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Discovery and the Rationality of Science

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DISCOVERY AND THE RATIONALITY OF SCIENCE

by

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VITA

The author, William S. Hill, is the son of Robert L. Hill and Claudia A. Hill. He was born on January 25, 1944, in Winters, Texas.

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CHAPTER I

INTRODUCTION

The problem of this thesis is the discovery of scientific knowledge and the importance of discovery for understanding the rationality of science. I will argue that an adequate account of the rationality of science must include an analysis of discovery, and that many of the problems that have arisen for philosophers of science are the result of their failure to examine discovery.

Clearly, this position runs counter to traditional wisdom in philosophy of science which holds that philosophy of science concerns itself only with the justification of scientific knowledge and that discovery is the province of the pyschology or, perhaps, the sociology of science.

DISCOVERY AS A DIRECTION FOR PHILOSOPHY OF SCIENCE

I will argue that discovery is more than merely another problem that deserves its niche in philosophy of science. It represents, instead, a new direction for philosophical research into scientific knowledge. I will argue that pursuing discovery will provide a broader framework for understanding science than the justification alternative. In fact, the framework for the rationality of science developed through the discovery approach will be shown to be sufficiently broad to encompass justification.

What is it about discovery that gives it philosophical significance when so many have assumed that it was the realm of creativity and impenetrable to logic? The answer to this question lies in the movement of the discovery process. In dealing with a problem the scientist is seeking an explanation of it such that it will no longer be seen as a problem but will instead become part of that which is expected. The explanation is a theory, of course, and discovery can thus be seen as moving from observation to theory. However, it can also be seen as moving from a problematic observation to a non-problematic observation. In either case the discovery problem will involve the character of observation and the relation between observation and theory. To deny that the character of observation and the relation between observation and theory are relevant to an empirical characterization of science would seem unreasonable.

THE JUSTIFICATION APPROACH TO PHILOSOPHY OF SCIENCE

How does this relate to the problem encountered by writers in philosophy of science? I will argue, beginning with Thomas Kuhn in Chapter II, that many of those problems stem from inadequate and self-defeating concepts of observation. Kuhn, as I will show, accepts the claim that observation is theory-laden, but he interprets that claim as essentially destroying the objectivity of science, insofar as it is based on observation. Observation for Kuhn becomes a matter of consensus of a community of scientists and this has powerful implications for his philosophy of science. One such implication is his assessment of the limits of philosophy of science. Science, he says, is rational only during periods of stability because these are the only times when consensus on observation statements is achievable. Philosophy of science, then, is limited in its characterization of the rationality of science to those stable periods when testing or justification relation is functional. I will argue that Kuhn's limits on philosophy of sciences are too narrow and that his abandonment of objectivity is unnecessary.

In Chapter III I will examine a position taken by Ernest Nagel which attempts to include the theory-ladenness of observation without the limitations or loss of objectivity that Kuhn was willing to embrace. He accepts theory-ladenness but argues that observation terms and statements are nonetheless relatively stable in comparison to theory terms and statements because, although theory-laden, they are laden with "common sense." He further argues that while theory-ladenness does create problems of circularity in the testing relation, those problems can be avoided simply by choosing evidence that is not laden with the theory being tested. I will argue that Nagel has not improved the situation left by Kuhn. Kuhn describes the theory component of observation as an "arbitrary element" and Nagel describes theory as a "free creation" of the scientist. Consequently, they both treat observation as if it were at least partially determined to be what it is by the theory component. Nagel says, for instance that "significant observation involves more than noting what is immediately present to the organs of sense" ([1], p. 24). Kuhn reaches the more radical conclusion that with the development of a new theory we observe a different world ([2], p. 111).

Both Kuhn and Nagel assume that in embracing the theory-ladenness of observation they must admit that theory determines what the evidence of observation is in a generative sort of way, that is, that theory in theory-laden observation is responsible to some degree for fabricating the evidence obtained through observation. In this interpretation of theory-ladenness theory determines an observation in the senses of making it possible and in dictating in part the content of the knowledge gained through the observation.

The result of this interpretation of theory-ladenness for Kuhn is that testing in science is circular since the observations that are offered as support for any theory are dependent on that theory for their meaning. In fact, Kuhn holds that many observations that were once possible on the basis of particular theories that no one believes any longer are no longer possible. Further, he believes that during periods of major theory change, the testing process breaks down entirely and the scientist is reduced to conversion tactics in order to persuade his colleagues.

Nagel attempts to avoid such a radically unempirical conclusion about science by arguing that while the knowledge gained through observation is determined to be what it is in part by theory, observation terms are relatively more stable than theory terms and that observation terms essential to describing situations that are relevant to testing a theory are often not laden with theory. I applaud the latter defense of the empirical character of science, but it is still a weak sense of "empirical" since the knowledge gained through observation is treated as partially fabricated by theory.

I will further show that Nagel continues to believe that circularity in the testing relation is a problem. The solution he offers is prudent choice of evidence. I will argue that a more reasonable and more empirical solution to this problem is an analysis of the relation between theory and observation.

The same assumptions about the theory-ladenness of observation motivate others to argue for a theory-neutral interpretation of observation. In Chapter IV I will argue that this interpretation leads to a characterization of observation that is too indeterminate to yield knowledge of the world.

While Nagel and Kuhn accepted the claim that observation is theory-laden, they failed to analyze how theory arises in relation to observation. The theory-neutral approach, on the other hand, seeks to separate theory from observation entirely in order to avoid the rationality problems encountered by positions like that of Kuhn. Having separated theory from observation, there seems no way to get them back together.

THE DISCOVERY ALTERNATIVE

The problem for philosophy of science, as I will argue in Chapters V and VI, is not to keep theory and observation separate or even to limit their relationship, but rather it is to analyze that relationship in order to see what sort of characterization of scientific knowledge is justified.

That analysis was begun by N. R. Hanson with his logic of discovery. The function of theory in theory-laden observation, Hanson found, was to provide a context within which problematic phenomena make sense or become non-problematic. What I will do is extend Hanson's analysis of theory-ladenness in order to draw out the implications of theory-ladenness for testing in science. This analysis has logical priority over the assumption that theory-ladenness implies that the theory determines what the world is like (in the strong sense of fabricating and making observation possible) since it proposes to examine the relation between theory and observation before reaching any conclusion about theory-function in the observation process.

The analysis of the relation between theory and observation in the testing process will be supported by an analysis of that relation in the observation process itself. That analysis will be given in terms of the contributions from the world in the form of energy and from the observer in the form of theory. Energy, I will argue, is not alterable by theory. The function of theory, however, is describable in terms of its ability to select from the available energy data and to connect that data in appropriate ways.

Among the conclusions that I will reach on the basis of the discovery approach are:

1. A re-interpretation of the justification-discovery distinction as a continuum.

2. A new basis for the theory-observation distinction other than empirical content.

3. A dissolution of the problem of the meaning-dependency of observation terms on theory.

4. A solution to the problem of the non-rejection of theories in the face of counter-evidence.

5. An interpretation of scientific truth with less emphasis on conventionalism.

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CHAPTER II

THE IMPLICATIONS OF THEORY-LADENNESS FOR THE HISTORY AND RATIONALITY OF SCIENCE

In The Structure of Scientific Revolutions Thomas Kuhn found himself in what appeared to be a dilemma. The history of science provided ample testimony for the fact that the growth of scientific knowledge was more complex than that for which the model of simple accumulation of data and theory could account. Scientific change, Kuhn saw, involved more than addition, it also involved subtraction. He had no difficulty marshalling theories and "facts" from the past that no one would consider good science today. The other side of the dilemma came from his belief that science was empirical. It seemed to Kuhn that science made genuine contact with the world and that it had done so even in its distant past using theories and data that are no longer accepted.

Kuhn was faced with unattractive alternatives. He had either to treat the history of science as partly irrational or make adjustments in the concept of the rationality of science. The latter course might not have been unattractive except that the adjustments amounted to reductions. The rationality of science, as Kuhn described it, is limited to epochs. When major changes occur the rationality of science breaks down. He saw no way to develop a trans-revolutionary criterion of rationality in science.

I will argue that Kuhn's philosophy of science is an elaboration of the implications of two principles. The first is the traditional empiricist assumption that the basis of objectivity in science lies in consensus on the content of observation statements. And the second is the theory-laden character of observation. Kuhn was faithful to both principles and I will show that the problems he encountered arise from incompatibility between them.

Did Kuhn actually accomplish the replacement of traditional philosophy of science with historical insight as he promised? I will argue that he succeeded in shedding light on science in several ways, including a deeper understanding of discovery and the conditions necessary for change in science. But I will also argue that his understanding of the theory-ladenness of observation leads him to an anti-empirical position with regard to science which has inadequate philosophical content for the void left by confirmation and falsification. He does not succeed in providing an alternative account of the rationality of science for two reasons. First, his concern for science remains with the character of the testing relation; the normal science/revolutionary science distinction amounts to a definition of the limits of that relation. And, second, his misunderstanding of the meaning of the theory-ladenness of observation leaves him stranded with little else to do but define the limits of a system of thought that he believes he has rejected.

My argument will have three steps. First, I will provide a summary account of Kuhn's analysis of normal science and revolutionary science in order to see what it actually accomplishes as a replacement of earlier approaches to philosophy of science. Second, I will show how his emphasis on the history of science shaped his understanding of the theory-ladenness of observation. And, third, I will argue that with this understanding of the theory-ladenness of observation, he was limited in what he could accomplish as well as predisposed to the sorts of problems that arose with his theory.

NORMAL SCIENCE

Normal science begins for the first time with the victory of one of the pre-scientific schools over all the others. This usually happens with the solution to a problem that was recognized at least in some form by most of the pre-scientific investigators into this part of nature. That achievement usually has two essential characteristics: 1. It is sufficiently unprecedented to attract a group of researchers away from other modes of scientific activity, and 2. It is sufficiently open-ended to leave all sorts of problems for this group to work on ([1], p. 10).

A number of consequences follow from the evolution of pre-science to science. First, it is no longer necessary for each researcher to elaborate and defend the fundamentals of his work. With the dominance of a particular arbitrary element there is consensus within the community on fundamentals such as the types of entities that populate the universe, how they interact with each other, and the appropriate methods for investigating them. This is what Kuhn describes as the emergence of the "paradigm." The paradigm includes the theoretical commitment which is the same thing as the arbitrary element for Kuhn, but it also includes such things as research techniques, instrumentation, "exemplars" (in the sense of finished pieces of research that serve as instances of successful application of the paradigm, often for the purpose of teaching the students of the science), and much more.

Having achieved consensus on the arbitrary or theory element, science progresses with far more efficiency than it could have otherwise. This is due in part to agreement on fundamentals, but it is also due to the psychological assurance that the past success of the science offers. The researcher is encouraged by more than the promise of further success, however. The paradigm that grows up around the science includes methods and tools which have also proven effective ([1], p. 38). Community adoption of a particular interpretation of nature has the effect of getting research off the ground and directing it toward problems of a sort that have proven solvable in the past.

For Kuhn, science does not progress in spite of the theory-ladenness of observation as it did for Nagel, but because of it. It is only if we have a theory to augment observation and experience that we can assess the relevance of the available facts and develop methods and instruments to deal with them. Having achieved this level, researchers have a great deal to work with as well as a history of sucess to encourage them.

Kuhn says that his new image of science will be one in which fact and theory are not categorically separable, "except perhaps within a single tradition of normal-scientific practice" ([1], p. 7). Why does he allow this exception? It would seem that the dependence of fact on theory would be as great in normal science as in situations where that dependence becomes problematic in that it leads to a revolution. What he seems to be saying is that during normal science theory input into observation is not a problem. Since consensus has been achieved on the theory to employ, facts can be treated as if they were independent of theory. This is similar to Nagel's attitude toward the problem of theory-ladenness. As long as there is a foundation on which all normal observers can agree, the evidentiary status of observation is saved.

Kuhn understands normal science as those periods when debate about fundamental assumptions is minimal or non-existent. Research during these times proceeds in a fashion which is amenable to the cumulative model. Fact and theory in these periods seem separable because the facts serve their testing function in a non-problematic way. Normal science is, in this sense, philosophically non-problematic science.

However, Kuhn's concept of normal science does more than merely tag it as that part of science which satisfies the conditions of testing in the "standard view" of science. It also tells us why science is cumulative, why there is little debate over fundamentals, and why science in this situation proceeds with such efficiency. Having a theory to guide research and having that theory held in common have a powerful impact on science.

It is interesting that Kuhn does not concern himself with the problem of circularity in the testing that occurs in normal science. Nagel will avoid this problem after admitting the theory-ladenness of observation by placing the commonly held theory outside of science, but this option is clearly closed to Kuhn. It is the dominance of a particular theory that results in the emergence of science from pre-science. The reason for his lack of concern is most likely that he remains committed at some level to science as an empirical endeavor. "Observation and experience," he says, "can and must drastically restrict the range of admissible scientific belief, else there would be no science" ([1], p. 4). Observation and experience are not sufficient, as we have seen, since the contribution of the perceiver is essential. But the arbitrary element or the theory contributed by the perceiver is not the whole story. The world which is experienced allows only a range within which such theory assisted observation can function. It continues to make itself felt, even though the way it is felt is determined in part by the perceiver. Is this a form of circularity? Perhaps, but not in the logical sense that that which needs to be proven is presupposed by the evidence. Part of what needs to be proven _is_ presupposed, but not all. The evidence is shaped by the theory, Kuhn would allow, but not generated in its entirety by the theory.

REVOLUTIONARY SCIENCE

The arbitrariness of the theory element has an additional aspect to it. It facilitates progress in the conservative sense discussed so far, but it also leads to the major changes that Kuhn calls revolutions. This seems odd in light of the fact that the aim of research during normal science is not innovation of either fact or theory, but is more of a "mopping up operation," an attempt to force nature to fit the contours of the paradigm ([1], p. 24). How does research with this sort of motivation lead to major innovations and the ultimate rejection of the paradigm that guided it from the beginning? The answer to this lies in the very arbitrariness of the element contributed by the perceiver. "So long as those commitments retain an element of the arbitrary," Kuhn says, "the very nature of normal research ensures that novelty shall not be repressed for very long" ([1], p. 5). In other words, an arbitrary characterization of nature is necessarily limited. It achieves dominance because of a spectacular solution of a problem. And it proceeds to solve problems in part because of the diligence of researchers who "force nature into its contours." But this cannot last indefinitely. Sooner or later, a problem will arise that cannot be forced into the "conceptual boxes" provided in this approach. When this occurs we have the beginning of a revolution.

The failure of a paradigm is usually heralded by a discovery. To say "unexpected discovery" would be repetitious for Kuhn since the aim of science, under "normal" conditions, is not to generate discoveries, but to make the phenomena that are already known fit into the paradigm. If something unexpected arises, an "anomaly," the first response is to try to show that it is compatible with the paradigm. If this attempt fails repeatedly then future attempts tend to incorporate assumptions that diverge further and further from the paradigm. As this process goes on it becomes increasingly difficult to achieve consensus on just what the paradigm really is ([1], p. 83).

Kuhn offers this analysis as a replacement of older philosophical theories because, among other reasons, anomalies are almost never treated as counter-instances although in the language of philosophy of science that is what they are. If scientific theories were rejected in the face of anomalies all scientiic theories would have to be rejected at any given time. To do this would be to reject science itself, for bringing anomalies into the fold of the paradigm is the major research activity of normal science.

He argues that scientific revolutions are "necessary." What he means by this is that radical changes of the sort which involve the rejection of part of what was previously considered scientific knowledge are essential to the evolution of science. Science might not have been this way, he admits. The "logical structure" of science does not require it. Instead, new discoveries might involve only previously unknown phenomena and new theories might represent only higher level integrations of previously divergent fields ([1], p. 95).

This is just what the latter day logical positivists would have us believe, Kuhn says. They take development by accumulation as the ideal for science and treat instances where this did not occur as the result of human idiosyncracy

([1], p. 96). They claim, for instance, that Newton's laws of motion were not proven wrong by the theories developed by Einstein. Newton's laws provide good approximations when the velocities of the objects studied are small in comparison to that of light. Any wider claims made by the Newtonians, the positivists say, were not supported by the evidence and were, therefore, "unscientific" ([1], p. 99). Kuhn counters that if scientific assertions were limited in scope and precision to phenomena clearly supported by the evidence, that most of scientific research would become illicit, "unscientific." Scientists would be limited to talking about those discoveries which are only part of the history of their science.

The assumption that science grows through simple accumulation also ignores the disparity in fundamental assumptions which always accompanies revolutionary change. The convertibility of matter to energy, part of modern physics, was inadmissible in the Newtonian paradigm, for instance ([1], p. 102).

In other words, the positivist notion of growth by accumulation fails on two fronts. First, it refuses to admit that the process of growth in science-pushing a paradigm until it fails is scientific and, second, it cannot account for fundamental disparity between competing systems.

The cumulative acquisition of unanticipated novelty almost never occurs in science, he points out. While accumulation does occur, during periods of normal science, the discoveries are usually anticipated. And when discoveries are not anticipated, as was the case with the discovery of X-rays, they are often <u>not</u> cumulative ([1], p. 96).

This discussion helps to explain why it is often difficult to determine just when a discovery is made and who should be credited with the discovery. Discovery is accomplished in steps, the first of which is the gradual realization that that which is being discovered does not fit into the current paradigm. The second step is the development of an alternative paradigm that is capable of explaining the discovered phenomenon as well as much that the old paradigm explained. These steps take time and they are frequently contributed to by many researchers. It is only in retrospect and for the sake of simplicity that particular discoverers and precise dates are designated.

KUHN'S HISTORICAL METHOD

Why did Kuhn characterize the contribution of the perceiver as "arbitrary"? The primary reason is that he was impressed by the deep differences that have existed between scientific descriptions of the world in various times in its history. He traces an interesting back and forth shift on the admissibility of innate forces which demonstrates this point: Aristotelian dynamics, he says, were rejected largely because it included the concept of innate forces. Aristotle explained the falling of a stone, saying that its innate nature drove it toward the earth. The commitment to mechanico-corpuscularism in the Seventeenth Century excluded such qualities as "occult" and unscientific since they were not included in that paradigm ([1], p. 104). Newton's concept of gravity caused problems for the same reason. While the standards of corpuscularism remained in effect the search for mechanical explanation of gravity was one of the most important problems among those accepting the Principia, including Newton himself. Failing to find such an explanation, and unable to proceed without Newtonian theory, gravity was gradually accepted as a force innate in particles of matter ([1], p. 105). This acceptance had impact in other fields such as electrical theory where it legitimized the concept of attraction at a distance, leading eventually to Franklin's interpretation of the Leyden jar experiments and to a Newtonian paradigm for electricity ([1], p. 106). And, finally, Einstein's theories represent a shift back to pre-Newtonian science in that they explain gravity without reference to innate forces ([1], p. 108).

Another reason is his commitment to the belief that science is empirical. It seemed to him that past scientic theories did a creditable job of making sense of the phenomena with which they were confronted. They did not merely manufacture those phenomena. Their theories were concocted in response to the environment.

Kuhn was faced with making sense of two aspects of science. First, radical change was revealed in its history, and second, that history also revealed that out-of-date systems of belief were both empirical and often highly successful ways of explaining the natural environment. He tried to explain both of these facets of science by treating science (and perception itself) as an amalgam of (a) genuine impact from the environment and (b) a creative contribution by the scientist. By retaining the impact of the environment he could keep his conception of science in accord with a basic commitment to empiricism. And by adding an element to perception that was arbitrary in the sense of being contributed by the perceiver and not by the environment, he had a way of explaining change in science that could reach all the way to observation. Further, he could accommodate such change without designating all previous bodies of belief as unscientific, as myth, or as simply in error.

THE SUCCESS OF KUHN'S REPLACEMENT

It is clear that Kuhn's theory of science is successful in some respects. He is able to explain the rapid progress of science under "normal" conditions by demonstrating the guiding aspect of the paradigm. He is also able to explain major change in science without adopting an anti-historical interpretation of science. He has shed new light on discovery, and he has explained why scientists are tolerant of apparent anomalies.

But has he solved the problems which will cause Nagel and Thrane to try so diligently to avoid the theory-ladenness of observation as a part of their philosophies of science? Has he developed a theory of science which can replace the analysis of the testing relation as the model for the rationality of science? I believe that the answer to these questions must be "no." As Frederick Suppe points out, most of the criticisms of Kuhn have centered on the assertion that his concept of revolutionary change in science is fundamentally irrational, and that he ultimately characterizes science as unempirical ([2], p. 150). Kuhn contributes to these criticisms with claims such as his "different worlds" thesis, saying that after a revolution the scientist responds to a different world ([1], p. 111). He seems to be saying that theory is not only constitutive of science, but that it is also constitutive of nature. He is uncomfortable with this claim, calling it a "strange locution" ([1], p. 118). However, he feels that we must somehow make sense of both attitudes, that even though the world has not changed with paradigm change, the scientist works in a different world. The reason is simple--what occurs in a revolution is not merely the re-interpretation of old data, but it also involves the emergence of new data ([1], 121).

Kuhn attempted to accomplish both these ends with one theory. By retaining the impact from the environment he hoped to keep his theory on firm empirical grounds. And, by introducing an arbitrary contribution by the perceiver he thought he could explain revolutionary change. He did explain many things about revolutionary change that previous theories had failed to explain, but he did not provide a rational structure for such change. In fact he denies that this sort of change is rational at all. It is a matter of conversion instead of proof, he says ([1], p. 148).

