claim in the case of deduction, we must note that there is no established canon for the evaluation of inductive arguments. Next, even in those cases in which deductive logic is clearly relevant, e.g. the case in which we have a theory and an observation which are mutually inconsistent, then, as Duhem pointed out and as we have seen *ad nauseum* over the past two decades, the question of what we are to do next is only being raised, and logic provides no guidance as to how we are to proceed. Finally, the claim that logical empiricists were engaged in a prescriptive analysis of science with the aim of being able to tell the scientists what they ought to do, is just not true. Rather, logical empiricists were attempting to analyze science from the viewpoint of a particular epistemological theory and in doing so they in fact engaged in two major endeavors: the attempt to solve problems generated by their epistemology, such as constructing an inductive logic or dissolving the paradoxes of confirmation; and the attempt to develop and modify their theory so as to capture the way in which science does operate. Thus when the early positivists discovered that physics does not, after all, conform to the demands of the strict verification theory of meaning, they did not declare physics to be meaningless, but sought ways to modify either their theory of meaning or their interpretation of high level claims in physics. When they recognized that contemporary physics is riddled with terms that cannot be explicitly defined on the basis of observables in any straightforward way, they began seeking indirect ways to establish the connection that their epistemology demanded between these terms and sensations; they did not advise physicists to get rid of these terms and construct different theories that refer only to observables. As it became clear that none of the high level hypotheses of contemporary science are demonstrably true, Hempel dropped the demand that explanatory premises be true from his theory of explanation and replaced it with the demand that they be well corroborated; he did not point out to scientists that they were not explaining anything.[29] Indeed, to the extent that logical empiricists undertook to evaluate and pass judgement on theories at all, we find that their pronouncements were exclusively directed at pseudoscience or at theories that are no longer considered part of currently accepted science.

Now this is not intended as a criticism of logical empiricism, rather it is aimed at the myth that, because logical empiricism took its foundation in formal logic, it produced a normative theory of science. As I suggested above, logical empiricism aimed at understanding science from the point of view of a

[29] See Brown, *Perception, Theory and Commitment* Part I for further examples.

particular epistemology, and did so under the perfectly reasonable assumption that, for the most part, scientists know what they are about and do not need philosophers to tell them how to do science. In fact, if we take seriously the notion that there is no single *a priori* scientific method, but that scientific methods have evolved along with science, and that, as Shapere suggests, scientists have been learning how to do science while doing it, then we cannot take seriously the suggestion that a philosopher, merely by thinking, and without the need to look at the real development of science, is going to discover the right way to do science—not even Glazorp.

III. Conclusion

The major point that I wish to emphasize in concluding this discussion is that philosophy of science is a hypothetical, tentative, theory building process of exactly the same sort that one finds within science proper. One attempts to understand the nature of science by developing a theory, and one tests that theory by assessing its ability to account for the actual features of science and to guide further research into the nature of science. Those of us who maintain, then, that it is time to replace logical empiricism with a new philosophy of science based on a different epistemology, do so because we think that logical empiricism is no longer adequate as a basis for pursuing these goals, the major goals of any theory.[30] Moreover, since one of the aims of theory construction is to carry on further research, one must presume that any theory developed thus far will eventually be overturned —and this is made multiply likely when the object one is studying is a growing, developing changing entity, as science is. Put somewhat differently, a major thrust of the work in history and philosophy of science since the 1950s is to attempt to take seriously for the first time the often stated thesis that science does not provide us with certain or eternal truths, and to begin to extend this claim to philosophy as well.

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[30] Cf. Harold I. Brown, "A Functional Analysis of Scientific Theories" *Zeitschrift für Allgemeine Wissenschaftstheorie 10*, 1979, pp. 119-140.