He attributes to the dominant epistemological paradigm of recent time the attitude that experience is fixed and neutral while theory is the man-made interpretation of the neutral data provided by experience. This paradigm no longer functions effectively but in the absence of an alternative, he says, he cannot relinquish it entirely ([1], p. 126).

The part of that paradigm that he did not relinquish is the genesis of theory. He sees theory as the free creation of the perceiver in the same way that Nagel and Thrane will see it. What does this mean for his philosophy of science? In combination with his belief that observation is theory-laden, it is a powerful assumption. Observation, given this pair of assumptions, is far more than merely theory-laden. It is at least partially theory-fabricated. This creates no problem as long as the theory is accepted by the entire community of scientists, but it makes choosing between competing theories necessarily non-rational. The evidence is fabricated in different ways from the perspective of each theory.

But Kuhn's attitude toward theory genesis is not the only reason why he comes to apparently non-rational conclusions. His philosophy of science retains another component of the dominant epistemological paradigm, its emphasis on the testing relation. His work can be read without distortion as a definition of the limits of philosophy of science, or of the limits of rationality in science. Testing proceeds as Nagel's "familiar methodological principle" would have it during normal science, but it breaks down occasionally, and these occasions are called revolutions.

His philosophy of science is constructed from incompatible components. He retained the "free creation" model of theories, but in the standard view of science this was but one of two principles that served in the analysis of the testing relation. The other was the belief that observation was theory-neutral or at least not laden with the theory being tested. His attempt to replace the positivist and falsificationist approaches to philosophy of science could not succeed. He retained the free creation model of theory and concern for the testing relation, but he gave away the second principle that served in the analysis of the testing relation. Testing can make sense of science only if one of the principles remains stable. Since neither of Kuhn's principles remained stable he was pre-disposed to non-rational consequences.

In summary, Kuhn proposes to replace the standard interpretation of the rationality of science with an analysis of its history. He is motivated to do this by the recognition that change in science has often involved more than mere re-interpretation of old and stable data, but may also involve change in the data as well. Ultimately, he sees this as the result of an arbitrary contribution on the part of the perceiver.

The arbitrary element has much positive influence on science. It provides direction and tells the researcher the relevance of available facts. Without it science and perception itself would be impossible. Its arbitrariness, on the other hand, guarantees that its usefulness will not last forever. Its usefulness ends with a discovery it cannot accommodate and with the emergence of another such element that can account for the discovery.

Kuhn has been able to explain many elements in science which are ignored or denied by other philosophical theories. I have argued, however, that he has not been able to achieve an alternative model for the rationality for science, and that the primary reason for this failure is his interpretation of theory as the "free creation" of the mind. Consequently, the theory-ladenness of observation is interpreted as at least partial theory-fabrication of observation.

Is there a way to retain Kuhn's insights into science and avoid his non-rational conclusions? I believe that there is, but a new model of theory and of the theory-ladenness of observation must be developed if this is to be done. The theory-ladenness of observation must be taken as a problem for careful examination in philosophy of science in order to see what it really means. One should not assume, as Nagel, Thrane and Kuhn have all done, that its meaning is clear and that its place as an assumption for philosophy of science is unproblematic.

The theory-ladenness of observation as a problem for the philosophy of science will be my starting point in Part II. The general character of my approach to philosophy of science will change as a result. The examination of the testing relation, for instance, will not be the first order of business. Until the meaning of the theory-ladenness of observation is clarified, one of the relata of that relation remains unspecified.

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CHAPTER III

THE IMPLICATIONS OF THEORY-LADENNESS FOR THE STABILITY OF EVIDENCE IN SCIENCE

Many attempts have been made to solve the problems that dominate Kuhn's work. Few philosophers have been willing to enbrace his conclusion that a significant part of science, its periods of major change, are non-rational. The concomitant conclusion that there is no trans-revolutionary cirterion of rationality has been found equally unpalatable by most philosophers.

In "Theory and Observation" Ernest Nagel offers a solution to the problems brought by the theory-ladenness of observation. He grants that theory-ladenness destroys any inherent difference between theory and observation statements and terms, but he argues that differences in "use" of these statements and terms are sufficient to ground the distinction. He further argues that differences in stability between theory and observation statements and terms justifies the continued assumption of a viable testing relation between them. In an additional argument he tries to show that, while circularity can be a problem as a consequence of theory-ladenness, the problem is manageable merely by choosing evidence that is not laden with the theory being tested. In other words, Nagel argues that pholosophy of science can accommodate theory-ladenness with no major change in its characterization of the rationality of science.

I will argue that the concept of the basis of rationality has not changed with Nagel, but continues to be based on consensus on the content of observation reports. I will show that this continued assumption undercuts all his attempted solutions.

Nagel's article is important for the added reason that it brings out a concept of theory that is as problematic as his concept of observation. The reason, I will argue, is the separation of theory and observation that results from the failure to examine discovery.

Nagel has three arguments which are intended to show that the testing relation in science has not been compromised by the admission of the theory-ladenness of observation. One involves his concern for the relative stability of observation terms and statements mentioned above. He argues for an identifiable class of observation terms which are not subject to the vicissitudes of theories in science. He locates this class, or sub-class, since it does not include scientific uses of observation terms, outside of science in what might be called "common sense" or normal, everyday uses of observation terms. He calls these uses of observation terms "core" uses as distinguished from "peripheral" uses of the same terms in science. I will argue that the placement of the "core" of observation terms outside of science puts observation beyond the ken of philosophical analysis.

A second argument defends the continuing viability of the theory/observation distinction, given that observation is theory-laden. In this argument Nagel first concedes that most if not all of the "inherent differences" that had been assumed to exist between theory and observation terms and statements are dissolved by the admission of the theory-ladenness of observation. He goes on to argue that none of these supposed differences are essential to maintaining the distinction, and that their loss does not impair the function of the theory/observation distinction in the analysis of the testing relation in science. All that is needed, he says, is to identify different "uses" to which the two sets of terms are put in the actual conduct of scientific inquiry. The uses he identifies are interesting, opening up the possibility of a broader investigation of scientific knowledge. I will argue that instead of investigating the possiblities implicit in the uses he identifies he actually abandons them as a source of insight into science and falls back on the different levels of stability mentioned above as a criterion of different uses.

His third argument is his defense of testing as non-circular. He admits that circularity is a problem. If an observation term that is laden with a particular theory is used in an observation statement that purports to test that theory then the test is circular and not valid. But as he indicated at the outset, he believes that observation terms, though theory-laden, are not meaning-determined by the entire set of theories and laws that make up a science at a particular time. Such a term can be used to test any theory or law other than the particular one that determines its meaning. He adds a historical argument against the seriousness of the circularity problem. I will argue that his historical argument is well taken and that circularity in scientific testing is not a serious problem at all. I will show that the reason why Nagel took the problem seriously was due to his inadequate concept of scientific theory, and that given that concept of theory, circularity is indeed a serious problem.

THE RELATIVE STABILITY OF OBSERVATION TERMS

Nagel grants that changes in theories and laws inevitably affect the way in which terms laden with those theories and laws are used. This is true even for "basic terms" like "red." The redness of a star, for instance, may be regarded as the effect of its motion and not as its genuine color after certain theoretical advances are made. He calls this a "peripheral" use of the term 'red' and he says that there remain "core" uses of such terms such as the color of apples and traffic lights which remain unchanged with theoretical changes of this sort. This is what he means by "relative stability." The relative stability of the "core" uses of observation terms is what, in part, makes the theory/observation distinction both warranted and useful, he says ([1], pp. 33-34).

It has been pointed out, he goes on, that the world might have been different, that one can conceive of physically possible circumstances in which the core meanings of observation terms would not apply. The argument is not relevant, he says, since relative stability is significant to the understanding of scientific knowledge even if it cannot be demonstrated that it is "cosmically necessary and unalterable ([2], p. 34).

In a later argument he makes a similar point, saying that when theoretical statements that report observations are threatened we must pull back to statements including predicates of "normal perceptual experience" ([2], p. 37). In this way ordinary non-scientific language provides a sort of foundation for science that is always available if theoretical expansions of knowledge fail to pan out.

It is interesting to note that both the "peripheral" and "core" uses of red are observation terms. The "peripheral" uses are those found in science and "core" uses are "normal" or 'common sense" uses of observation terms. Relative stability has been demonstrated not between theoretical terms and observation terms, but between scientific and non-scientific uses of observation terms. This is the consequence of the theory-ladenness of observation, of course. Having accepted that observation is theory-laden, Nagel cannot deny that the observation terms of science are theory-laden and therefore subject to the vicissitudes of scientific theory. Non-scientific uses of observation terms are theory-laden in some sense as well, according to Nagel, for he has already granted that "significant observation involves more than noting what is immediately present to the organs of sense" ([2], p. 24). But the non-scientific uses of observation terms do not have their meaning determined by science. It is here that their relative stability and their value lies.

This is not the problem of circularity because it is not a particular testing relation that Nagel is worried about. Rather it is the general character of the ground of testing that concerns him. If testing is that which characterizes science then a stable ground of test must be isolated. It is not, apparently, to be found <u>within</u> science since changes in science would affect its stability. Nagel has chosen to place the ground of test outside of science where it is beyond the theory-ladenness of observation, at least insofar as theories are generated by science or scientists.

Placing the ground of testing in observation terms outside of science also places it beyond philosophical, that is, epistemological analysis. In a sense, this is not a surprising move since Nagel's predecessors in the empiricist tradition did the same thing. Prior to the airing of the issues surrounding the theory-ladenness of observation, R. B. Braithwaite insisted that the philosophy of perception was irrelevant to the philosophy of science. Regardless of the answer one reached regarding the philosophical character of observation, he said, it would serve the purpose of identifying the facts of observation which are the same for all normal observers ([1], p. 4). One might have expected this attitude to change with the introduction of the theory-ladenness of observation into the discussion, but Nagel side steps this problem by locating the ground of testing in a sub-set of observation terms that, if theory-laden, are at least laden with theory that is common to normal observers.

But what is more surprising than Nagel's agreement with Braithaite is the similarity between his position concerning the location of the ground of test and the position of Kuhn on the same issue. For Kuhn the ultimate ground of test or justification for any observation is the paradigm. And the paradigm, like Nagel's "core" of observation terms, is outside of science in the sense that it is not open to any test. Like Nagel, Kuhn takes the testing relation to be primary in understanding science. The main point of <u>The Structure of Scientific Revolutions</u> was to define the limit of the testing relation. That limit is defined by the concept of the "revolution." Testing, for Kuhn, can proceed in the "normal way" so long as the paradigm is not questioned. But as soon as the paradigm becomes the issue the testing process breaks down. And so does philosophy of science. Kuhn offers no philosophical insight into the process of revolutionary change other than the fact that it happens. His discussion of it is given in terms taken from sociology and pyschology.

The point of bringing up Kuhn here is to emphasize the limitation on philosophy of science caused by placing the ground of test outside of science. Nagel differs from Kuhn in that he places his ground of test in common sense, and since common sense might never change, revolutions might never occur. But should a change of such depth occur, he would have no more than Kuhn to say about it.

Other important questions are ruled out as well. For instance, the character of observation is more open to philosophical analysis with the recognition of its theory-ladenness. Its character is not investigated by Nagel. Instead its character as evidence for testing is presupposed and treated as exhaustive. The determination of just what it means to say that observation is theory-laden is not addressed either. By placing the observational core outside of science Nagel hopes to neutralize the impact of theory-ladenness on philosophy of science. A closer look at what theory-ladenness means would not be important on these conditions. Neither is the more general issue of the relation between observation and theory given priority since observation is essentially an unanalyzable term in this approach to philosophy of science.

THE "USE" CRITERION FOR THE THEORY/OBSERVATION DISTINCTION

Some critics of the theory/observation distinction have suggested that the admission of the theory-ladenness of observation dissolves the "inherent differences" between theory and observation terms and statements. Nagel identifies three types of inherent difference that have been attacked. First, some critics have argued, the proponents of the distinction have sometimes held that it was justified because theory terms are inherently problematic while observation terms are understandable in their own right. Nagel grants that either type of term may be clear in some of its applications while it is problematic in others. In other words, he says, all terms have a "penumbra of vagueness", including observation terms, but this does not vitiate the distinction itself ([2], p. 30).

Secondly, it has been charged that the assumption that theory terms and observation terms represent two different "languages" in science, a self-contained and autonomous language of observation which deals only with directly observable matters and a theoretical language which deals with unobservable matters ([2], pp. 27-28), does not survive the admission that the observation language is theory-laden. Nagel agrees. Insofar as the "two languages" locution has any meaning, he says, it refers to different uses or functions to which certain groups of expressions are put in the process of articulating inquiry in science ([2], pp. 31-32).

The third inherent difference between theory and observation terms is actually part of the second one above. The assumption that the sets of terms differ because observation terms, but not theory terms, can be predicated of things on the strength of direct observation alone, falls before the admission of the theory-ladenness of observation since the addition of theory to observation makes it indirect as well ([2], p. 32). Again Nagel agrees. And, again, he says that it does not matter.

Why does it not matter? Because all that we need to be able to do is distinguish different <u>uses</u> to which the sets of terms are put. What are the different uses? Nagel identifies five typical uses for observation terms and three typical uses of theoretical terms: Observation terms are used to, (a) "mark off in perceptual experience" objects and processes, (b) to characterize an entity as of a certain type, (c) to describe instrumentation, (d) to report measurements and other perceptual findings, and (e) to "codify experimentally ascertained data" ([2], p. 29). Theoretical terms are used to (a) codify idealized or limiting notions (such as point-mass and instantaneous velocity), (b) prescribe how the things identified in perceptual experience are to be analyzed and manipulated, and (c) provide inferential links between experimental data and "conclusions of inquiry" ([2], p. 30).

Nagel entertains an additional argument regarding use which appears to dissolve even that way of making the theory/ observation distinction. Critics have pointed out, he says, that predicates ordinarily classified as theoretical are often used to describe situations which are "directly apprehended" ([2], p. 35). Since he has already granted that observation terms cannot be predicated of things on the basis of direct observation he clearly must not mean that theoretical terms can be predicated of things in this way. What he must mean is that theoretical terms appear in some cases to be predicated of things in as direct a way as are observation terms. Examples of such theoretical predication are the description of a land formation as a "glaciation" and the description of a track in a cloud chamber as having been produced by a positron-electron pair. It is beyond doubt, he says, that this sort of thing happens. Some theoretical predicates are used to describe observable matters while others apparently never are. We would not, for instance, describe what is observed in the electrolysis of water as the rearrangement of electrons in hydrogen and oxygen atoms ([2], pp. 35-36).

Just why some theoretical terms are used to describe observable situations and others are not is not clear, Nagel says, but he endeavors to shed some light on those situations where theoretical terms _are_ so used. In many instances of this sort the theoretical term in question appears to serve as a "shorthand formula" for describing observable but complex features of an experimental event. The shorthand replaces a long and involved account if that account were to be given in "terms of perceptual experience" ([2], p. 37). "Accordingly," he concludes, "it is only in a Pickwickian sense that the theoretical predicates can be counted as observation terms" ([2], p. 37).

It is interesting to note, he adds, that when theoretical terms that report observations are threatened they must be replaced by terms from "normal perceptual experience." This part of the argument was mentioned earlier in connection with the discussion of the relative stability of observation terms. Why must we pull back to the "more familiar observation predicates?" His answer is that they are "better warranted by the actual evidence" ([2], p. 38). But why are these more familiar terms better warranted? Because, he says, the theoretical terms assert _more_ than the ordinary observation terms, "on pain of being totally superfluous" ([2], p. 38).

It is interesting that the "use" basis of the theory/observation distinction has shifted here. Theoretical terms <u>are</u> used differently from observation terms in that they assert more than observation terms, or expand on those terms. Both assert more than is immediately present to the senses, but theoretical terms go beyond observations that are "normally" recognized. To repeat, the new "use" of theoretical terms is to expand knowledge beyond what is "normal" and the "use" of observation terms is to provide a stable retreat when that expansion is in doubt.

Why does Nagel shift his ground for the use basis of the theory/observation distinction? One reason may be that he has not found anything in particular to do with the uses he identified earlier. They are interesting in that they provide some possibility for expanding his treatment of science. The use of theories, for instance, in providing inferential links between experience and conclusions of inquiry suggests the possibility of examining the relation between observation and theory in a detailed way.

But Nagel has no intention of following up such a suggestion. His concept of science will not allow it. In particular, his concept of theory blocks his invesitgation of science. His treatment of theories is given in terms of "free creations" of the scientist. If theories are free creations of the scientist it is troublesome to allow that they report observations. Such a concession would appear to make a mockery of the testing relation and empiricism in general. This is why he seeks to explain away the sense in which theories are said to report observations. But his attempt to do this raises more questions about his approach to science than it answers. Why, for instance, does he say that theories are "free creations" of the scientist and then assign to them the role of shorthand for observation statements? It would seem an unusual coincidence for a free creation to dovetail so well with observation. If it is not a coincidence, then why is it not a coincidence? An adequate understanding of science is at stake here. But if theories serve this function in science, why suggest that they also assert more than that for which they are shorthand? To avoid being superfluous, of course. Nagel clearly recognizes that theories must do more than serve as shorthand for observation statements, but just how they accomplish more than this is not an issue that Nagel is interested in addressing. And what of the theories that apparently never report observations? Perhaps they codify limiting or idealized notions or prescribe how things identified in experience are to be analyzed, as he outlined earlier. It appears that the uses of theories need to be better clarified if "use" is the criterion of the theory/observation distinction which, in turn, is essential to understanding the rationale of science.

What concept of science must Nagel have in order to employ arguments of the sort that we have seen so far? It would appear that science for him is a collection of theories. Observation statements cannot be included since they are placed outside of science. And it appears to be a finished product rather than a process. Such a concept of science, it seems, would not require a scientist.

THE PROBLEM OF CIRULARITY

Nagel entertains one last attack on the theory/observation distinction which he considers a "radical" challenge. Some critics of the distinction have claimed that every theory determines the meaning of the observation predicates that they used to test it. In other words the theories "manufacture" data in such a way that every test is "fatally circular." Only evidence which is generated by the theory, according to this criticism, can serve as the basis of testing. As a result, no theory could possibly be refuted, but neither could they be said to have any factual content, according to Nagel.

Further, if the meanings of observation terms were determined by the theory for which they serve as evidence, the same observation report could not confirm one theory and disconfirm another. In other words, it would be impossible in principle to ever decide between competing theories ([2], p. 41).

Nagel has two arguments against this radical thesis. First, he says, the history of science provides evidence against both its aspects. Many theories have been refuted on the basis of observational findings. Therefore, it must not be the case that observational evidence invariably is molded by the theory it is supposed to test ([2], p. 39). It is also the case, he says, that even theories that have profoundly different presuppositions often share "some hard core pedicates and laws" ([2], p. 41). Newtonian and Einsteinian dynamics, for instance, share important predicates such as "acceleration of bodies falling near the earth's surface," as well as a number of laws in which such shared predicates are found ([2]), p. 42).

His second argument is that while an observation predicate may be determined in part by a theory, it need not be dependent on all the laws that make up the theory. Since some of the laws that make up a theoretical system may be logically independent of each other, it is possible for an observation term to serve in evidence statements for those laws which do not determine its meaning ([2], p. 41). For example, it is possible to count the laws concerned with measuring instruments as well as the laws of Euclidean geometry as parts of Newtonian dynamics. This does not, however, make observation terms relating to measurements or geometrical assumptions circular as evidence supporting some other part of Newtonian dynamics ([2], p. 40).

The theory-ladenness of observation appears to Nagel to present a serious problem for our understanding of the rationality of science since that rationality is given in terms of the testing relation. He seeks to avoid the supposed implication that evidence is determined by the theory it is evidence for by distinguishing between the impact of the theory-ladenness of observation on evidence in general and its impact on evidence in particular situations. This argument is linked to his earlier argument for the relative stability of "core" observation terms. Competing theories, he says, often share such "hard core predicates." If this is so then meaning-dependence must not be immediate in the sense of dependence on the particular theory being tested.

The issue here is not the meaning-dependence of observation terms on theories as a result of the theory-ladeness of observation, but rather the scope or immediacy of the dependence. But why does he accept the meaning-dependence of observation terms at all? Is this what it means to say that observation is theory-laden, that the meaning of observation terms is determined by theory? And if theories are the "free creations" of the scientist, does this mean that observations are generated by theories? It appears that this is the implication for Nagel since, at least in some cases, observation is no longer counted as genuine evidence. This is what circularity means.

Under what conditions might the problem of circularity arise? Suppose that a biologist prior to the discovery of viruses hypothesized that there was a living organism smaller than any bacterium which was responsible for certain diseases whose causes remained unexplained. He then executed the following steps in the hope of gaining evidence for or aainst his hypothesis: 1. Cultures were taken from individuals before, during and after the onset of a particular set of symptoms. The cultures were viewed under the electron microscope with the result that the culture which was taken while symptoms were active showed "shapes" that were not present in either of the other two cultures. 2. Otherwise healthy individuals were infected with the active cultures with the result that they developed the same set of symptoms. 3. Cultures taken from these people showed similar "shapes," when viewed under the electron microscope. 4. Other diseases that were unexplained as to cause but which appeared to be transferred by contact were examined using these techniques with the subsequent discovery of more such "shapes." He then presented his results as evidence for his hypothesis, saying that the "shapes" were in fact living organisms called "viruses."

Is there anything circular about this sort of reasoning? All the observations are clearly theory-laden in the sense that they involve a theory of disease, that disease is the result of the parasitic infestation of one organism by another. Additionally, a great deal of theory was involved in the development of the electron microscope. These, however, are not the theories being tested. One can recognize characteristic shapes under the microscope or observe symptoms without being committed to the theory of viruses.

But what about step four? In this case we look in a place not previously examined for evidence to support our theory and we look <u>because</u> of our theory. This is theory-guided observation, in a sense, but it does not seem at all circular. The reasons are the same as above, the theory of viruses is not essential to the observation of symptoms or of "shapes" under the microscope.

What, then, would constitute circular evidence? Suppose our experimentor had stopped at step one of his investigation and described the "shapes" viewed as "viruses," the cause of the disease. The question which sparked the research was, "What is the cause of this disease?" Is the answer, guided as it was by the hypothesized theory of viruses, circular? No, it is merely an insuficiently supported assumption. Suppose, then, that prior to step one, without viewing before and after cultures, our researcher asserted that he had found "disease vectors" that he had named "viruses" in the cultures of sick people. But to offer an explanation of a disease without evidence is similar to attributing the sleep inducing quality of opium to its "soporific" effects. It presupposes that which needs to be explained. It represents a very primitive form of question begging. But again, is it circular in Nagel's sense? No, the recognition of shapes does not require the theory of viruses. It is, of course, quite unsupported.

What is the difference between question begging and circularity? In question begging the problem is whether the explanandum is presupposed or entailed by the explanans. In circularity the problem is whether the explanans determine the meaning of the explanandum. With question begging we are concerned about the quality of an argument or explanation. Does it explain that which it set out to explain is the issue. With circularity the quality of observational evidence is the issue. Is it genuine or not?

In cases as simple as my example question begging is highly unlikely, but it can become a problem as questions become more complex. Circularity, on the other hand, is highly unlikely in any case. The unlikeliness is not a function of complexity but of what it means to say that observation is theory-laden. If theory-ladenness means that theories are capable of generating observations, then circularity must be guarded against. But theory-ladenness should represent the substance of questions for philosophy of science rather than the source of presuppositions. It may be that it is more accurate to say that observations generate theories than that theories generate observations. There seems no reasonable sense in which observations could be said to be generated by theories. And, further, there seems no reasonable sense in which the theory-ladenness of observation raises any doubt about the geniuneness of observation as a source of evidence.

The difference between circularity and question begging is the difference between two approaches to the philosophy of science. If one understands the philosophy of science to be the analysis of the testing relation where observation is placed outside of science and beyond philosophy of science, and theory is treated as the "free creation" of the scientist, then the theory-ladenness of observation will raise the problem of circularity. But if the problem of philosophy of science is the investigation of the relation between theory and observation, then the theory-ladenness of observation represents grist for the mill. Should the issue of question begging arise it would arise in that context as part of the problem of what constitutes an adequate explanation.

In summary, Nagel has presented three arguments he believes demonstrate that the theory-ladenness of observation has no significant implications for the "familiar methodological principle" that theories in science must be tested by confrontation with the findings of observation in the form of observation statements.

First, he argued that even though oservation <u>is</u> theory-laden, and even though this means that observation statements assert more than is immediately present to the senses, there remains within observation terms a "core" which is relatively stable in comparison to theory terms or observation terms that are used within science. In doing this he has placed his foundation of evidence outside of science and beyond the realm of philosophical analysis, with the result that he is in a position similar to that of Kuhn. An important part of science has been excluded from philosophy of science.

Second, Nagel argues that while no "inherent differences" between theory and observation terms survive the recognition of the theory-ladenness of observation, such differences are not essential to maintaining the theory/observation distinction. Differences in "use" can be distinguished in actual scientific practice and this is enough to make the theory/observation distinction warranted and useful. However, he declines to develop these different "uses" as instruments for shedding light on scientific knowledge. Instead, when he entertains a challenge to this sort of difference he appears to shift his ground of "use" to reflect the difference in stability argued for earlier. This second sense of different "uses" reveals in more detail his attitude toward theories. He wants to keep theories within parameters that would allow them to be supportable by observations. To this end he labels them as "shorthand" when they appear to report observations. But he also wants them to have a genuine function in expanding scientific knowledge, for, as he points out, they would otherwise be superfluous.

I applaud his attempt to expand the role of theories beyond "shorthand," but this concept of theories is not adequate to resolve the inherent conflict between these two characterizations.

Third, Nagel argues that circularity is not a serious problem since a theory-laden observation can test any theory with which it is not laden. The history of science bears out this position, he says. I agree with him that circularity is not a serious problem. I have argued that it is even less serious than he takes it to be, that there is little reason to suppose that the theory-ladenness of observation puts observation in jeopardy as regards its status as evidence in any case. He sees circularity as a problem because of his inadequate concept of theory and because of his subsequent misinterpretation of what the theory-ladenness of observation means.

The theory-ladenness of observation should be seen as a potentially significant insight into scientific knowledge and as the starting point for further investigation. Nagel sees it as a threat to the way science was understood prior to the recognition of the theory-ladenness of observation.

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CHAPTER IV

THE THEORY-NEUTRAL APPROACH

The rift between observation and theory that was revealed in Nagel's work is in fact an implicit part of justificationism as an empiricist philosophy of science. This comes out most clearly in an article entitled "The Proper Object of Vision" by Gary Thrane. If the basis of objectivity in science is consensus on the content of observation reports as Thrane sees, theory cannot be accommodated but must be purged from observation. If basic observations were not theory-neutral, he says, there would be no way to test theories.

I have included this article here because it shows that the problem of theory-ladenness is unsolvable in justificationist philosophy of science. The concept of objectivity implicit in justification is not compatible with theory-ladenness, but the return to fundamental or theory-free observation will fail to solve the problems of that school. Thrane elaborates the conditions that must be met for theory-neutral observation as well as the conditions that must be met for theory-neutral observation to serve as the epistemological foundation of science. As we will see, neither set of conditions can be satisfied. Having separated theory and observation at the start justification has created a gap that cannot be bridged.

Thrane uses quotations from C. I. Lewis to make the significance of the issue of theory-neutral observation clearer. The problem for Lewis was whether both the data of experience and the interpretation put on them belonged to the mind, whether there was anything in experience that the mind could neither create nor alter. He believed that there was in experience a "given" which was characterized specifically by the fact that it was "unalterable" ([2], pp. 6-7).

This seemingly plausible position has been attacked by, among others, Hanson and Kuhn, Thrane says. Both of them, he holds, argue that different people may see different things when apparently confronted with the same situation. That is, both of them argue that, "What we see is altered by what we think ([2], p. 7).

In response Thrane proposes to argue that what we <u>really</u> see is "the pattern of light projected on the retina ([2], p. 9). He is well aware of the many problems that attend this type of theory and much of his article is addressed to those problems. His motivation is clear; he hopes to establish a level of seeing that is sufficiently fundamental to escape the influence or impact of theory or knowledge. The pattern of light on the retina would appear to satisfy this condition as well as providing a reasonable sense in which all perceivers with "normal" eyes could see the same thing when confronted with the same situation.

Thrane's article is important to this thesis for several reasons. First, he does not merely presuppose that theory-neutral observation is possible, but he tries to show _how_ it is possible. His article helps to establish minimal conditions for what it means to see, or more generally, to perceive. It brings out certain problems that appear to be inherent in "fundamentalist" approaches of this sort. It clarifies the epistemological implications of this type of theory of perception. And it helps to clarify that it means to say that observation is theory-laden.

My discussion of Thrane's article will have four major sections roughly reflecting the reasons for its importance mentioned above. First, his "awareness argument" will be treated. Awareness is a problem for Thrane because we as perceivers seem to be unaware of that which his theory says that we really see. I will present his argument for the possibility of perception without being aware of what is perceived. Further, I will offer a distinction within the concept of awareness to see whether his theory can be made to work on other grounds. And, finally, I will argue that awareness of that which is perceived is a minimum condition for perception.

Second, the "fundamentalist" approach to perception will be examined. I will show that this approach is the result of trying to avoid the input of knowledge in perception. This approach, I will argue, leads to "compound" perception, or the seeing of one thing in order to see another. This will be shown to be an error that is inherent in the fundamentalist approach.

The third section will deal with the epistemological implications of such a theory of vision. Thrane's assessment of those implications is largely accurate as far as it goes. But as I will show there are larger implications for his project as it relates to philosophy of science.

The last section will deal with the meaning of the theory-ladenness of observation. Like Nagel, Thrane misunderstands what it means. Had he understood it differently, as I will show, he might have been a better able to integrate all his insights regarding perception into his theory.

Before taking up these four points let us see how Thrane avoids certain obvious problems with a theory such as this. While he holds that what we really see are the patterns of light on the retina, he does not say that we see them _as_ on the retina. The well-known criticism that seeing retinal "pictures" would require another eye and so on, is side-stepped in this way. Similarly, seeing how the pattern is situated on the retina would require another eye. Thrane draws three basic conclusions about the patterns of light on the basis on this stipulation. First , since we do not see their setting, they are "free-floating." Second, they have no determinate third dimension. And, third, they are entirely "metric-free" ([2], pp. 10-11).

The patterns of light are not pictures for Thrane since the construction of a picture or seeing a pattern as a picture requires a great deal of knowledge. He leaves them indeterminate in an extreme sort of way. They cannot have a three-dimensional shape since the retina is essentially two dimensional and any representation in two dimensions can be generated by an infinite number of actual three-dimensional scenes. Being able to "disambiguate" a two-dimensional picture into a unique scene requires a great deal of knowledge ([2], p. 22). The same is true for the determination of size. In fact, Thrane says, there is good reason to believe that what we know about the world "informs our determinations of how things look" ([2], p. 23). But this is not a matter of what we "really see" for what we really see is indeterminate in the way that it must be if it is prior to alteration by knowledge.

AWARENESS OF WHAT IS SEEN

Thrane grants that we might object to his theory of seeing on the grounds that we are not aware of seeing patterns of light. The conclusion that this is what we see, he says, is the result of a highly theoretical argument, including among other things, projective geometry and optics

([2], p. 15). If this theory is to work there must be some sense of seeing in which we can say that the perceiver is not aware of that which is seen.

His argument is as follows: Suppose that Jones has successfully climbed a flight of unfamiliar stairs. We ask him whether he saw the last step and he answers that he must have, although he was not aware of it. Why does he answer that he _must_ have seen it? Because he did not stumble ([2], p. 16).

Similarly, he says, the reader of these pages must have been seeing his thumb all the while he was reading but he was not visually aware of it. Another example of this sort of thing is the apparent lateral motion of objects around us as we move. We are so unaware of this occurrence that we may be surprised when it is pointed out. Yet, we see it, and we can become aware of it when it is brought to our attention.

He will later argue, correctly I think, that we are sometimes inclined to employ the model of the conscious noting of evidence and the conscious drawing of conclusions to analyze seeing. But if this were an accurate analysis of seeing, or perception in general, it would make seeing infinitely more complex than it actually is ([2], p. 36). It would also make it slower than it is, he might have added, since conscious inferences of this sort take time.

If awareness of what is seen is not necessary to seeing it, what _are_ the necessary conditions? First, Thrane says, the object seen must be there. It must be visible from Jones' vantage point. He must be looking at it. And he must see the visible thing he looks at. In order to know the latter, we must know that he is conscious and that his eyesight is not defective. In order to know that the first three conditions obtain we need merely observe the object, the light source and Jones' eye. The judgment that he sees the object is admittedly inferential. If these conditions were realized it follows that he saw the last step, even though he was not aware of it.

I find Thrane's argument regarding Jones' ascent of the stairs not entirely satisfactory. It would seem that seeing something _and_ responding appropriately to it would always involve some sense of awareness of that thing. Is Jones' testimony that he was not aware of the last stair adequate to establish his lack of awareness as factual? Perhaps he was aware of it but, since there was no reason to deliberate on the stair or his awareness of it, he simply forgot about it. What is the success of his ascent, that is, not stumbling, evidence for? It would appear to support the conclusion that he was aware of the last stair as well as it supports the conclusion that he saw it but was not aware of it.

Suppose we make a distinction within the concept of awareness between a minimal sense in which we would be able to respond appropriately to objects that we see and a deliberative sense in which we could report that we have seen them. The behavioral sense of awareness would include much of our daily experience. This distinction would allow us to report that we drove to work successfully without the absurdity of adding that we were unaware of the traffic around us or the traffic signs and lights (when we do not remember them).

Does this distinction help in Thrane's case? Learning to see could be merely learning to behave appropriately in response to particular retinal patterns. Behavioral awareness might be sufficiently fundamental to be the same for all "normal" observers. But can he allow that Jones was even behaviorily aware of the last stair without negative impact on his theory? The point of his theory is to show that perception is possible with no input from theory or knowledge. And even responding appropriately seems to imply some knowledge of that to which we respond. Not stumbling on the stairs, for instance, suggests that we know something about stairs, that they are solid for one thing.

Further, if knowledge is implied in appropriate response, then awareness of that to which we respond seems that much more certain.

Instead of clarifying Thrane's theory for him this distinction brings up an additional problem for it. In the case of Jones' negotiation of the stairs we can say that he was behaviorily aware of them, <u>and</u> that if we call his attention to them that he can becom deliberately aware of them. The same is true with regard to the examples Thrane provides. We may not be deliberately aware of our thumb holding these pages or of the apparent lateral motion of objects when we move, but we can become aware of these things in the deliberative sense. But this is apparently not true of the patterns of light on the retina.

Could we, perhaps, train ourselves to be deliberately aware of the patterns of light? We <u>are</u> occasionally aware of our visual apparatus as a result of discomfort from bright lights and from "after images", for instance. But after images are not retinal patterns in Thrane's sense because they are not patterns of light at all. Discomfort and after images seem more like artifacts of the perceptual process or indications of its limitations than lessons in how to be aware of retinal patterns.

Are there other things in our environment that we are behaviorily aware of but of which we are never deliberately aware? There surely are things in our environment that we are incapable of perceiving without special instruments. But this alone is not enough, since <u>with</u> those instruments we are able to be deliberately aware of such things.

Perhaps instruments of this kind could reveal things in our environment which we had not been able to be deliberately aware of in the past, but which could be correlated with behavior. Suppose, for instance, that the presence of radio waves was found to correlate with an increase in violent behavior in a segment of the population. Would such a finding support Thrane's analysis of perception? It would represent an instance of behavioral awareness where no deliberate awareness was possible, at least prior to the development of the appropriate instrumentation. But in this case we would have the problem of identifying the sensory source for the behavioral awareness. In other words, instead of Thrane's unaware perception, we would have unperceived awareness.

There are other things in our environment such as background noises of which we are not usually aware in a deliberative sense. But even if such noises had always been there we could still imagine circumstances whereby we could become deliberately aware of them, by covering our ears, for instance.

It would seem that if one can be behaviorily aware of something that becoming deliberately aware of it would require only a shift of attention. We are frequently aware of our environment in only the behavioral sense, but I can see no reason to believe that there are some things that we can perceive only at this level or that there are some things that we can perceive with no awareness at all.

A more reasonable conclusion is that a minimum condition for perceiving something is that we are aware of it at least on the behavioral level and that we can become aware of it at the deliberative level.

THE THEORY-NEUTRAL APPROACH TO PERCEPTION

The fundamentalist approach to perception is an attempt to identify an object of perception at a level which is not influenced by theory or knowledge. Sense data theories aim at this goal as does Thrane's theory that what we really see are "patterns of light" on the retina. This approach to perception has the problem of explaining how perception at this fundamental level facilitates perception at other levels, such as the perception of common objects like tables and chairs.

To a theory like Thrane's one might merely respond that it is obviously false. It is perfectly obvious that what we see are objects and objects are outside of our bodies, not inside on our retinas. But this is part of what a theory of vision should explain, Thrane feels. He considers it a "surprising fact" that we perceive an object <u>as there</u> when we are <u>here</u>. Every theory of vision should be able to explain how we perceive a distant object as distant even though it is not touching our sensitive organs. It is a reasonable hypothesis, he says, that what we see is something that is touching our visually sensitive surfaces, patterns of light. In other words, seeing the surface of a distant object just is seeing a pattern of light ([2], p. 19).

The patterns of light "correspond" to the surfaces of objects, he says. By "correspond" he means to suggest an identity. That is, seeing the pattern of light is identical to seeing the surface of an object. He is not suggesting that the pattern of light is identical to the surface of the object for this would be contradictory on a number of levels, one being the invariance of the object's surface and the variability of the pattern of light, depending as it does on perspective. Instead, the identity is between seeing the pattern of light and seeing the surface of the object. What that means is that seeing an object's surface just is seeing a pattern of light ([2], pp. 20-21).

In other words, Thrane is offering a theory of "how we see objects in space, not a theory that we do not" ([2], p. 19). He is not saying that we see patterns of light instead of objects. Seeing patterns of light is merely the way we go about seeing objects.

He offers an analogy for this approach to perception, saying that one might hold that science has proven that objects have no color. When an object looks red, on this view, what is really happening is that it is reflecting light of a particular electromagnetic frequency. When that frequency impinges on the retina it causes the sensation of redness. But this does not prove that an object such as this has no color. Instead, it is an account of what it is for an object to have color ([2], p. 19).

Thrane obviously does not want to say that we do not see ordinary objects in space, but only that we see them by means of seeing something else, the patterns of light on the retina. But this "compound" approach to perception is troublesome. Do we really see one thing in order to see another? Is this understanding of perception commonplace, especially in regard to perception in scientific research? It is surely true that scientists often observe one thing by observing its effects on another. Tracks in a cloud chamber and Brownian motion are instances of this. In the cloud chamber a minute but visible quantity of condensation results from the passage of an otherwise invisible sub-atomic particle. In Brownian motion we witness molecular motion by seeing how tiny but microscopically visible particles react to it.

But these examples do not illuminate the aspect of Thrane's theory that the term "compound" was intended to indicate. In the examples the things seen are all outside the perceiver. Further, there are two objects in the world that are involved. In the cloud chamber we have condensa tion and sub-atomic paticles, and in Brownian motion we have pollen or some other tiny particles and molecules in motion. It is possible, at least in principle, to observe each of these objects apart from others. Sub-atomic particles can be observed through other effects as can molecular motion. But Thrane's theory is compound in the sense that there is only one object outside the perceiver, but two "seeings" (Thrane's term) involved in our coming to know about it. To explain one "seeing" in terms of another seems questionable. It is similar to explaining cohesion in terms of atoms with tiny interlocking hooks.

Would examples using other organs of sense be helpful? Do we hear a symphony by hearing the vibrations of our auditory apparatus? Do the hammer, anvil and stirrup of the inner ear reproduce the sound of the orchestra by striking each other? It might be possible to place a microphone inside the ear to see whether they reproduce the sounds that cause them to move. But is this really necessary? If they _did_ reproduce the sound, what organ would hear _that_ sound? We would need another ear, even if we did not hear the sound _as in_ the ear, just as others have argued that we would need another eye in order to see the retinal picture.

What is the point of the compound or fundamentalist approach? It is to establish a level of perception that is prior to the intrusion of knowledge. Would it be enough to say that there is at least one element of seeing, which is necessary but not sufficient to explain seeing, that occurs without the aid of knowledge? No, for what is sought is a source of the "data of experience" which is free of the influence of knowledge, and the data of experience can come only from actual, completed perception. Thus, the element that is identified must be treated as perception itself.

The inherent conflict in the fundamentalist approach is that we are not aware of the fundamental "object" of perception. The approach is forced into its compound position in order to explain how it is that we perceive the things of which we *are* aware.

THE IMPLICATIONS OF THE THEORY-NEUTRAL APPROACH

Thrane began his article with a clear statement of the meaning of the theory-ladenness of observation for the philosophy of science. If Hanson and Kuhn are right, he said, then what we see is altered by what we think. For this reason he thought it important to develop a theory which preserved vision as a source of data from experience which is not altered by what we think.

Having developed a theory which is sufficiently fundamental to be prior to the impact of what we think, he proceeds to define criteria which will determine the meaning that such a theory has for philosophy of science. These criteria are stated as types of "priority" that a theory of this sort might be expected to have. These types of priority are as follows:

1. The conceptually prior is that which we believe pre-theoretically that we see, tables and chairs and so on.

2. The perceptually prior is "that by virtue of seeing that we see anything at all."

3. The epistemologically prior is that whose nature we can know for certain merely by seeing it.

The conceptually prior is that which we would identify when asked, "What in general do we see?" The ordinary objects around us largely exhaust this category, according to Thrane. The conceptually prior is "pre-theoretical" and therefore pre-sciene. It is the level Nagel identifies as the "core" of observation terms.

The perceptually prior is obviously the pattern of light on the retina, according to Thrane. The point of these distinctions is to determine whether the perceptually prior is also conceptually or epistemologically prior. Is the perceptually prior also conceptually prior? It is not, Thrane says. The reason is simple, the things that are identified as conceptually prior are far richer than the "impoverished array" of patterns of light could ever support. His appreciation of the richness of the conceptually prior is surprising. It is a fact, he says, that "the agony is there to be seen in Picasso's Guernica, the serenity there to be seen in Claude Lorrain's The Herdsman" ([2], p. 36). The patterns of light, on the other hand, are so indeterminate that they defy description.

Why have so many writers assumed that the perceptually prior would also be conceptually prior, Thrane asks. The likely reason is that they assumed that we as perceivers will be aware of the "cues" and "evidence" upon which our knowledge is based. If this were true, he says, it would make our daily life infinitely more complex than it really is. The model that is being employed is the conscious noting of evidence and the conscious drawing of conclusions. Aside from the complexity issue, he has already argued that we are neither conscious nor aware of the patterns of light which are the "cues" and "evidence" level of perception. Since we cannot be aware of this level the model does not help to make the identification between the perceptually and the conceptually prior. In another sense, it does not matter that the model fails since the perceptually prior is far from adequate as the foundation, even by inference, for the conceptually prior.

But a more important issue is the possible identity between perceptual priority and epistemological priority. But this identity cannot be asserted either. Why not? Because the epistemologically prior as "the foundation of empirical knowledge, must be in those things the nature of which is apparent and certain." And the patterns of light, he allows, are "not there to be seen in their naked indeterminacy" ([2], p. 37). Now, this is a bit odd since the patterns of light were supposed to answer the question, "What do we <u>really</u> see?" What he most likely means to say is 1. That we are never <u>aware</u> of seeing them, and 2. That their extreme indeterminacy makes such awareness unlikely. If we are not aware of them their nature cannot even be apparent, not to mention certain.

An additional reason for the failure of this identity, Thrane says, is the fact that "much that we see is 'imposed' on" the patterns of light. That which is imposed could be mistaken. The level at which this imposition occurs must be the level of conceptual priority. And unlike Nagel, Thrane is unwilling to accept the "relative stability" that might be found at this level as adequate for epistemological priority.

Thrane's insistance that the epistemologically prior must be apparent and certain forces him to concede that it is probably not to be found anywhere. It is certainly not to be found in vision, he says, and vision is the source of our most refined knowledge of the world ([2], p. 37).

What does this mean for philosophy of science and the possibility of identifying a source of data from experience that is prior to the influence of the mind? If the perceptually prior is not the same as the epistemologically prior, then no such source is forthcoming.

Does this mean that Thrane's theory has failed? That depends on what you want from a theory of vision. If you want to justify our claims to know with the justification resting on a "sensorily self-evident base," then the theory has failed. But the theory had far more modest goals, he now says. Instead of aiming to justify knowledge on a self-evident base, its goal was to explain "<u>what</u> in part the evidential base of our visual judgments is" ([2], p. 35).

Thrane refers to Quine in supporting this goal for a theory of vision. It is sufficient, according to Quine, to seek only "the casual mechanism" of our knowledge of the external world.

Thrane's theory of vision has helped to clarify the conditions that a fundamentalist approach must satisfy. One condition, that it be grounded at a level beyond the influence of knowledge, now appears to place it beyond anything that could be called the "data of experience."

THE MEANING OF THE THEORY-LADENNESS OF OBSERVATION

Why is it so important to Thrane to establish a fundamental level of perception that is beyond the influence of knowledge? The answer lies in his understanding of what the theory-ladenness of observation means to him. What it means can be seen first in his response to Hanson and Kuhn. Both of them hold, he says, that what we see is <u>altered</u> by what we think. He fears that in accepting the theory-ladenness of observation one accepts the position that the data of experience somehow "belong to the mind," as Lewis put it ([2]), pp. 6-7).

He believes, as we have seen, that much of what we see is <u>imposed</u> on the patterns of light. He quotes Dewey, with apparent approval, saying that qualities other than those detectable by the eye are obviously controlling factors in perception.

In other words, the theory-ladenness of observation means the theory-<u>generation</u> of observation just as it did for Nagel. Since this seems incompatible with the testing of theories in science, a level prior to theory must be sought. And, further, if that level fails to satisfy all the demands of the testing relation, it at least identifies the "causal factor" in perception which <u>is</u> prior to knowledge. This is enough, he says, and he takes Hanson to task for not realizing "the importance of the retinal pattern" ([2], p. 31). If theory-laden observation means theory-generated observation then there seems little point to an investigation like that Hanson pursues. When Hanson tries to distinguish two senses of seeing, a sense in which Tycho and Kepler see the same thing in the east at dawn and a sense in which they do not, Thrane accuses him of equivocation ([2], p. 30). If the sense in which they do <u>not</u> see the same thing is due to theory-ladenness, then it is not significant for a theory of

<u>vision,</u> in Thrane's terms. It might contribute to an analysis of how the fundamental data of experience are altered or added to by knowledge, but a theory of vision must explain how we come to know what <u>is,</u> and what is surely cannot be altered by the caprice of the knower. His theory may not have accomplished all he had hoped, but at least it identifies something in perception which is not added by the mind. He and Hanson differ on the import of the retinal pattern because he sees it as the end point of an analysis of vision and Hanson does not.

But Thrane is not entirely oblivious to problems relating to perception that are of the sort that Hanson takes seriously. As indicated earlier, he grants that, "The agony is there to be seen in Picasso's <u>Guernica</u>, the serenity there to be seen in Claude Lorrain's <u>The Herdsman</u>". How is it that "agony" or "serenity" are there to be seen in the painting? "Although it is clearly something about the painted surface that makes <u>The Herdsman</u> serene, it is not easy to say what." About this issue he says, "I have nothing to say" ([2], p. 36). It is not to the point in any case, he says, since a theory of vision need only establish that which is perceptually prior for vision.

But like Nagel, Thrane is wrong about what the theory-ladenness of observation means. If it did mean that observation was theory-generated or even partially theory-generated then it would follow, as he says, that ordinary objects would have a rich range of "visible but not necessarily optical qualities" ([2], p. 37). This represents a conflict in Thrane's position. To suggest that we can see something that has no optical aspect would appear to be the assertion of extra-sensory perception. But this is not what the theory-ladenness of observation means. Theory-ladenness is entirely compatible with the belief that <u>all</u> of what we see is there in the object to be seen.

A theory of vision, or perception in general, must be able to explain how we can see such things as serenity. It must give a detailed account of what the theory-ladenness of observation means. As I will argue in Part II theory-ladenness does not mean that observation is theory-generated, even in part. Instead of treating the theory-ladenness of observation as an obstacle to be overcome, it should be treated as the beginning point in understanding the logic of observation.

In summary, I have argued that Thrane's theory of vision is flawed in that he is forced to argue for the possibility of seeing without being aware of what is seen. His argument fails to support that position. The mere fact that we do not deliberate on a particular perception does not mean that we are not aware of the object perceived. In fact, I have argued, awareness of the object is a minimum condition for perceiving it.

Further, I have argued that his fundamentalist approach carries the inherent flaw of requiring "compound" perception or two "seeings" for every visual perception. This flaw is the result of the position on awareness, which in turn is due to his misunderstanding of the theory-ladenness of observation.

I have argued, with Thrane's help, that the implications of this position point to serious limitations. It is too impoversihed to account for what we ordinarily think that we see, and it is too indeterminate to provide the foundation for science that empiricism has traditionally assumed was essential.

Finally, I have argued that Thrane's understanding of the theory-ladenness of observation is mistaken. He treats it as the theory-generation of observation. This explains why he feels compelled to avoid theory in his treatment of vision. It also explains the paucity of his theory. Much of what needs to be explained about observation must be left out if theory-ladenness is interpreted in this way.

The challenge offered by the theory-ladenness of observation is not to find a way to accept the claim that perceptions and that which is perceived are somehow fabricated by what we think. Instead, it should be taken as an insight into the problem of how we come to know the world. It will also help us to better understand theory and the relation between theory and observation.

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CHAPTER V

THE LOGIC OF DISCOVERY - HANSON

Hanson has often been grouped with philosophers such as Feyerabend, Kuhn and Toulmin as a proponent of a position which leads to epistemological relativism and an irrational characterization of science. Relativism and irrationality are thought to follow from the claim that observation is theory-laden, a claim which appears to destroy the empirical base of science, to make the comparison of competing theories impossible and to make the rational acceptance or rejection of a given theory impossible. Even among those such as Peter Machamer ([5]) who doubt that such consequences necessarily follow from the theory-ladenness of observation, the placement of Hanson in this group fails to raise an issue.

But Hanson should be viewed differently. What he offered in _Patterns of Discovery_ is the outline of a new concept of the rationality of science based on the discovery of scientific knowledge rather than its justification. In his introduction he insists that micro-physics be used as the model for philosophy of science for the reason that it continues to be a research science. To use finished systems such as planetary mechanics or optics is a mistake he says.

The point of research is not the rearrangement of known facts into more elegant formal patterns but is instead the discovery of new patterns of explanation. The program for philosophy of science that he proposes shifts the emphasis from theory-using to theory-finding, from the testing of hypotheses to their discovery. Instead of looking at how observation, facts and data are built up into systems of explanation, he proposes to examine the influence of those systems on observation, facts and data ([2], pp. 1-3).

I will argue that Patterns of Discovery constitutes an outline for the rationality of science based on the espistemological relations into which observation enters. I will also show how this analysis represents a philosophy or logic of discovery.

I will divide Hanson's work into three parts. First, I will examine his analysis of observation. The point of this analysis is to develop a sufficiently complex model of observation to be able to account for the things that we ordinarily report that we observe, e.g., that the animal before us is a mammal. Second, I will examine his treatment of facts. His primary concern here is the relationship between facts and the fact-stating language. This relationship has important implications for the objectivity of science. I will show that Hanson's account of this relationship disputes the traditional empiricist understanding of objectivity, but that it does not preclude the understanding of science as a rational process. And, third, I will deal with Hanson's treatment of theory. Two major points will be made here, first that the understanding of theory is inseparable from the understanding of observation. Secondly, the sense will emerge in which the examination of the epistemological relations between observation and theory constitutes a logic of discovery.

OBSERVATION

I have described Hanson's purpose with regard to observation as an attempt to develop a sufficiently complex model of observation to be able to account for the things we ordinarily report that we see. The need for a more complex model arises from paradoxical situations like the one confronting Tycho and Kepler in Hanson's example. Do they see the same thing in the east at dawn or do they not? Many would answer that they do, that any difference is due to alternative interpretations they put on the visual data. What they report that they see would not be the same, however. One might respond that regardless of their first person account what is actually going on is first the observing of an event by two men who then interpret it differently. In other words, they report their interpretation and not merely what they see. But this is at least paradoxical since both men, if asked, would likely say that what they have reported is exactly what they saw, even though their reports were different.

Do they see the same thing or not? This is not a de facto question, Hanson says, but it is rather the beginning of an examination of the concept of observation. To take this as a de facto question should be viewed as the refusal to participate in that examination. The reason for the refusal is most likely the feeling that one must insist that Tycho and Kepler see the same thing or else scientific knowledge based on observational evidence will lose any claim to objectivity.

There are other reasons why a more complex model of observation is needed besides the paradoxical character of first person reports such as those from Tycho and Kepler. One of them is the apparent failure to the "interpretation" explanation for the different reports. Hanson uses the gestalt example of the perspex cube to bring out the problem with this explanation. Most observers of this drawing are able to see it as a box viewed from above or as a box viewed from below. The shift, when it occurs, Hanson says, does not seem at all like a change in interpretation. For one thing interpretation is something with which we are all familiar. It is an intellectual process, e.g., we interpret a literary work, and it takes time. We might, for instance, say at one point that we are half through with our interpretation of a particular work, but we could say no such thing of our "interpretation" of the box from above or of the sun as a satellite of the earth. One might respond that interpretation in this case is instantaneous, but there is no ordinary or philosophical sense of the word "interpret" that fits this usage ([2], p. 10).

At this point Hanson offers his alternative model of observation. The "interpretation," he says, is part of the seeing itself. He calls this sense of interpretation the "organizational element" of seeing. The differences in the ways we see the perspex cube and the dawn are due to differences in organization.

Just what is this organizational element? It is _not_ another line in the drawing or another detail in the landscape. It is similar to the tune of a piece of music or the plot of a story. It is that which makes the details, notes or lines "hang together" in the way they do.

Is the organizational element something that is added by the perceiver or is it there to be seen as Thrane conceded that the agony is there to be seen in Picasso's _Guernica_ ([8], p. 36). An essential issue about the character of _empirical_ science is at stake here. If the organizational element is seen, it is not seen in the same way that the lines of a drawing are seen or the notes in a piece of music are heard. It seems to belong to the perceived object, and yet the perceiver seems to contribute something too. The _ad hoc_ interpretation formula does not appear adequate to explain this complexity in perception since such interpretation would surely be the exclusive property of the perceiver.

Hanson goes on with another example, the x-ray tube: Does the layman or the child see the same thing as the physicist when they look at the x-ray tube? He has already conceded that there is something common about what they see, but the physicist sees more than either the child or the layman. Is this because he has learned more and can provide an interpretation based on his knowledge? No, he does no more than they when he looks at the x-ray tube. Observation is what is happening for all threee, Hanson says ([2], p. 16).

Is the knowledge of the physicist relevant to this problem? Yes, it provides the context appropriate to such pieces of apparatus; it gives the physicist a pattern of concepts which relate x-rays to other forms of electromagnetic radiation as well as numerous other theories, problems and techniques. The layman and the child see the same lines, colors and shapes but they do not organize them in the same way because they lack the appropriate conceptual background.

Knowledge is also relevant to the sense in which the layman and the physicist do see the same thing. They both know enough about glass to know that if the x-ray tube were dropped it would probably break. Tycho and Kepler share even more knowledge about the sun and consequently organize what they see at dawn in much the same way. The sense in which they see the same thing is not at odds with the sense in which they do not see the same thing. What is seen is in part the "organizational element" and while that element is there to be seen in the object or situation, the perceiver must have particular knowledge in order to see it. When individuals see the same thing it is because they share knowledge that allows them to organize the situation in the same way. When they do not see the same thing it is because they do not share the same knowledge.

Hanson further clarifies the character of the organizational element through his discussion of "seeing-that." He describes "seeing-that" as a "logical element" which connects observation with knowledge and language. Seeing involves, at least, the having of knowledge of certain types. It is to see that if certain things were done to the objects we see, other things would result. Every perception, he says, involves an aetiology and a prognosis. To see an object x is to see that it will behave in ways characteristic of x's. If it does not behave in that way, we tend not to see it as a genuine x any longer ([2], pp. 20-22).

Observation could not have been any other way, Hanson argues. The formula presented earlier which makes the knowledge contribution an *ad hoc* feature which explains any differences in what we say about what we see. Knowledge on this view is not part of observation, but is used to manipulate observations. This model is too simple, Hanson believes. It would have us treat visual observing as the simple absorption of retinal pictures. But pictures are not adequate to account for the fact that we make <u>significant</u> and <u>relevant</u> observations. Why is this so? It is because of the way pictures accomplish their end. Hanson explains this by contrasting it with language.

How do pictures and language differ? First, Hanson says, they represent originals by <u>copying</u> certain of their aspects. The lines, shapes and colors of pictures stand in much the same relations to each other as the lines, shapes and colors of the originals. Language, on the other hand, does not represent or copy originals at all. Instead, language <u>characterizes</u> the original as of a particular type, and it <u>states</u> the relations that obtain.

Further, statements can be true or false but pictures can be neither.

Statements are also more versatile than pictures. It is said that a picture is worth a thousand words, but this is true only if what the words attempt to describe is picturable. A picture of a bear will tell us nothing about its growl, but language can tell us that and more about its texture, smell and habits ([2], p. 27).

Hanson describes the differences between language and pictures as "logical" in type. He wants to emphasize the gap between the simple model of seeing (as the absorption of retinal pictures) and significant or relevant observation. Seeing according to the simple model would be like a view through a kaleidoscope, for pictures like objects and events have no intrinsic significance ([2], p. 26).

The point of "seeing-that" is to bring observation and language together, or rather, to show that they are not separate. If we can see the significance of an object or event it is because our linguistic knowledge is part of our seeing.

Does it matter that the knowledge contribution to observation is not ad hoc in the way the interpretation formula would have it? From Thrane's perspective it matters a great deal since allowing theories to intrude in the realm of observation means that the data from the world is diluted by what we think and no longer genuinely empirical. But Hanson's model intends to do more than shift the temporality of the knowledge contribution to perception. Just as a student can learn nothing if he does not help, the perceiver can see nothing if he does not contribute to the process. The knowledge element is his contribution but it is not contributed in absentia from the observational situation. The knowledge contribution is more like gaining a better perspective from which to see the world than it is like laminating something onto observations of the world. The knowledge contribution of the perceiver is essential but it does not constitute dilution of the data any more than the effort of the student alters the lesson.

Microphysics, Hanson says, is the search for new organizational elements for observable data. If new modes of conceptual organization are found, the discovery of new entities will follow. Gold, he says, is rarely found by those who have not got the lay of the land. Microphysics is only secondarily the search for new objects and facts, although he adds, these two endeavors are "as hand in glove" ([2], pp. 18-19).

This is an interesting point in part because it is similar to Kuhn's suggestion that theoretical discoveries are often "open-ended" in the sense that they lead to further discoveries and new applications of those discoveries in previously unexpected areas. Why should this be true? For Kuhn it is largely a psychological matter. Past success provides the scientist with the assurance he needs to devote time and energy to further "articulation" of the paradigm. What this really means for Kuhn, as he says frequently, is that the scientist is willing to devote sufficient energy to "forcing nature to fit the contours of the paradigm."

Hanson's point is very different. His way of stating the connection suggests some kind of guarantee--that if a new conceptual organization is found then the discovery of new entities will automatically follow. But the more accurate statement of the case is suggested by the phrase "hand in glove." New conceptual organization always works intimately with the actual experimental or perceptual context. It could do nothing else. The discovery of new aspects of nature is part of what it means for new organizational elements to be considered successful. If they reveal nothing about the world that was not already known, they would be simply dropped.

The discovery of new aspects of nature is not guaranteed in any mysterious way by new conceptual organization. Instead discovery is part of what "new conceptual organization" means. To label one of these aspects of research as "primary" and the other as "secondary" is misleading. It leads to the suggestion of a guarantee where none is needed. Kuhn thinks that a guarantee is needed because he does not see theory as <u>essentially</u> related to observation. Theory is not part of observation for Kuhn but is an addition to it. It <u>is</u> part of observation for Hanson. So why does he separate the search for intelligibility from the search for entities? It is because he wants to emphasize the former. It is the <u>pattern</u> that the scientist is looking for, not new things. He is not a scout in the sense of one who is sent randomly looking for problems. Instead, he is more like a detective searching for relationships between his problem and other elements of the observable situation.

Recognizing that there is an organizational element in observation is the same as recognizing an aspect of the epistemological significance of observation to science. The more complex model of observation is the cornerstone of his philosophy of science. It is on the basis of this concept of observtion that his concept of the rationality of science is explicated. What he has accomplished here is a concept of observation that allows for the contribution of the perceiver without substituting that contribution for the input into perception of the world itself. It is for this reason that his philosophy of science does not preclude the understanding of science as an objective process.

FACTS

Hanson assumes all along that science is objective. His philosophy of science is not aimed at defending that point but rather it is concerned with understanding the rationality of science as a _process._ In order to appreciate the character of that process it appeared important to Hanson to determine what, if any, parts of the process were static. Particularly the concept of "fact" has been treated as if it were as irreducible and unequivocal as the world itself. This attitude is misleading, Hanson said, since facts are not observable, not even picturable entities. White was later to agree, pointing out that we can _state_ the facts but that we cannot see them ([9], p. 83).

The status of facts appeared significant for understanding science. How do they relate to observation, given that they are not observable? They are clearly related since what we observe determines the facts that we are willing to state about a given situation. Hanson's reason for dealing with facts is to show that, like observation, this concept is complex. Understanding it properly is important to a similarly complex account of the rationality of science.

The limitations or peculiarities of our language may tell us something about facts. The English adjectival idiom, for instance, tends to give a passive account of the properties of objects. "Grass is green," we say and "Bears have fur." But some languages express these properties in a verbal idiom: "Grass greens," and "Bears fur," suggesting activity. The facts that are expressed in the latter idiom are not exactly the same as in the former. Certain conclusions will follow from one statement of the situation that will not follow from the other. To say, "The sun rounds," instead of, "The sun is round," is to suggest that the sun is constantly arranging itself in a sphere, Hanson argues ([2], p. 34). This is very much what fluid mechanics suggest that liquids do in even gravitational fields. To say that the sun <u>is</u> round misses this active aspect of its shape.

There may be, Hanson goes on, many things about ordinary situations that elude our current language. If our language had been different we might have come to think about the world differently, to see different aspects of it and to know different facts about it ([2], p. 35).

This is not to say that the world might have been different, he says. "Given the <u>same</u> world, it might have been construed differently" ([2], p. 36). Theory, language and knowledge have importance in the way we see our world, but this admission is fully compatible with an empirical characterization of science. Empiricism does not require a simple model of observation.

THEORY

The more complex account of the rationality of science offered by Hanson stems from two sources. First, it begins with a more complex account of observation, and second, it analyzes a process instead of a product. These two points are related since the process is explicated in terms of the dynamism within observation. This comes out in Hanson's discussion of theory.

The notion of "dynamism within observation" makes sense because of the complexity of observation, that is, because it involves an "organizational element" as well as a contribution from the world. The ways in which those work together and evolve together represent the evolution and growth of science, i.e., the development of scientific knowledge.

The failure to appreciate the complexity and dynamism of observation has resulted in misconstrual of the nature of scientific theory, Hanson argues. The isolation of causes by science represents such a case of misconstrual. Science sometimes explains a problematic phenomenon by showing that it is caused by a better understood phenomenon. Russell, among others Hanson says, assumed as a result that causes represent something very like chains of sense experience that can be traced from any point backward to the beginning of time ([2], p. 50).

Again, the issue is complexity. To name a cause is to give an explanation. And the giving and understanding of an explanation may presuppose a great deal of knowledge. This is not to say that causes are not observable. Since observation always involves an "organizational element" the requisite knowledge for understanding a causal explanation also makes it possible to see the cause. We do commonly see the causes of events in our environment, but there are also events whose cause escapes us.

The point is that the determination of a cause is or may be as complicated as the development of any explanation in science. In fact, there may be many causes for any event, as many as there are reasonable explanations of it. When we attempt to determine the cause of even the most ordinary events, the knocking over of a chair, for instance, we often find ourselves in the process of listing numerous factors which contributed to it. The chair had been moved out of its usual place, the lights were dim and the package I was carrying could all be offered as "causes" of the accident.

Further, Hanson says, cause words are as theory-loaded as words like "finesse" and "offside." Without some knowledge of bridge and football these terms mean nothing. Neither can we observe the events they refer to without knowledge ([2], p. 57). That one billiard ball is the cause of the motion of another is <u>obvious</u>, but only because all of us know enough about the elastic properties of such bodies to know that, e.g., they will not stick together or merge like water droplets on contact.

The chain analogy misses the significance of causality in science in another way. The chain relation suggests a sort of equality between cause and effect which fails to illuminate the explanatory power of causality in science. To say, for instance, that I knocked the chair over because it was out of its usual place is to provide an explanation in only a very modest sense. This may be all that is required to explain a causal accident, but it would not be adequate for science. It does not afford a <u>pattern</u> from which I can predict future events or link this phenomenon with other phenomena not previously known to be connected (except in the same modest sense that other chairs or other objects inappropriately placed . . .). Causal connection in science is diagnostic and prognostic, Hanson says. The description of the cause must be at a different logical level from the description of the effect to perform these functions ([2], p. 60). Otherwise the explanandum could not be deduced from the explanans in a scientific explanation.

This insight concerning logical levels and the explanatory relation between cause and effect helps to clarify another concept relevant to scientific knowledge, the notion of "necessary connection." Causes are certainly connected to effects, Hanson says, "but this is because our theories connect them, not because the world is held together by cosmic glue. The world _may_ be glued together by imponderables but this is irrelevant for understanding causal explanation" ([2], p. 64).

This comment is interesting for three reasons. First, it tells us the source of the necessity in scientific explanation. It is the necessity of the syllogism. Given certain premises the conclusion _must_ follow. This allows a view of scientific theory which is compatible with the history of change in science. When one theory replaces another it is not because the older theory has been exposed as having falsely isolated necessary connections in nature. Necessity does not reside in nature at all, Hanson is saying.

Second, it tells us something about what we should expect of causal explanations in science, what is missing in the "modest" causal explanation above. Causal laws are not built up by merely noticing that A's are followed by B's repeatedly and then summarizing this observation under the umbrella formula, "All A's are followed by B's." Exceptions to such laws would raise no conceptual issue. There _are_ exceptions to causal laws in science, Hanson admits, but they also raise conceptual issues. They cause the pattern of our concepts to "warp and crumble" ([2], pp. 64-65). There are ways to save favored patterns from warpage, as Hanson surely knows, but these too raise conceptual issues.

And, third, Hanson seems in a certain way to have overstated this point. The connection identified by a theory or law between two events is not there _merely_ because our theory or law connects the statements that describe them. If theories are to be cast as empirical, they must tell us about the world. It is important to identify the source of any supposed necessity in causal laws, but it is also impotant to understand the sense in which laws and theories tell us about the world as it is. Much of Hanson's discussion of theories is aimed at making just this point.

Whether causal connection is left solely in the realm of statements is important. An essential feature of Hanson's philosophy of science is his attempt to bridge the gap between statements and experience. That is why he began his philosophy of science with an analysis of observation, culminating in his discussion of the relationship between language and seeing. If the two are ever separated, it is indeed difficult to get them back together. His method of "bridging" the gap was in fact the demonstration that they were never separate.

The chain analogy is deficient in the additional sense that it fails to appreciate the genius that is required of a Galileo or Newton in accomplishing their explanations of nature. When experiments appear chain-like it is because they were designed to appear that way. It is the chain-like character of logic and not of objects or events in the world.

Experiments are designed to direct attention to a particular sequence of events, Hanson says, and philosophers who dwell on those events miss what is involved in directing attention in this way. Nature, he says, must have been tampered with to achieve this end. One way in which nature is tampered with is by holding all but one variable constant ([2], pp. 66-68).

Philosophers who focus on the spectacular event, usually the impressive conclusion to a lengthy experiment, fail to appreciate what the scientist had to do in order to expose its spectacular character. It is not that they generated the event for this is not the sort of tampering that scientists engage in. But the scientist did have to design its exposure using a theoretical background against which its spectacular character could be seen.

Focusing on the spectacular is equally likely to lead to the conclusion that the event was merely there to be seen by any passer-by.

This discussion of causality sets the stage for understanding the process whereby theoretical explanation is achieved. Again, Hanson's point is to characterize the rationality of science through the examination of discovery. He proposed to do that by looking at theory-finding, at the influence of theory on observation, facts and data. Everything that he has done to this point is in the service of explicating discovery. In order to understand discovery, the complex character of observation must be appreciated for discovery will involve new observations in old and familiar landscapes. We must appreciate the relation between facts and the fact-stating language, because discovery will also result in the statement of new facts, or, at least, the statement of facts that had not been stated. It is also essential to understand that we are looking for an explanation when we look for a cause, and to appreciate the applicable sense of "necessity" if "necessary connection" is used to describe scientific theory.

It is against this background that Hanson gives the details of the dynamics of theories and the rationality of discovery. Philosophy of science, he says, has typically provided two accounts of theories, the induction by enumeration account and the hypothetico-deductive account (H-D). They are different but compatible. The H-D account tells us what laws and theories do--serving as higher order propositions in a deductive system. The inductive account tells us how they are arrived at--by enumerating particulars.

There is something wrong with each account, according to Hanson, and something right as well. Scientists do not come up with theories by enumerating and summarizing data. And as a description of research H-D fails as well since scientists do not start with hypotheses, but instead start from data ([2], p 70). The inductive view is correct in its starting point, but it misses the critical point that a theory must explain why something occurs, instead of being a summary account of what occurs. The H-D account included the explanatory character of theories, but it left out any reference to the connection between data and those theories. The reasoning H-D takes as fundamental, from higher order propositions to lower order propositions, will englighten our reasons for accepting an hypothesis after it is got, but it tells us nothing about the reasons for proposing the hypothesis in the first place ([2]), p. 71).

While such proposals may require genius, their genesis is of more than psychological interest.

This is Hanson's introduction to the problem of theories in science.

If theories are freely created with their only empirical connection provided after the fact in the form of deducing observational propositions, then their creation will be difficult to understand or trace. But if they are responses to an observable environment with empirical connection at every step, then their creation may be subject to logical analysis.

The logical form of this process is retroduction or abduction, according to Hanson. He employs Peirce and Aristotle in support of this "form of inference." It differs from both induction and deduction, neither of which can originate any new idea whatever. While induction tells us what _is_ the case and deduction shows what _must_ be the case, abduction tells us what _may be_ the case ([2], p. 85). What may be the case is given in the form of hypotheses which provide a new pattern or background against which the data could make sense.

That an hypothesis of this sort is not achieved by induction is suggested in the way statements are falsified. If a bird-antelope drawing has four lines added to it we might say that it is a drawing of a bird with four feathers. About the number of feathers we could be wrong; the way of deciding whether we are wrong would involve a simple count. But about whether the drawing is of a bird, we could not decide in the same way. Pattern statements such as, "It is a bird," are different from detail statements such as, "It has four feathers," in ways that inductive summaries such as, "All birds have four feathers," are not different from detail statements such as, "This bird has four feathers." The inductive summary and the detail statement could be falsified in the same way--a simple count would suffice.

Both pattern and detail statements are empirical, Hanson says, but not in the same way. What he has offered is an interpretation of what it means to say that a statement in science is empirical. His interpretation is richer than that common to empiricists such as Nagel and Thrane. The theory-ladenness of observation, as I understand it, represents evidence for the claim that theories in science are empirical. It is not evidence against the claim that observation statements in science are empirical. How do I reach this conclusion? Let us examine the assumptions that are involved in each approach. Why does theory-ladenness appear to threaten the empirical character of observation statements: The answer lies in the sense in which theories are assumed to be created. For Nagel, Thrane and Kuhn theories are "freely" created by the scientist. How they are related to experience or reports about experience is never specified. Hanson, on the other hand, assumes no such freedom in the creation of theories. For him theory creation is always intimately in contact with observation. As a result his account of theory creation represents no threat to the rationality of science because it is not a non-rational process.

How _are_ theories created, according to Hanson? First, they are generated _in context_. This is hardly a new insight since people unfamiliar with scientific contexts rarely make scientific discoveries. In order to see how theories are generated we must appreciate the ways in which they are related to that context.

Theories are intended to provide a pattern or conceptual framework within which observable phenomena make sense, Hanson says. They also make possible the observation of phenomena as of a certain type and as related to other phenomena in understandable ways ([2], p. 88). Any pattern which appears to have the potential of making a problematic phenomenon explicable as a matter of course is a potential theory. Inductive accounts of theory generation, like the "modest" causal explanation presented earlier, cannot illuminate the capacity of scientific theories to explain why something occurs. The process of theory generation will be developed in greater detail in connection with Hanson's discussion of classical and modern physics.

The significance of Hanson's approach to theories can be seen in at least three ways. First, it develops an aspect of science that has been ignored. Research is the major activity of science, but previous characterizations of science by philosophy of science have failed to provide any acceptable account of how discovery comes about. This omission was justified by the claim that discovery was of psychological interest only. Hanson has tried to show that discovery is a rational process, proceeding in a step-wise or walking fashion. The scientist always has one foot firmly grounded in observation before the second foot attempts, through hypothesis to make further contact with the world. He need never "jump" from data to theory.

This account of theories is significant for the second reason that it makes theories empirical both in their genesis and in their function. They tell us about the world in a way that is at least the equal of observation statements.

And, third, Hanson's treatment of theories leaves no "gap" between theories and experience. "Correspondence rules" and "bridge laws" are not needed in his approach, giving his philosophy of science an economy and relevance that is missing in the other accounts of science that have been examined here.

But Hanson's point in discussing theories in relation to the H-D and inductivist accounts is not merely to criticize them but also to identify where they were correct. Induction was correct in trying to give a rational account of theory generation and H-D was correct in assuming that theories and observation statements were deductively related. The recognition of the deductive relation is important because it establishes the logical sense in which theories provide a context within which problematic phenomena become the expected. This is the point of Hanson's attempt to formalize abduction ([2], p. 86). But as Harold Brown points out, any such attempt is destined to be paltry since formal logic is concerned only with formal relations, with no concern for content ([1], p. 134). A dialectical logic, which Brown wants to pursue, is concerned precisely with the context of science in its historical setting. He argues that philosophy of science should abandon "absolutist" epistemology (logical analyis based on an irreducible empirical foundation) in favor of historicism and relativism ([1], p. 152). The consequence of this shift, he admits, is that philosophy of science must give up any sense of correspondence in its theory of truth and re-define objectivity to mean "non-arbitrary" ([1], pp. 153-154).

CLASSICAL PARTICLE PHYSICS

Part of the value of Hanson's work lies in his attempt to define a sense of rationality between these absolutist and relativist/historicist extremes. The structure of that rationality is given flesh in his discussion of classical particle physics and elementary particle physics.

There is more to be said about the relation between theory and observation than is contained in the reference to deduction or in the historical context approach of Brown. It has long been recognized, for instance, that some of the laws of classical physics are used in such a way that disconfirming evidence is conceivable and some are not. This presents a problem for some philosophers who argue that physics must keep in touch with experience in the sense of always being falsifiable. Hanson responds that "the orderings of experience are limitless; we force upon the subject matter of physics the ordering we choose" ([2], p. 98). What he appears to mean is that, having chosen a particular ordering of experience, we see the world according to the pattern it provides. The fact that we may not be able to conceive of another pattern at a particular time does not count against the empirical character of a theory. In order to be empirical it must provide the background against which observational details make sense. On the contrary, the fact that some laws of physics appear to be functionally *a priori* represents testimony for the power of the patterning function of theory. They do their job so well that, having accepted them, we find it difficult to conceive of any other way of making sense of this aspect of nature.

But no scientific theory has ever been *a priori* in the sense of having been generated prior to or apart from experience.

Falsification and falsifiability are concepts which have the potential for shedding light on the rationality of science but they are not touchstones of empiricism in the naive sense suggested in the concern over the functional *a priori*-ness of some theories. Non-falsifiability may not count against the empirical character of a theory, but contrary evidence may not result in a theory being rejected either. The entire system of science is empirical, Hanson believes, and as a result contrary evidence counts against the system as a whole. Naive falsificationists would have such evidence count against the fundamental tenets of the science, but to reject them when confronted with such evidence would amount to refusing to think about this part of nature at all ([2], p. 103). A more reasonable attitude is to take the contrary evidence as counting against the system as a whole--it did not apply where it might have. The "hard line" of the naive falsificationist may be due in part to a misunderstanding of the location of "necessary connection" in science. If a theory has failed to isolate necessary connection in nature then it is untrustworthy and should be rejected. But, as we have seen, the function of theory is not to isolate necessary connection in nature.

Further, as Hanson has alreay pointed out, the falsification of theory is not accomplished in the same way that it would be for an observation statement. Theories provide patterns for observable data and they may succeed in doing that even when particular bits of observable data that were expected to fit into that pattern fail to do so.

The point is that the patterning relation between theory and observation explains how one goes about seeking a theoretical explanation. It is understandable on the basis of the complex character of observation and facts discussed thus far, and it provides the basis for assessing the relevance of falsification to the rationality of science.

Whether one subscribes to this as the best or only way of understanding the rationality of science is not the point. Rather, the point is that Hanson has presented an integrated system whose purpose is to present in outline form the rational structure of science as research.

Hanson's discussion of elementary particle physics helps to further articulate the character of the patterning relation between theory and observation.

ELEMENTAL PARTICLE PHYSICS

Elementary particle physics has been thought by some philosophers to present special problems for philosophy of science. For instance, ultimate matter seems to be characterized in such a way that it is in principle unvisualizable or unpicturable. This is an insight, according to Hanson, which lays bare the essence of explanation in science. Instead of presenting a special problem for philosophy of science, it helps us to understand the character of science in general.

What is it about unpicturability that is essential to scientific explanation? It is that an explanation must not rely on that which requires explanation. Various attempts at explanation have been rejected for this reason. The suggestion that crystals might be explained by a reference to a brick-like structure, for instance, was rejected because the bricks would then have to be invested with those properties of crystals that require explanation. Similarly, explaining cohesion with "hooked atoms" fails to explain, Hanson says. This is part of the promised account of how it is that theories are generated. Scientists, in order to be successful, must understand this and more about the structure of explanation.

Atomic theory attempts to explain visible or picturable properties. It must do that by reference to something which does not possess those properties ([2]), p. 120). E.g., if atomic theory is to explain the color and odor of chlorine it must do so without endowing atoms with either color or odor.

The classical concept of the atom with its postulated properties such as impenetrability, homogeneity and sphericity is no longer adequate to pattern the data of physics with its array of sub-atomic particles, Hanson says. The properties of these particles are "discovered and (in a way) determined by the physicist." He ascribes properties to sub-atomic particles which he hopes will support inference to the phenomena he has observed. An intelligible conceptual pattern is the goal ([2], p. 123).

In what way does the physicist determine the properties of sub-atomic particles? By choosing only those that are explanatory, must be Hanson's point. The whole story of micro-physics, he says, is that the sub-atomic particles show themselves to have just those properties which they must have in order to explain the phenomena which require explanation ([2], p. 124).

Only when the quest for picturability was dropped was the essence of explanation in science laid bare. As Hanson recognizes, however, this is not the only essential feature of scientific explanation. Explanation in science must <u>unite</u> phenomena that might otherwise have been anomalous or wholly unnoticed ([2], p. 121). A theory must be concerned with more than a particular phenomenon or a particular property of a particle in order to constitute a <u>pattern</u>. It must connect with other phenomena in order to avoid being merely <u>ad hoc</u> in the way that epicycles were in Ptolemy's astronomical theory.

Unpicturability does not present a problem for the real existence of such particles. Intelligibility, Hanson says, demands that they exist ([2], p. 123). In other wods, unpicturability does not count against the empirical character of micro-particle theories. These particles have just those properties they must have in order to explain problematic phenomena. Such properties are not postulated at random but are responses to actual laboratory situations. They are attempts to explain problems that, as a type, are picturable and have therefore to be, as a type, unpicturable.

The same is true for other supposed special problems in modern physics such as the absolute identity of atoms and sub-atomic particles as well as the uncertainty principle. Observations forced physicists to construe the world with the help of these principles in order to make sense of the data ([2], p. 131). Had the world been different such ideas might never have been formulated. They are justified in every experiment in quantum physics since those experiments would not make sense without them. They are parts of "interlocked and systematic accounts" of the behavior of complex bodies, Hanson tell us ([2], pp. 134-136).

Why is picturability such a pivotal issue? Hanson emphasizes one reason, that it reveals the essential feature of scientific explanation that such explanations must not rely on that which requires explanation. It also brings out the other essential feature that scientific explanation must put the problematic phenomenon in the context of other phenomena. This helps to distinguish scientific explanation from the "modest" sort of explanation of why the chair was upset. The latter was modest because it did little to connect this phenomenon with any others. The latter was modest because it did little to connect this phenomenon with any others. The law of gravity, on the other hand, connects the falling of the chair with planetary orbits and other phenomena whose relation to the chair would have been unthinkable without it.

Similarly, inductivist approaches are by nature concerned with particular types of phenomena and are even less likely to make the sort of connections that Hanson's notion of "pattern" aims to illuminate.

The picturability issue has the added significance of shedding light on the problem of circularity that plagued Nagel. I contended in discussing this problem that circularity was not a serious problem in science. It appears to be a problem if theories are seen as "free creations" of the mind and the theory-ladenness of observation, in turn, is interpreted as (at least) partial theory generation of observation. But if theories are not free creations, I argued, but are responses to the environment of the scientist such circularity is unlikely. Now we can see why. An explanation must constitute a pattern within which the problematic phenomenon makes sense along with other phenomena. The two essential features of explanation help to clarify the problem of circularity. The first, the requirement that the explanation not depend on that which requires explanation, makes Nagel's concern unnecessary. That an observation is theory-laden does not mean that the theory <u>constitutes</u> its meaning. Atomic theory can explain the color of chlorine but it does so by reference to principles that do not include color. In this case the color of chlorine would count as genuine evidence for the theory that intends to explain it. Such evidence would not be adequate to win acceptance of the theory, but it would not be circular either.

The only genuine sense of circularity arises because of the other essential feature. The explanation must put the problem in a context along with other phenomena. Ptolemy's epicycles were circular in this explanatory sense because they explained only the apparent retrograde motion of some planets. In other words, if circularity occurs it occurs because the explanatory relation is too narrow.

For Nagel circularity is a problem of the empirical character of observation. Instead, it should be seen as a problem of the empirical character of theories. A theory like Ptolemy's which can explain only the problem at hand has questionable empirical status. It does poorly what theories actually do in science. Theories provide the conceptual backgound against which observable phenomena make sense. For background to <u>be</u> background it must be wider than a particular problem.

Does the theory-ladenness of observation mean that the meaning of observation terms and statements is determined by theory? Yes, but only in the sense that the meaning of a term or statement is determined by its context and the other terms and statements to which it is related. Does this create a problem of circularity? No, the problem of circularity arises when theories <u>fail</u> to provide such a context, as was the case with the theory of epicycles.

HANSON'S CRITICS

Finally, it should be pointed out that criticism of Hanson has often missed the point. Carl Kordig, for one, misinterprets the point of Hanson's work, treating it as if it were framed by traditional concerns and assumptions about objectivity. Kordig believes that he has accompished a <u>reductio ad absurdum</u> when he argues that if seeing x requires knowing certain of x's properties, then one could not change one's knowledge state with regard to those properties and still be said to see the same x ([3]), pp. 457-459). This argument has force only if one's concept of objectivity is based on the content of observation reports, as Kordig's is, but it has no impact if an alternative basis of objectivity is presupposed.

It is surely true, as Kordig says, that we can see a lamp without knowing that it is our maiden aunt's favorite possession, but it is also true that we cannot see the lamp without the aid of theory or knowledge. This does not create a problem for our concept of observation if we take it as an insight suggesting a direction for research as

Hanson did. If we assume that the answer to that research has already been given it creates serious problems.

Another award-winning paper taking Hanson to task was written by Paul Tibbetts. He argues that Hanson fails to give adequate emphasis to the distinction between reports of seeing as discrimination and reports of seeing as interpreting. Seeing as discriminating, he says, is nothing more than "describing or discriminating a figure x <u>relative to a background</u> y, rather than describing some property or feature of x per se . . ." ([7], p. 151). Such reports, having to do only with such things as change in direction and size, are theory-neutral. The problem with Hanson, on his account, is that he failed to give sufficient empahsis to this level of observation reports and consequently reached the inaccurate conclusion that there are no theory-neutral observation statements.

Tibbetts is wrong on two counts. First, the level of seeing as discriminating involves knowledge even if it is at a level that is unlikely to be contested. But, second, he fails to see that Hanson is offering an alternative basis for understanding the rationality of science. The problem of theory-neutrality is important to Hanson only because it suggests the need for a better understanding of observation in relation to science.

What Hanson has done is substitute an analysis of observation for certain assumptions about observation. He does not assume that observation is objective because of the possibility of consensus on the content of observation reports, and therefore he need not seek a level at which that content is theory-free. Both Kordig and Tibbetts continue to assume that the objectivity of observation is based on observation reports and they structure their arguments to show the error of failing to incorporate this assumption into one's philosophy of science.

What is needed at this point is to make explicit the concept of the objectivity of observation that an analysis of observation such as that given by Hanson can support. That concept will be based on the character of observation rather than on the content of observation reports. That is the task of Chapter VI.

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CHAPTER VI

THE DISCOVERY APPROACH

I share with the philosophers I criticize the belief that science is empirical. What I find lacking in their work is an adequate account of how it is possible for science to be empirical. The works of Kuhn, Nagel, and Thrane represent a dilemma. If observation is taken as a theory-laden endeavor, the empirical character of scientific knowledge becomes a problem; both Kuhn and Nagel attempt to accommodate theory-ladenness to philosophy of science, but they fail to show how observation that is impregnated with theory can offer evidence for or against theory. On the other side of the dilemma is Thrane who defends a theory-neutral account of observation only to find that on his account observation is irrelevant to epistemology.

The alternative I offer is the discovery approach. This approach assumes that the scientist begins with the observation of a problematic phenomenon, an anomaly or malady of some kind, and seeks an explanation whereby this phenomenon becomes non-problematic.

The alternative discovery approach will be developed and defended in five steps. First, I will argue that the goal of discovery is explanation or theory. I do not deny that entities are discovered in science, but I will argue that one can make better sense of science by pursuing discovery as the discovery of explanation. I will show, for instance that the discovery of entities can be accounted for, given this assumption, while the reverse is not the case, and also that the sense in which "accidental" discoveries are genuinely accidental can be explained with this assumption. I do not assume that all explanations are scientific theories, but I will assume that all scientific theories are explanations. As Karl Popper puts it, "Theories are nets cast to catch what we call 'the world': to rationalize, to explain, and to master it" ([6], p. 59).

Second, I will argue that observation is theory-laden. As will become clear in my arguments, I do not mean by "theory"-laden that all observation is informed, directed or somehow loaded with scientific theory. "Knowledge"-laden might be a better term for I would count fundamental knowledge such as colors and shapes to be sufficient to result in theory-laden observation. I am aware that some consider this sense of theory-laden observation "trifling" ([3], p. 176), but my argument will show that, with regard to the empirical nature of science, it is not.

Third, I will give a detailed analysis of what it means to say that observation is theory-laden. The purpose of this analysis will be to define precisely the contribution that is made to observation by the environment as well as that made by the observer. This is critical to understanding the sense in which science is empirical. The theory-ladenness of observation created a problem in philosophy of science because it appeared to erode the evidence character of observation. Does observation remain a source of independent information about the world or must it be understood as "mind dependent" in light of theory-ladenness? Observation is clearly mind dependent in the sense that it could not occur without a mind, but a more serious sense of mind dependence such as "contamination," "dilution," or "alteration" is the more usual concern when this issue is raised. My concern in this section will be to show that observation can be treated as theory-laden without precluding the possibility that observational evidence is objective. That is, I will show that mind dependence in the second sense is not a consequence of the theory-ladenness of observation.

In section four, I will argue that this interpretation of the theory-ladenness of observation has powerful implications for philosophy of science. First, it will provide the basis for a clear description of the theory/observation distinction. The distinction will not be collapsed but neither will it be treated as representing a difference in levels of empirical or theoretical content.

This re-interpretation of the theory/observation distinction will provide the basis for treating the problem of "unobservable entities" in science, for the meaning-dependence of observation terms on theory and for the problem of circularity in the evidence-theory relation.

The positive characterization of theory developed in section three and the beginning of this section will be used to support a re-interpretation of the justification/discovery distinction as the discovery-justification continuum. It will also be shown to aid in solving the problem of the non-rejection of theories in the face of counter-evidence from falsification theory.

In section five, I will argue that a theory of scientific truth must include elements of correspondence, coherence and pragmatics.

Finally, in section six, I will take one more look at the positions taken by Kuhn, Nagel and Thrane. I will argue that the error common to all of them, as well as to Popper and Scheffler, is the failure to analyze observation.

DISCOVERY AS THE DISCOVERY OF THEORIES

That discovery in science is the discovery of theories is by no means a unique view. Popper describes discovery in science as "the act of conceiving or inventing a theory" ([6], p. 31). Hanson, of course, has the same view, suggesting that science is "primarily a search for intelligibility," or the seeking of "new modes of conceptual organization," which, when it is successful, will be followed by the discovery of entities ([4], pp. 18-19). It would be better to say that the discovery of entities often signals success in the search for intelligibility, but the point here is to distinguish the options. The scientist either sets out to discover new entities or he sets out to discover theories. I will argue that the latter makes better sense.

The starting point of the discovery process is important in deciding this question. The scientist always begins with the recognition of a problem, that is, with a problematic observation. The sense in which an observation is problematic may vary. An observed measurement may not conform to prediction (e.g., the total energy released from a sub-nuclear reaction may be less than predicted); an unfortunate event may be observed, the cause of which is unknown (e.g., a recurring set of disease symptoms); or a phenomenon may accompany an experiment which the operative theory does not explain (e.g., Roentgen's glowing screen for which then current electro-magnetic theory could not account).

Having begun with an observed problematic phenomenon, the scientist seeks a context within which the phenomenon no longer appears problematic. This does not necessarily mean that we will look for or find a new entity. In the case of a physical illness, he may find that its cause is an already known agent. He has solved his problem by locating it within the aegis of the theory of a particular disease agent. New sets of relations are discovered in such a case instead of a new entity.

In an instance such as unexplained energy loss an entity may well be sought, but even then the course of research can best be understood in terms of the operative theory. It is that theory that will tell the researcher the sort of entity that is likely to be responsible for this quantity of energy under these conditions. Without such guidance he would know neither where nor how to look for the entity.

The alternative, to assume that the goal of reseach is entities, would seem to leave research without a context. It would make of science a sort of "prospecting" where the most successful scientist would be the one with the best luck, who happened to look into the corner of the universe that was richest in unknown entities. Even if such behavior accurately characterized scientific research, it would not explain the development of intelligibility that science achieves. Each new entity would have to be placed, _ad hoc,_ into the explanatory structure of science. We might expect that process to be rather far behind the work of the "entity prospectors," with a constant backlog of things waiting for a place in the system of science. But this does not coincide with the order of events in science where entities and explanations seem to come along hand in hand, but with the explanation commonly leading the way, if only in the form of an hypothesis that is on its way to becoming an accepted explanation. "Acceptance" often corresponds with the discovery of the entity predicted by the hypothesis.

This is not to say that new phenomena are never encountered for which an explanation is lacking. Roentgen's discovery of x-rays is perhaps the best known case of such an event. But from an epistemological perspective the discovery of x-rays is not different from the discovery of a virus. A problematic phenomenon (a glowing screen or a disease symptom) is observed and an explanation for it is sought. When the explanation is found, (in each of these cases the explanation involved the existence of a new entity) an entity is discovered.

The discovery of x-rays was no more accidental than any other in science. The visual sighting of a glowing screen in the presence of the cathode-ray tube could not be called the discovery of x-rays since at least one other researcher had seen the screen without making anything of it. X-rays were not discovered until they were placed within the context of electromagnetic theory.

The term "problematic phenomenon," as indicated, covers both the unexpected or new phenomenon as well as a well-known problem such as a particular illness. To describe the discovery that follows from the first (e.g., x-rays) as "accidental" and the discovery that follows from the second (e.g., a virus) as "non-accidental" is to disguise what they have in common. In each case the discovery involves the theoretical context that makes them intelligible.

ARGUMENTS FOR THE THEORY-LADENNESS OF OBSERVATION

The problem of this thesis is not so much whether observation is theory-laden as it is the best philosophical approach to science. I believe that the best approach is from the perspective of discovery, and one of the reasons is that this approach involves the analysis of observation. Since the movement of discovery is from the observation of a problematic phenomenon to a theoretical explanation, it requires that we understand how observation and theory relate. In the process of investigating that relation Hanson saw that theory was part of the observation process itself and he labeled that discovery "the theory-ladenness of observation."

The importance of analysis of observation lies in the fact that it provides the opportunity to specify the sense in which theory contributes to the observation process, thereby making clearer the sense in which observation can provide evidence for or against theory. I.e., the analysis of observation represents, as one might expect that it would, the determination of the sense in which science is empirical.

What led Hanson to the conclusion that observation was theory-laden? At least two arguments can be distinguished in his chapter on observation in Patterns of Discovery:

A. "Gestalt" shifts

The issue as Hanson sees it is whether interpretation is part of the perceptual process itself or whether it is something that occurs after perception is completed. He offers the "Gestalt" drawings such as the perspex cube and the bird-antelope as evidence for the former option ([4], pp. 9 ff.). The argument is simple. The shift from seeing the cube as from above to seeing it as from below or from seeing the bird to seeing the antelope occurs in an instant. It takes neither time nor conscious deliberation. Interpretation as an intellectual process requires both.

The ability to see the drawing as a cube or as either a bird or antelope will require training which is clearly of an intellectual nature, but once the training is mastered it becomes part of the observation process and no longer represents intellectual functioning in the same sense.

Clearly the change cannot be attributed to the drawing itself. If it cannot be attributed to a change in intellectual interpretation either, then there must be something else about observation that can change, and Hanson calls that the "organizational element" or the theory aspect.

B. Argument from the Complexity of Perception

Hanson also argues that perception is too complex to be accounted for solely by the contribution from the world, or, in the case of vision, by the retinal "picture." He uses the contrast between language and pictures to clarify this sense of complexity. Pictures copy aspects of originals, typically shapes, spatial relations and colors. Pictures represent the original in ways that language does not. The limitation of picturing brings this contrast into focus. Pictures can represent only those things that are picturable, e.g., physical elements such as shape, spatial relations and color. Language is not so limited. It refers to originals instead of representing them, it characterizes the original instead of arranging its parts according to that found in the original. But language can refer to and characterize any aspect of the original whether it is visual, auditory or tactile.

The most important aspect of the complexity of perception lies in what Hanson calls "seeing-that." We can see, for instance, that birds have hollow bones, that the universe is heliocentric, or that the car is parked in an inopportune spot ([4], p. 25). In each case what is seen involves relations that are not obvious without relevant knowledge. A picture of the situation or a description of each part would not necessarily convey the information that is observable to a suitable aware observer.

This argument can be used with simpler instances of observation as well. To observe a red pen involves knowledge of shape, color and use. The point is admitted by many philosophers who are in general disagreement with Hanson. Israel Scheffler, for one, concedes that observation apart from concept is impossible ([8], p. 36). He attributes the same concession to C. I. Lewis. Nagel, as we have seen, agrees that every observation is determined by theory ([5], p. 18).

It is clear that even if the theory-ladenness of observation is indubitably established its implications are far from certain. I will nonetheless add two arguments for the theory-ladenness of observation which will be developed in more detail in the next section.

C. Argument from the Complexity of the World

Hanson's second argument above suggests that the product of perception is too complex to be accounted for solely in terms of the contribution from the world. One might also argue that the world itself is too complex to be perceived without the help of theory. We are immersed, in the terms of J. J. Gibson, in a "flowing array of ambient energy" ([2], p. 5). Perception is the process of extracting information from that flowing array. If we are to be successful we must have some method of selection or attention since there is simply too much energy to attend to all at the same time. We would be overwhelmed by the sea of energy if we were without theory, that is, if perception were nothing but the process of conveying energy through sensory channels.

D. Argument from Non-Seeing

We often do not perceive things that are available in the sense of being directly before us. There are at least two types of situations where this occurs. In the first, our failure to see what is before us may be due to lack of knowledge. I do not, for instance, see what the radiologist sees when looking at an x-ray film. This will not sway those who are not convinced of the theory-ladenness of observation, since they will, in opposition to other arguments presented here, hold that what is seen is the same but the interpretation put on it is different as the result of different states of knowledge.

On the other hand, we often fail to see things that are directly before us when our state of knowledge with regard to them is adequate to "interpret" what we have seen appropriately. It might be answered that in such cases, although we have the necessary knowledge, we fail to apply it to this experience. But it is not unheard of to actively seek a particular item and still not see it when it is directly before us. This is explainable on the assumption that observation _is_ theory-laden, because on this account whatever is seen (and in cases like this there are always other things to be seen in the environment) requires the employment of some bit of knowledge. Not-seeing can be explained by the fact that perception as an activity involving theory is involved with some other object when that which is sought is available. The theory-neutral view of observation that is followed by interpretation is less open to an explanation of this sort. With the separation of processes the likelihood of such common perceptual mistakes or malfunctions is apparently eliminated. All the data is, on this view, presented with equal value, and we need only sort for that which fits the item sought.

If observation is theory-laden it is also a skill and is thus open to both error and excellence. If observation is theory-neutral it is not a skill but is the mere absorption of energy which is then processed by the intellect. Our common experience of both error and excellence in observation is evidence for the theory-ladenness of observation.

Finally, I will only mention the argument developed in Chapter III. There Thrane's attempt to develop a theory-neutral acount of seeing resulted in the conclusion that observation is irrelevant to epistemology. This is due largely to the totally undifferentiated character of perception without theory (the insight that was responsible for both Nagel and Scheffler accepting the theory-ladenness of observation). The assumption of theory-neutrality is therefore self-defeating.

THE ANALYSIS OF OBSERVATION
AND THE EMPIRICAL CHARACTER OF SCIENCE

The assumption that observation is theory-laden has been seen as casting doubt on the evidence-theory relation. I believe that the solution to this problem must lie in an analysis of observation, the purpose of which is to specify the contribution made by theory to the oservation process as well as the contribution from the world. I will begin that analysis with a discussion of the "data" of perception.

A. The "Data" of Perception

J. J. Gibson argues that as perceivers we are immersed in a sea of environmental energy, all of which is potential information about that environment. The energy of the environment is in that sense the "data" of perception. But this sense of "data" should be carefully distinguished from any "accomplishment" sense of data. That is, ambient energy is not information but only potential information. Without a perceiver it is not information, and in the presence of a perceiver it may or may not be information; it will depend on the nature of the perceiver (including the constitution of his sense-organs), as well as his interests or needs.

Therefore, ambient energy represents the data of perception but not in the sense that, say, a measurement would be a datum in a blueprint. The measurement is information apart from the blueprint but energy is not information apart from an actual perceptual process. This explains why I may not hear the traffic outside my office even though sound waves from it are striking my eardrums at an audible intensity virtually all the time. The sound waves are not the data of perception unless I perceive the traffic and I cannot do that unless I attend to it in the sense of employing some theory or other.

B. Analysis of Theory-Function

1. Selection

As I indicated in my third argument for the theory-ladenness of observation, the energy of our environment is a constant and complex source of potential information. It comes in the form of electromagnetic radiation, heat, sound waves, pressure and chemical action.

The massiveness of the source provides theory with its first function. In order for perception to occur the perceiver must limit his attention to particular sources of energy. He must select from the vast array of energy just those types and sources which are most likely to yield the basis for information at a particular time. When walking or driving, for instance, we select for visual data in the region directly ahead of us most of the time. When listening to a lecture we select for audible data of the sort characteristic of the speaker.

To argue for theory-ladenness is not the point here. That argument has already been pursued to the degree that it will be. Instead, selection should here be seen in terms of the specification or description of theory-function in the observation process. I begin with the assumption that all perception involves the absorption of energy from the environment. This provides needed breadth to perception, but it also requires a tool for limiting the data source to manageable proportions. Theory in a selective role accomplishes this end adequately.

It should be noted that since the energy of the environment is potential data and only becomes data when it is selected, the issue of the factual separateness between theory in its selective role and energy cannot arise. These are logical distinctions in the sense that theory and energy-data are separable in thought but not in fact, but in the case of its selective role this problem cannot arise since the energy does not become data until it is selected.

2. Connection

The data selected by theory must also be connected with other data in order to make sense of a complex environment. In our ordinary situations we regularly connect such things as a whistling sound with visible steam in order to gain the information that the water in the kettle is boiling. In science one may make connections between pendular and planetary motion in order to explain one or the other. Or the scientist may employ mathematics to manipulate data into a recognizable phenomenon.

The connective function of theory is the process of establishing relationships among selected energy-data. It has the additional function of directing the selection process, of course. Selection is never random, but is always for a purpose. The perceiver may be unskilled in finding relevant data, resulting in the appearance of randomness, but the perceiver who remained unskilled would not succeed and might not survive. He would surely not succeed in science.

Suppose, for instance, that a researcher into a rare form of early senility recognizes what appear to be symptoms similar to those he has read about accompanying a particular form of paracitosis. He reads the available reports on the paracite and, perhaps, contacts the people involved in that research. He knows that his patients are not suffering from the same paracite since it is not found in his environment, but through his library research he finds that this particular organism injures its host by selectively absorbing an important nutrient from its host's diet. The result is a form of malnutrition with symptoms like senility. He then tests his patients for the presence of the crucial nutrient, and finds that while their diets are adequate, their digestive tracts are incapable of processing the nutrient properly. He then administers the chemical in the form of an injection and observes his patients for changes in their symptoms.

In this example the researcher is guided by the recognition of a similarity between very different dieseases. His hypothesis was that early senility was the result of a nutritional deficiency of the sort found in the paracitic disease. He then knew what sort of experiment to perform. The hypothesis was nothing but a suggested connection between his problem and other data, but the possible data to which it might have been connected were virtually endless. It is the guide-capacity of theory that makes research non-random.

C. Theory as Non-generative

The point of this analysis of observation is to leave open the possibility of an empirical characterization of science. For science to be empirical it must be possible for observation and observation-reports to give information about the world that corresponds to the world in some meaningful sense. It is important to emphasize that possibility is all that is sought. I will not try to show that any particular report or set of reports has accomplished this end or that it ever will. The point is to base the empirical characterization of science on the analysis of observation and not on the content of any set of observation reports.

There are two critical points about this analysis of observation. First, every observation involves an energy contribution from the environment. Any process of gaining information about the world without such energy contribution would not be called observation. And second, the contribution by the perceiver in the form of theory is a non-generative contribution. That is, the energy is not amplified or altered by theory function. Great effort may go into the determination of the appropriate relations among data that constitutes theory, but that effort is the process of theory discovery and not the product. In any case, it does not represent the same sense of energy as that in the environment and could not, therefore, amplify or alter environmental energy. Theories, after all, are conceptual and concepts are too different from environmental energy to dilute, amplify or alter it.

1. Theory as empirical in function

The non-generative function of theory is important but there is more to the analysis of the empirical basis of science than merely pointing out that the perciever's contribution to observation is too different from the contribution from the world to replace it. We must give a positive account of the function of theory if we are to understand what it means to say that observation is theory-laden.

Hanson argues that theories represent "pattern" statements which provide the context within which detail statements make sense. Two points need to be made about this claim. First, the pattern statement is responsible for revealing the world since it constitutes information in exactly the same way that the detail statement reveals the world, by making appropriate connections. The only difference is the level of generality and the level of complexity of the theory.

In other words, the selective function of theory in science is the same as in observation itself. There is a good reason for this similarity and it is that theory in science always serves observation. That is, there is no theory in science that does not function as the theory component of some theory-laden observation. A theory that did not would have no place in empirical science.

2. Theory as empirical in production

Theories are discovered only by those who are immersed in the context of the problematic phenomenon. It grows out of careful observation; it is not an "armchair" activity in the sense of idle speculation. According to justificationist approaches to philosophy of science, theory is simply there with nothing said about how it came to be. The support that is given for this omission is that the creative act is the realm of psychology. Discovery doubtless has a psychological component, but the dependence of discovery on the observable problem situation, and its emergence from experience is not a matter of psychology. Neither is the relationship between the theory and the energy components of perception.

The assumption that theories simply "leap" into existence from the mind of the scientist ignores obvious facts about the relation between the scientist and his subject matter. But it also ignores significant information about the character of theories. Theories are as much the product of the world as they are the product of the mind of the scientist. It is only because the scientist is in such intimate contact with the world that he is able to solve a problem with regard to it.

This again raises the issue of the beginning and end points of discovery. The scientist begins with the observation of a problematic phenomenon and his research ends, or is successful, when he is able to explain that phenomenon in such a way that it is no longer problematic. This description of the end point of research is another way of saying that research ends when the scientist has achieved a new observation, when he has developed a new way of seeing the original phenomenon. This understanding of the movement of the discovery process is part of the support for the claim made above that the development of theory is always in the service of observation. This is because the stimulus for every research project is an observed problem and the solution always involves a new, non-problematic observation. The credit for this change can go only to theory. Whatever else a theory does, it <u>must</u> facilitate this observational advance.

Otherwise the problem which stimulated its development would persist.

IMPLICATIONS FOR PHILOSOPHY OF SCIENCE

A. The Theory/Observation Distinction

The most important implication of this analysis of observation is the meaning it has for the theory/observation distinction. In the discovery approach "theory" and "observation" are logical distinctions within the process of meaning-determination in science. The distinction between them is not based on empirical content or theory content. In order to make the remaining basis of the distinction entirely clear, it should be separated into two applications--its meaning within observation itself and as a way of distinguishing statements in science:

1. The theory/observation distinction and observation itself

As a distinction that is relevant to observation, "theory" and "observation" indicate aspects of the observation process itself. The redundancy of the term "observation" is misleading, and it comes from the tendency of past empiricists to associate observation exclusively with the contribution to the observation process of the world. In terms used here the T/O distinction is <u>not</u> applicable to the observation process at all since the appropriate logical distinctions for analyzing observation are "theory" and "energy-data."

The tendency to assume that observation is exhausted by the energy contribution from the world is also responsible for the sense in which the term "theory-laden observation" is misleading. In fact observation is not laden with theory or anything else. Part of what is meant by "observation" is theory. Theory is logically distinguishable or separable in thought from observation but not separable in fact. Without theory there is no observation.

2. The T/O distinction in science

Within science T/O distinguishes statements or sets of statements from each other. From the perspective of discovery statements are not here distinguished on the basis of empirical or theoretical content. Instead "theory" and "observation" indicate <u>explanans</u> and <u>explanandum.</u> They differ in terms of generality. Each is empirical, i.e., each describes or is intended to describe the world. Hanson suggests that the way they do that, the way in which they are empirical, is different but I believe that he overstates this difference. If any scientific explanatory structure is to have many levels, then there would have to be many senses of "empirical" in an heirarchic relationship. To distinguish many senses is more of a task than I believe is necessary. It is enough if the one sense sought here, the sense in which theory can be accommodated without precluding the possibility of undiluted contact with the world, can be made clear.

This sense of "empirical" allows any statement in empirical science to be labeled either theoretical or observational, depending on the context. A statement from the _explanandum_ of one scientific argument may be found in the _explanans_ of another.

a. Unobservable entities

This interpretation of the T/O distinction avoids the "two-tier" characterization of scientific statements adopted by empiricists like Scheffler where the top or theoretical tier is thought to refer to observable entities ([8], pp. 46 ff.). Given the theory-ladenness of observation there is no reason to describe any of the entities referred to in science as unobservable. To do that is to make a mockery of the empirical characterization of science. The reasons for it seem to be, (a) the fact that these entities cannot be observed with the unaided senses, and (b) that the production of the necessary instrumentation will require theory. Neither of these reasons represents a philosophical problem for the analysis of observation developed here. If the intervention of an instrument were sufficient, then anything observed with the aid of eyeglasses or the light microscope would have to be labeled "unobservable." The fact that optical theory is necessary for the production of eyeglasses or the light microscope does not change the situation either.

We need not know optical theory in order to use either instrument, but even if that knowledge were necessary it would not have a pejorative impact on observation since it is admittedly theory-laden.

There remains at least one additional reason for the "unobservable" label, that some entities that require instruments for their observation are in principle beyond our sense organs. Eyeglasses and light microscopes work with light of wavelengths in what is called the "visible" range, but the electron microscope uses a stream of electrons to which the eye is not sensitive. Similarly, x-rays, radio waves and sub-atomic particle motion represent forms of energy which none of our sense organs can detect at any level of intensity without the appropriate instrument. But the fact that a theory is necessary in order to connect the "energy-data" of observation to the entity in the world does not complicate our theory of observation. A theory is necessary in the case of the light microscope also. In neither case need we necessarily know the theory in order to use the instrument. There may be cases where knowing the theory is important in assessing the relevance of particular observations, but this could be true whether we have sense organs tuned to this type of energy or not.

This problem needs a great deal more work but I will make only one more remark about it: The fact that my analysis of observation does not depend on any necessary connection between the actual structure of our organs of sense and the entities found in the world is an advantage. It requires only that the theory component of observation function in such a way that it does not alter or replace the energy-date contribution from the world, and that its function be empirical in character. The intervention of instrumentation has no effect on that analysis.

b. Meaning-dependence of observation terms on theory

I will offer two arguments against the meaning-dependence of observation terms on theory. The first is a negative argument that the problem itself has contradictory presuppositions. The second is a positive argument based on the analysis of observation given above.

It is odd that the problem of the meaning-dependence of observation terms on theory should be encountered in an empiricist tradition since the most fundamental assumption of empiricism is that knowledge (and, therefore, meaning,) arises from experience or observation. The reason why it occurred, I believe, is the failure to examine observation. R. B. Braithwaite's introduction to <u>Scientific Explanation</u> gives an argument which expresses the prejudice against observation as a philosophical problem in the treatment of science. The problem of philosophy of science, he says, is scientific law and theory and how they relate to the facts of observation. It is not the problem of how we come to know those facts through perception. The reason why this second problem need not be examined is that the disputants in the philosophy of perception debate (the phenomenalists and the realists) can agree about what the facts of observation are even though they will disagree about whether there is a more fundamental sense of experience, to be analyzed in terms of sense-data ([1], pp. 4ff.).

It is important to note that this work by Braithwaite predated the debate about the theory-ladenness of observation. The assumption that all observers will be able to agree about the facts of observation cannot be made so casually if the claim that observation is theory-laden is accepted. At least some who accept that claim will argue that the facts of observation vary with the theory employed in observation.

It is also important to note that Braithwaite's attitude toward the philosophy of perception places the question of how observation terms and statements achieve meaning outside the parameters of the philosophy of science. If universal agreement is achievable on the meanings of these terms and statements, he is saying, we can pursue other questions without worrying about how observation terms and statements attained their meaning. This is not to say that Braithwaite is not an empiricist. It is to say that that part of his position which represents empiricism has the character of an assumption rather than a problem or argument.

Further, there is an obvious contradiction in this claim, although a different one from that which is involved in inferring the meaning-dependence of observation terms from the theory-ladenness of observation. Braithwaite's contradiction lies in the fact that the uniformity in observation reports depends on the assumption that observation is theory-neutral. This assumption is a philosophy of perception, one that is essential to his construal of philosophy of science. It is contradictory to define the parameters of philosophy of science (as excluding the philosophy of perception) on the basis of a philosophy of perception.

But the response to the theory-ladenness of observation is my real concern here. For the meaning of observation terms to be treated as dependent on the theory for their meaning, one must first make an assumption which leaves the question of how the meaning of observation terms is achieved unanswered. The meaning of observation terms must be a sort of philosophical void in order for theory-ladenness to imply that theory supplies the meaning of those terms. Otherwise the most we could infer from theory-ladenness would be that the meaning of observation terms would have to be re-assessed in light of theory-ladenness. In other words, if the problem of the meaning of observation terms has already been examined, then the theory-ladenness of observation would force a re-examination. It is only under the condition that it has not been examined at all, (i.e., that philosophy of perception is irrelevant because observation is theory-neutral) that from the theory-ladenness of observation we can infer the meaning-dependence of observation terms on theory.

Braithwaite's assumption that philosophy of perception is irrelevant to philosophy of science provided the needed assumption: we need not examine perception. Theory-ladenness then appears to imply that the meaning of observation terms comes from theory. Since the irrelevance of philosophy of perception to philosophy of science is based on the theory-neutrality of observation, we must first assume theory-neutrality in order to infer meaning-dependence of observation terms from the theory-ladenness of observation.

The positive acount of theory gives it the roles of selection and connection in observation and in science it has the correlative function of providing the context within which observation reports are related to each other. There is no factual separation between theory and observation statements of the sort that would support a dependency relationship. The assumption of a factual separation is supported by the prejudice against the philosophy of perception. In this atmosphere theory invention or discovery appears to be speculative in an "armchair" sense. When observation is given only an evidence or testing role it can have no effect on theory production. The result is the isolation of theory from observation and the assumption of dependency.

c. The Problem of Circularity in the Evidence-Theory Relation

I will argue here that there is no conflict between observation being theory-laden and the belief that observation is the source of evidence for or against theory. This argument will be based on a clarification of what it means to say that observation is theory-laden.

In the above argument I hold that the theory-ladenness of observation does not mean that observation terms are meaning dependent on theory. In that argument the point was to emphasize the ways in which theory and observation are related in order to distinguish their relationship from that which would be appropriate for dependency of meaning. In this argument I will show that the theory-ladenness of observation indicates or refers to the connective or patterning function of theory, and that as a consequence of this interpretation the theory being tested is never required for the observations that constitute the test. In other words, I have argued against meaning-dependency, and, and here I will argue against existence-dependency of observation terms and reports as a consequence of theory-ladenness.

Let me begin with an example. Suppose that a medical researcher hypothesizes that disease symptoms A, B and C are caused by the degeneration of a particular part of the brain.

If that part of the brain were in fact degenerated it would be revealed by test X. He then performs test X on patients with A, B and C. If test X is positive for a significant number of patients with this syndrome, the hypothesis is supported.

All the symptoms in such a case would have been observable and observed prior to the new theory of their cause. Test X might well have been available too. None of these observations depend on the theory, except in the sense that they would not have been associated or connected with each other without the theory. This is the function of theory that Hanson describes as "organizational," as the "pattern" for observational details.

This is not to say that the experiment, which in this case involved the performance of test X on patients with symptoms A, B and C is theory-neutral. The ability to recognize particular physical conditions as a symptom of disease requires theory. It might be argued, then, that even though the experiment used to test the new disease theory is not determined by that new disease theory, it remains dependent on other theories, particularly theories which describe physical conditions such as blood pressure as indices of health. This might appear to lead to the conclusion that theory retains the definitive role in testing for empirical adequacy. I will show that this conclusion is unwarranted.

The fact that other theories are involved in the observations which are essential to the experiment in question does not represent a problem for the empirical character of science if it can be shown that in these cases theory has the same guide-function that was described in connection with the new disease theory. That is, if theory does not determine those observations in the sense of making them possible, but only guides the researcher to make the appropriate observations, then the influence of theory at this level is also not anti-empirical with respect to the experiment designed to test the new disease theory.

The observation of elevated blood pressure was grouped with other symptoms into a single syndrome by the new disease theory. Theory or knowledge is required for the recognition that elevated blood pressure is an index of health. That theory is not required however, in order to observe and measure blood pressure. The theory of blood pressure as a disease symptom guides the researcher to measure blood pressure, but it does not make that measurement possible. The ability to observe and measure blood pressure (apart from any understanding of its relationship to human health) is theory-laden too, but by still different knowledge or theory. In order to observe and measure pressure one must know at least that it involves a mathematical relationship between force and area.

We can continue to trace this example downward toward simpler instances. While theory or knowledge is required in order to relate force and area in the mathematical way required in order to express pressure, this knowledge is not required in order to observe either area or force.

At this simpler level, the recognition of area as a measurable entity requires the application of mathematics to the energy contribution from the world, but it does not fabricate that energy contribution. Neither is mathematics necessary for that energy contribution to play a role in perception. Non-mathematical adults and children are not prevented from observing the surfaces of tables or other objects around them. Some theory or knowledge is required, of course, but it need not be mathematics.

As science evolves, higher and higher theoretical levels are reached, but the guide-function of theory is the same at each level. Theory guides research toward ever more complex integrations, but never does it supply the data that are to be integrated. The fact that the data being integrated by a theory have separately been integrated by lower-level theories does not change the empirical character of the experiment that is designed to test that theory.

This same point can be used to show how a single experiment can decide between the conflicting theories. Suppose that our current theory of light characterizes it as composed entirely of energy in wave form having no mass.

Suppose also that an alternative theory is offered which allows that light travels in a wave pattern but suggests that it has a small but detectable mass. A proponent of the latter theory might offer a "crucial experiment"--at a particular time during a near total eclipse of the sun the light from a distant star will pass close to the sun before reaching earth. Since a large part of the earth's surface is darkened it is possible to detect the light from the star over a wide area. If the light passing near the sun traveled in a straight line its detection point on the earth would be predictable relative to its detection point along other paths passing farther from the sun.

In order to make the observations necessary for this experiment one need not have any knowledge of the make-up of light. The scientist who suggested the experiment might ask an astronomer to perform it for him, saying nothing of the theory it was intended to support.

Proponents of the older view might be expected to support such research, expecting that it would corroborate their theory of light.

Neither theory of light is necessary in order to conduct this experiment. Therefore, if it shows that in fact light does bend when passing massive objects one might expect that all parties would agree that the experiment offered support for the new theory. This may not happen. Proponents of the old view, as Kuhn argues, will doubtless question the experimental design or suggest intervening variables that have not been taken into account. These arguments will prompt an examination of technique and possibly a search for specific, possible variables. Such responses are not unreasonable. Fraud and error are not unheard of in science and the search for unaccounted for variables sometimes results in significant discoveries. But if no fraud, error or variable is found, the experiment can legitimately be treated, if only in retrospect, as "crucial."

It is certainly possible that the theory necessary for making the observations might itself be replaced. Euclidean geometry, for instance, is not the only way to conceptualize spacial relations; alternatives have been offered. These alternatives, however, would have no relevance unless they demonstrated the falsity of the Euclidean principles that were used in making the critical observations. Even then the philosophical point being made here would be untouched. If the relevant Euclidean principles were proven false then both theories of light would have to reassess the value of an experiment that had appeared relevant. The geometric principles that replaced Euclid's might support a similar experiment and they might not. But there is no reason to suppose that another experiment could not have been designed that both parties could agree was decisive (given flawless technique and no intervening variables).

The point is that this interpretation of theory-ladenness gives theory a connective or guide-function. When an hypothesis is first suggested, it directs the researcher to the appropriate observation. This observation may have been made any number of times by others or it may have been possible with available instruments. But its relevance would not have been known prior to the hypothesis which suggested the previously unknown connection between this and other observations. An observation is T_1LO because we are guided to it by T_1, not because T_1 is necessary in order to see whatever is there.

Observations that act as evidence for a theory need not have been made nor need they be possible with existing instruments in order for this interpretation of theory-ladenness to be viable. Dudley Shapere, in "The Concept of Observation in Science and Philosophy," gives a detailed description of the development of a neutrino detector which was expected to confirm or disconfirm theories about this sub-atomic particle. It required the building of a large tank far below the surface of the earth in order to shield it from other particles that might have similar effects. Chemicals that would react to a particle of this sort were used in the chamber. Specifically an isotope of chlorine was used because it could be expected to decay on contact with such a high energy particle yielding radioactive argon. The latter could be removed by bubbling helium through the tank which could then be separated from the helium with a charcoal filter and conducted to a detection chamber ([9], p. 487).

The theory of the behavior of the chlorine isotope and its reaction to helium and charcoal filters as well as the theory relevant to radioactivity detection were all available prior to the construction of the elaborate neutrino detector. The theory of the neutrino was not involved in any of these individual components of the device. The theory of the neutrino was involved in the choice of those components. Only chemicals that could be expected to react in a predictable way would be useful and neutrino theory told the researchers which chemicals would most likely do so. Other considerations such as cost had to be taken into account since the character of the particle indicated that vast amounts of the primary detection material would be needed ([9], p. 501). Neutrino theory "guided" researchers to inexpensive material in the same way that it "guided" them to an isotope of chlorine. To suppose that we can only understand this experiment from the perspective of neutrino theory makes no more sense than supposing that neutrino theory is necessary for understanding the cost of the primary detection material.

The theory being tested has enormous impact on the choice of evidence, but the impact is not of the sort that could cast doubt on the espistemological warrant of the evidence so chosen.

At least one further problem should be considered that might have relevance to the issue of epistemological warrant. Shapere tells us that results other than those that were expected prompted the researchers to reassess the theories of the reactions within the tank. It was suggested that the low counts initially achieved might be caused by argon remaining an ion and being captured by another molecule in the mixture. In other words, the theory of the instrument was adjusted because it failed to yield results predicted by neutrino theory. But as indicated with reference to the light bending experiment, criticism of experimental technique is a reasonable part of any research. If results are other than those expected there may be something wrong with the design of the apparatus. However, no scientist would simply conclude that this was so because of the failure of prediction. He would test the implicated aspect of his experiment against the background from which it came. In this case ionization theory could be consulted to see whether such aberrant behavior might be expected under these conditions. Other experiments might be set up to determine whether alterations were called for.

No epistemological problem is created by this sort of interplay between prediction and experimental design. The critical point remains. Theory-ladenness does not imply that the experiment used to test the theory is determined by that theory. Instead, theory-ladenness tells us that the theory being tested guides us to relevant data; it tells us what sort of experiment to perform.

This is a general theory of the meaning of the theory-ladenness of observation. I have argued that theory-ladenness does _not_ imply that the experiment used to test a theory is determined by that theory, but the intent of the analysis is to make the general point that theory-ladenness does not imply that science is non-empirical.

In order to make the case for the general argument more clearly, I will entertain one further possible objection. It might be argued that the notion of scientific theory should be taken more broadly as including all the theories that are involved in the experiment. In the light experiment this would include Euclidean geometry and in the medical research experiment it would include the theory of blood pressure as a disease symptom. There are at least two senses in which this objection can be taken and I believe that both can be satisfactorily accommodated within my analysis of the theory-ladenness of observation.

First, it must be determined under what conditions the expansion of the notion of theory will be helpful in solving the problem of the meaning of the theory-ladenness of observation for philosophy of science. Insofar as the issue concerns the less general point about whether the theory being tested determines the observations involved in its own test, it is not helpful to expand the notion of theory. The theory being tested in the light example is a theory about the nature of light, not Euclidean geometry. It is only because Euclidean principles are not in doubt that the experiment could be designed in the way that it was.

This is not to say that Euclidean geometry could not be tested, but if an experiment were designed with that in mind, it would surely not be based on the assumption that the connections proposed by those principles were valid. To do so would be to beg the question in an obvious way.

No experiment should be expected to test all the knowledge that is presupposed by its design and relevant observations. The point of controlling variables in scientific experimentation is to limit to one the number of things being tested. If all relevant knowledge were included in the notion of theory, then every experiment would presuppose most of what it was supposed to test.

However, a second interpretation of this objection suggests a less obvious sense of circularity. It might be granted that "theory-ladenness does not imply that the experiment used to test a theory is determined by that theory," while insisting that the experiment is determined by _some_ theory (e.g., Euclidean geometry), and that this is enough to give theory a definitive role in testing. This is a weaker objection since it does not imply the vicious circularity that would characterize testing when the tested theory determined the condition of its own test, thereby guaranteeing its own success. But it suggests circularity nonetheless even though the circularity has been spread out over a range of scientific theories. The point of describing my argument as "general" is to indicate that this sense of circularity can be countered as well. This was the point of the analysis of the blood pressure datum used in the earlier medical experiment example. There I showed that the theory involved in recognizing the datum functions in the same guide-capacity described in connection with the theory being tested. Consequently its function in the experiment in question is not to determine a datum if by determine we mean to make it possible. Instead, the theory of blood pressure as a symptom of disease or Euclidean geometry will be responsible for directing the researcher to make a particular observation (which could have been made without it, but which might not have been made), and, naturally, for ignoring others, in fact everything else seen or felt.

To generalize the argument that theory-ladenness does not imply that the experiment used to test a theory is determined by that theory is to show that it applies at every level. The argument itself showed that theory-ladenness implies a guide-function in the sense of guiding the researcher to make appropriate observations, but that it does not mean that theory makes the observation possible. If that is true of all the observations that led up to the experiment in question, then there is no level of theory-ladenness which threatens the empirical character of science.

When we say that an observation is T_1-laden we are saying that we are directed to make that observation by T_1, not that T_1 is necessary in order to see whatever is there.

B. The Positive Account of Theory

The discovery approach provides the context within which theory can be given a positive characterization. That positive characterization has four different aspects. It is non-negative in two ways. First, theories need never be interpreted as referring to unobservable entities. The use of instrumentation does not make the entity unobservable and it need not raise any doubt regarding its existence status. Second, theory function in science does not have the negative impact of making the testing process circular.

The third positive aspect of this account of theories comes directly from Hanson. Theories, he said, provide a "pattern" within which observation details make sense. This account is impressive in that it is simple and understandable. Other accounts of theory function, such as that offered by Nagel, are less clear. Theories, Nagel says, "codify highly idealized (or 'limiting') notions . ..," and "serve as _links_ in the inferential chains that connect the instantial experimental data with the generalized as well as the instantial conclusions of inquiry" ([5], pp. 29-30). This description conflicts with his later admission that theories also, occasionally, report observations ([5], p. 36).

Scheffler is similarly vague and negative in his characterization of theories; they represent the upper tier of the two-tier structure of scientific language. Theories are described by contrasting them with observation statements. The latter, he says, formulate observable facts that are directly testable and that can be expressed independently of theory. Theories, by contrast, neither formulate observable facts nor directly testable generalizations ([8], p. 47). The meaning of theory-terms is determined by the theoretical context in which they are found. The function of theories on Scheffler's account is more vague than that offered by Nagel. It appears that they are important in his system because they provide an area in which scientists disagree without casting doubt on the empirical foundation of science.

The fourth positive aspect of theories developed here involves their actual discovery. Given a positive account of the function of theory as providing the pattern or organizational structure within which observational details fit, it is easier to see how theories evolve. Until the T/O distinction is divorced from the assumption that it represents a difference of empirical or theoretical content, theories merely "appear." But function and discovery cannot be separated without confusion. Once we see how theories serve as part of the empirical structure of science, the role their discovery plays in understanding the rationality of science becomes more apparent. Among the points clarified by discovery are certain aspects of falsification theory and the importance of the justification/discovery distinction for philosophy of science.

1. The justification/discovery distinction

The traditional use of this distinction has been to distinguish philosophical issues from psychological or sociological issues. Discovery, it was thought, was of no interest to philosophy of science. The distinction was mistaken in two ways. First, as I have argued, the examination of the discovery process has important epistemological consequences. And, second, justification can be better accommodated as part of the discovery process than in isolation.

Traditional empiricism has equated justification with the testing process. According to this school, theory statements and statements describing antecedent experimental conditions are combined as premises. From these premises singular statements are deduced which are then compared with statements describing the relevant observable situation to see whether they match. If they do the theory is justified and if not it is not.

There are many problems with this account, but I find two particularly troubling. First, since no account of theory discovery is given, it seems from a philosophical point of view pure chance when a theory succeeds in supporting the deduction of observation statements.

The second problem is that as an account of science, the justification approach is particularly paltry. All it gives us is the "bottom line" of science. Philosophers, of course, have found a great deal to do within this framework, but much of it has the flavor of patching a leaky boat.

The alternative that I offer is the discovery-justification continuum. As soon and as often as an hypothesis is developed which has promise it is tested. The testing process need not be particularly formal since its essence is to determine whether the old problem can be seen in a new way. In the example given earlier of hypothesizing brain tissue decay to explain a particular syndrome, the researcher might, as a first step, call a colleague and ask if he knew of patients with symtoms A, B, C <u>and</u> X. At this point the process of justification has begun. The answer, of course, may be equivocal and the initial hypothesis may require refinement or replacement. But the point is that justification or testing is important because it serves discovery and not because it <u>proves</u> that scientific assertions are true. In my view the use of the term "true" as well as the specification of its meaning should be given following the analysis of discovery. Otherwise it may represent a source of problematic presuppositions. The concept of "truth" should serve epistemology and not the other way around.

Similarly the concept of "empirical" should refer to science as a whole and not merely to the evidence used in science. Treating discovery-justification as a continuum is part of the process of expanding "empirical" to cover all of science.

2. The non-rejection of theories in the face of counter-evidence

Why are theories in science not rejected when the scientists employing them are fully aware of the existence of counter-instances or anomalies? Kuhn answers that to do science is to work under the aegis of a guiding theory or paradigm. To reject a theory without another to turn to for guidance would be to reject science itself. But this is not a satisfying answer; science might still be irrational for maintaining a position which is in conflict with the evidence of observation.

Others, notably Popper and Lakatos, have offered programs which aim to outline the conditions under which it would be rational to consider a theory falsified. This is not a straightforward project, according to Kakatos, for two basic sorts of reasons. First, almost any theory can be saved by _ad hoc_ additions to it which make exceptions for recognized anomalies. And second, the evidence of observation cannot _prove_ anything in the realm of statements since "proof" is a concept applicable to the logical relations among sentences ([10], pp. 97-98). As a result all the statements of science are fallible including those called "observational" or singular statements of fact. They are all adopted as a matter of agreement or convention ([10], p. 106). It is on the basis of these fallible statements that theories are rejected. The choice, according to Lakatos, is between this "risky conventionalist policy" or irrationalsim.

Lakatos explains the failure to reject or consider falsified a theory on the basis of anomalies or counter-evidence by adding what he calls a "sophisticated" proviso to the falsificationist criteria for rational behavior in science. This proviso stipulates that no theory be rejected unless a new and better theory is available to take its place. By "better" he simply means that the new theory must have "corroborated excess empirical content over its predecessor" ([10], p. 116). History suggests, he says, that scientific tests are not the two-cornered fights between theory and experiment of naive falsificationism, but instead are three-cornered fights between rival theories and experiment ([10], p. 115).

Taking historical factors into account may give an historicist ring to a philosophical account of science and it may not. In this case I believe that it does. Lakatos gives no reason for the added sophistication other than history. In that sense, his account is no better than Kuhn's.

Scientists, Kuhn has said, do not reject a theory until they have a better one because they could not continue to do science in the absence of a theory. Lakatos has altered falsificationism to take that historical fact into account. He would doubtless point out that falsification could not be "progressive" otherwise, but this is also Kuhn's point. To dogmatically reject the only available theory would surely halt progress.

The point remains that the rationality of science is not adequately clarified by falsificationism. Part of the reason for this is the conventional character of falsifying observational statements. What is not made clear either by Kuhn, Lakatos or Popper is the sense in which the observational report or statement is conventional. That the observation statements are fallible tells us little except that they are not as good evidence as we had thought. The reason why they are fallible, according to Lakatos, is because the truth-value of statements cannot be decided by the facts. His admiration for Popper is due to the latter's willingness to proceed on the basis of fallible, conventionally chosen evidence statements, fully aware of the risks, in an attempt to salvage some sense of rationality in science.

In my view such willingness is imprudent. The problem lies in the supposed conventional character of observation reports. Any structure built on admittedly conventional statements is of dubious value for explicating the rationality of empirical science. It is for this reason that the problem of discovery is important. Discovery begins wih observation and it examines the ways in which observation and theory interrelate. It shows us a sense in which the perceiver contributes to observation statements and a sense in which he does not. It does not preclude the view that observation provides information about the world that is objective.

But more than that, the process of testing is integrated into the process of discovery. Testing is perhaps a less formal procedure than either Lakatos or Popper recognize but this is part of the problem. For Popper, the analysis of discovery is impossible ([6], p. 31), and for Lakatos it is the same as the "rational appraisal of scientific theories" ([10], p. 115). The point is that such rational appraisal is a constant feature of scientific research. A theory or hypothesis is successful only if it facilitates observation, and as often as it promises success it is tested. The testing usually takes the form of an experiment, which may or may not be highly complex and time consuming. But by integrating testing into the philosophy of discovery it is possible to see more clearly how theory actually functions within science. Testing, in this sense, is a tool used in the discovery of theories. Viewed in this way we are less likely to treat theories as imaginative leaps or sheer speculation.

The problem lies in finding something to which to link the actual historically supported tendencies of scientists with regard to falsification. To say that falsification proceeds as it does because we could not have progress otherwise is lame and historicist.

What does discovery do for falsification theory, and specifically for the problem of the non-rejection of theories in the face of anomalies? First, the sense in which observation statements are conventional and the sense in which they are not becomes clearer. Also important, however, is the fact that falsification is given a rational context as part of the discovery process. That same context tells us that observation statements are conventional in the sense that the history of science, including its language, will dictate the direction of reearch. But those statements are not entirely conventional since the theory-ladenness of observation does not preclude the possibility of observation reports giving an empirical account of the world which, although guided by theory, is not fabricated by theory.

And second, by focusing exclusively on falsification one misses a surviving sense of justification. The claim that no theory is ever proven (since a falsifying instance can always turn up) misses the point of why theories are sought in the first place. They are valued because they facilitate observation and an hypothesis is called successful (and raised to the status of theory) when it is found to do that. This is a genuine sense of justification. It also tells us why theories are not rejected in the face of apparent counter-instances: They succeed in helping us to observe in important ways. That a theory could do more is stimulus for further research but it does not detract from what the theory is able to accompish. Neither does it bring into question the rationality of science. The positive account of theory offered above makes it unnecessary to apologize for non-rejection.

The numerous retreats that Lakatos defines are necessary because he has chosen to characterize the rationality of science using only a narrow band of the spectrum of scientific activity, justification or falsification. Without the broader context provided by discovery he is forced to busy himself with adjustments to a system that had been crippled by the framework within which the problem of scientific knowledge is placed. If falsification is the principle that defines scientific rationality, then non-rejection becomes a problem. But if falsification is put in the context of discovery, non-rejection is reasonable, i.e., it does not complicate a rational account of science.

A THEORY OF SCIENTIFIC TRUTH

My theory of truth has an undeniable realistic flavor to it. I believe that the terms of science refer to real entities in the world. No other assumption is compatible with an empiricist interpretation of science, and I believe that the evidence for an empirical interpretation of science is overwhelming.

Defining the exact sense in which I am a realist may be aided by reference to an article by Richard Rorty, "The World Well Lost" ([7]). Rorty concludes in this article that the coherence and correspondence theories of truth are "non-competing trivialities" ([7], p. 665). He identifies the source of the philosophical presuppositions which are responsible for such fruitless positions as Kant's distinction between spontaneity and receptivity and his distinction between necessary and contingent truth ([7], p. 649). I will concentrate here on the errors he finds implicit in realism in order to show how my theory avoids such a fate.

The dispute between the realist and anti-realist (correspondence and coherence) has been waged in terms of whether it is reasonable to assert the possibility of an alternative conceptual framework replacing entirely the one we currently have, according to Rorty. Without getting into the details of his argument, Rorty rejects the notion of different conceptual frameworks carving up the world differently. An equivocation is involved here on the meaning of "the world," which is particularly relevant to realism. The realist, Rorty says, wants the world to be independent of our knowledge in such a way that it might turn out that the world contains none of the things we attribute to it. The world, in other words, must not be conditioned in any way by the receptive faculty of our concepts, whether those concepts are innate or optional.

The equivocation appears when we realize that what the realist means when he refers to "the world" is what the vast majority of our beliefs that are not currently in question are thought to be about ([7], p. 662). For realism to be interesting, it must at once treat the world as having those entities we refer to and also treat it as unspecified and unspecifiable. It does not help, he says, to talk of the world in terms of "sense-data" or "stimuli" of a certain sort which effect our sense organs, for this is to involve oneself in a theory specifying how the world is ([7], p. 663).

My theory escapes this equivocation by virtue of the fact that it contains no distinction between receptivity and spontaneity. If the realist is to include a receptive faculty, he needs an independent test from the world in order to balance the order imposed by that faculty. He must take some position, however general, on the nature of the world in order to show that it can count as the source of independent test. In other words, he has not fully escaped from ontology. The equivocation that Rorty points to could be equally well described as the result of doing ontology and epistemology without distinguishing which is which. In the process of analyzing the relationship between concepts and knowledge, the realist is pursuing epistemology, but in positing a receptive faculty, he has retained an implicit ontology. Whether the character of that faculty is innate or optional makes no difference.

I believe that observation is spontaneous, that there is no reception apart from activity on the part of a knower. What I have done here is analyze that activity in order to see whether there are reasons to believe that science is empirical in the sense of referring to real entities in the world. The reasons that I have offered have nothing to do with the privileged claims about the way the world really is. The problem of how the world really is I leave to science, art and common sense.

My reference to "energy," for instance, has nothing to do with privileged information on my part. The concept of "energy" is itself theory-laden, but this is an advantage and not a defect. My point, after all, is to provide a <u>theory</u> of the empirical character of scientific knowledge. If the theory provides good reaons for believing that science is empirical, it succeeds. It cannot succeed if it relies on privileged claims.

Rorty's realist wants the world to be independent of our knowledge in order to have it serve as a source of independent test. I too believe that the world is independent of the knower. I also believe that it is a source of test for science. I further believe that our knowledge and observations are of real entities in the world. But in order to maintain these beliefs I need not take any position on what the world is like independent of our knowledge of it. The question is nonsense for it is knowledge that tells us what the world is like.

I have good empirical evidence for the first belief, that the world is independent of the knower, for as Scheffler says, it frequently surprises me and resists my attempts to deal with it. By analyzing observation I have attempted to give a philosophical interpretation to that sense of the independence of the world. That interpretation has implications for our understanding of science and I have investigated some of those implications. But nowhere do I hold that the knowledge that we gain of the world through oservation "represents" the world in the sense of being a sort of carbon copy or impression on a wax block, for that is the receptivity assumption criticized by Rorty. Consequently, when knowledge changes my theory does not fracture, for on my view knowledge does not correspond to the world by virtue of copying or picturing. This is not what it means to have empirical knowledge of the world.

The belief that the world is the source of test in science is supported by both empirical evidence and philosophical arguments. Physical science supplies the empirical evidence that the contribution to the perceptual process by the world is energy. Physical science also supports the argument that thought or theory could not possibly alter or generate energy of the sort supplied by the world. The philosophical argument provided in connection with the analysis of observation and the medical and light research examples gives a reasonable analysis of theory-function in theory-laden observation without supposing that theory alters or fabricates the energy contribution from the world. The combination of these arguments gives us a concept of observational evidence in science which is based on theory-laden observation, and yet which has no non-empirical aspect.

It is my position that this is an adequate argument to support a realist interpretation of science. I also believe that this argument supports the inclusion of a correspondence component in a theory of scientific truth. The claim that scientific knowledge corresponds to the world is based in part on the fact that we have good empirical reasons for believisng that there are entities apart from human observers. It might be objected, however, that the issue in supporting correspondence is not <u>that</u> there are entities apart from us but rather <u>what</u> those entities are like. In a sense I agree with this objection; correspondence cannot be established between determinate knowledge and an indeterminate world ("unspecified and unspecifiable" in Rorty's discussion). But the issue is complex and distinctions are needed in order to clarify it. First, it is clear that the correspondence relation is between our knowledge and the world. The problem is how we could possibly know whether correspondence is possible.

There are two possible solutions to the problem of how to know whether correspondence is possible. The first is a philosophical analysis of the processes of coming to know the world which may or may not support the belief in correspondence between knowledge and the world. The second is a comparison between scientific knowledge and some other source of knowledge of the world such as ontology or metaphysics. The latter is unlikely to succeed since it shifts the problem from correspondence between knowledge and the world to correspondence between two types of knowledge. The problem of how we know whether correspondence between the more basic type of knowledge and the world is possible would remain.

If appeal to special (non-scientific) knowledge of the world does not succeed, the philosophical analysis of the processes of coming to know the world seems to be the most reasonable route. But how are we to respond to the charge that the issue is not <u>that</u> there are entities but <u>what</u> those entities are like? We cannot know what the entities are like apart from our knowledge (in this case, scientific knowledge) of them. Even if we held open the possibility of appeal to special or privileged knowledge of a metaphysical or ontological sort, it would not answer the question of correspondence. At least this much is clear: if we are to show that correspondence is possible we must do more than argue that there are entities apart from human observers. We must show that the impact of those entities is neither altered nor fabricated by the observer.

Does this leave open the problem of how correspondence can hold between determinate knowledge and indeterminate entities in the world? The fact that those entities are unknown apart from our knowledge of them should not be confused with the assumption that they are indeterminate apart from our knowledge of them. There is no reason to suppose that the entities of the world are indeterminate or unspecifiable apart from our knowledge of them.

Further, a qualified sense of correspondence is supportable based on the theory-ladenness of observation if it can be shown that theory-ladenness means only that observation is guided by theory and not determined by theory. If this is so, then observational evidence is objective in the same qualified sense.

Coherence has a role as well since the scientist is most likely to seek answers in directions or areas mapped out by his predecessors. But coherence should not be interpreted in the strong sense that the truth of a proposition is decided by whether is is "logically deducible from some of the other propositons . . . of the system" ([11], p. 111). Instead, it should be taken to mean something weaker such as "not compatible with some of the other propositions of the system." Discoveries in science are often incompatible with some of the propositions previously accepted. And further, the new theory will probably not be strictly deducible from anything contained in past assertions.

But coherence has additional value in that it tells us why particular bits of data were picked out (and why others were ignored), and why they were connected in the ways that they were. Historical background has a powerful impact on virtually all research since there is, perhaps, an infinite number of possible connections that could be made among the data of our environment. The connection that is chosen will have to demonstrate that it corresponds to the world, but it is unlikely that it is the only connection that could do that. Correspondence cannot tell us why this particular connection was chosen, but coherence may. This is part of the sense in which a theory of truth should apply to the process of science and not merely to the product.

Pragmatism contributes to this theory of scientific truth both in terms of process and product. A theory fulfills its function when it makes a new connection among the available data, and we know when that has happened when new observaions occur as a result. This functional quality represents a pragmatic aspect of science, but not in the "large and loose" sense that an assertion is called true if it satisfies the purpose of the inquiry that brought it about. This sense of pragmatism confuses reasons for accepting something with the reasons for accepting it as true ([11], pp. 124-127). The sense in which I wish to employ pragmatism is as a tool of correspondence. To say that an assertion is true when it corresponds to a fact is important and there is no reason not to maintain that sense of truth in connection with science. But it remains an open question <u>how we know</u> when an assertion corresponds to a fact and it is this aspect of truth that pragmatism fleshes out. We know that a theory corresponds to a fact when it makes possible a new observation. This is a more specifically empirical interpretation of pragmatism than the more vague criterion of "satisfying the purposes of the inquiry that brought it about." The point is the same, however, since the purpose of scientific inquiry is to establsih a context within which a problematic phenomenon no longer appears problematic.

TRADITIONAL EMPIRICIST PHILOSOPHY OF SCIENCE

The problem that is common to Kuhn, Nagel and Thrane is the failure to examine observation.

Kuhn and Nagel both retain the philosophical underpinning of an earlier age when it was assumed that observation was theory-neutral. If theory-neutrality were the case then it would be reasonable to expect that the content of observation reports would be uniform for all normal observers. With the introduction of the theory-ladenness of observation into the debate, Kuhn and Nagel retreated from the solid foundation of uniform observation reports to "epochal stability" and "relative stability" in those reports. They thought that these interpretations of observational evidence were enough to support rational characterizations of science. The error of these approaches is due to the fact that the philosophical underpinning of uniformity in observation reports--the theory-neutrality of observation--cannot be watered down without losing it. That is, the assumption of uniformity among observation reports is based on a philosophy of perception, that perception can be accomplished without theory. Once that philosophy of perception is given up, as it must be with the acceptance of the theory-ladenness of observation, there is no longer any philosophical support for the belief in the uniformity of observation reports. That belief provided the implicit foundation for the empirical characterization of science. What is needed to re-establish an empirical characterization of science is an examination of observation, or a new philosophy of perception.

The structure of the rationality of science can then be built on that analysis, but its shape cannot be predicted prior to the analysis of observation. Kuhn and Nagel tried to retain the stucture of rationality of science that was based on the justification of scientific knowledge, but they did so without the foundation on which the justificationist approach was built.

Braithwaite takes a straightforward approach to this problem, arguing that philosophy of perception is irrelevant to philosophy of science, so long as all observers report the same things. As we have seen, that argument has contradictory assumptions.

Popper argues for much the same point with similar conflicts. The problem of epistemology, he says, lies in the logical relations between statements, "which alone interest the epistemologist" ([6], pp. 43, 99). He admits that all knowledge of the world comes from observation, but he insists that this knowledge can justify other statements.

The conflict in Popper's position is obvious in the sense that testability is his primary criterion for the acceptability of scientific statements, as it must be for any falsificationist. In fact, he retains Braithwaite's contradictory assumptions. He believes, like Braithwaite, that observation reports should be the same for all normal observers. Any scientific statement, he says, "can be presented (by describing experimental arrangements, etc.) in such a way that anyone who has learned that relevant technique can test it" ([6], p. 99). This instrumentalist approach avoids none of the problems introduced by the theory-ladenness of observation. The data achieved with the aid of the instrument must be fitted into a context. That context is theory and if the theory is in dispute the relevance of the data may be in dispute as well.

Popper's position in this regard can be easily misconstrued, I believe. He takes great pains to argue that which statements we employ as "basic" in science is a matter of choice, giving the appearance of a conventionalist stance. If he had a genuinely conventional interpretation of observation, he could not be accused of Braithwaite's contradiction. But the matter of choice for Popper is purely the problem of where to stop in the deductive chain of reasoning. He happily admits that any basic statement at which we choose to stop has the character of a "dogma," but the admission of dogmatism is innocuous because we can at any time test it further by deducing further consequences from it ([6], p. 105).

Popper must include observation in some form and he does so with the criterion of "observability" (any basic statement in science must be about an observable event). We need not <u>define</u> observability, however. Instead, we should treat it as a primitive concept, he says ([6], pp. 102-103).

Like Braithwaite, Popper restricts philosophy of science to the logical relations among sentences. And like Braithwaite he rules out any examination of observation. This appears to be a reasonable ploy because "basic statements" are unproblematic as to their content. I.e., philosophy of perception is treated as irrelevant because of the assumption of uniform content in observation reports, which is based on the assumption of the theory-neutrality of observation, a philosophy of perception.

The importance of examining observation is demonstrated in another way in Israel Scheffler's Science and Subjectivity. Scheffler wants to include the notion of theory-ladenness in his interpretation of observation, while maintaining that science is objective. That observation is the source of that objectivity can be seen from the fact of disharmony between what we expect and what we observe ([8], p. 44). This leads him to attempt an interpretation of observation that, although theory-laden, nonetheless provides the basis of agreement among scientists who may not agree about theory. One of the ways in which he does this is to offer an extensional interpretation of the meaning of observation terms in order to establsih the possibility of uniformity of content for observation reports, even among theoretical disputants. How this interpretation of the meaning of observation terms is compatible with theory-ladenness is never entirely clear, especially since theories cannot be given an extensional definition, referring as he says they do to unobservables.

There are two things to be emphasized about Sheffler's argument. First, it ends almost where it began, with the dependence on the notion of disharmony between what we observe and what we expect ([8], pp. 118 ff.). He provides no philosophical interpretation of this disharmony and that brings up the second point of emphasis. What he has given does not constitute an analysis of observation because he begins with the assumption that observation must provide "independent control" over belief ([8], p. 45). To begin with such an assumption is to give the answer to the question of philosophy of perception in advance. The proper question about observation is, how is it possible for observation to achieve knowledge of the world? To begin with the assumption that it must be independent of belief is to beg the question.

This is exactly what is wrong with Thrane's approach. His analysis of seeing begs the question. He substitutes a defense of the possibility of theory-neutrality for an analysis of observation. He believes that the objectivity of science depends on theory-neutrality. Such an approach appears with hindsight to be fainthearted. The outcome is interesting, however, since it leads to the conclusion that observation as theory-neutral is irrelevant to epistemology.

The analysis of observation is the key to any empirical characterization of science. It is because the discovery approach leads us through an analysis of observation as a first step that it is superior to the justificationist approach. The question of how it is possible to obtain knowledge of the world through observation must be answered before the question of rational structure of science is raised. The former has been avoided by Kuhn, Nagel and Thrane as well as by Scheffler and Popper, but only by begging the question or making contradictory assumptions.

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CHAPTER VII

CONCLUSION

My point has been to argue that an adequate understanding of science must examine the problem of the discovery of scientific knowledge. The failure to do so, I have shown, results in limited knowledge. In general, my thesis can be taken as an argument against justification-discovery distinction which is treated by Reichenbach ([3], p. 382) and others as the outline for the program of philosophy of science. That is, philosophy of science concerns itself with reasons for accepting an hypothesis after it is offered or justification, and not with the reasons for offering that hypothesis or its discovery.

The reason why the failure to examine discovery has caused problems is because of inadequate and self-defeating concepts of observation. The inadequacy of the understanding of observation was first revealed by the recognition that observation was theory-laden. I have examined works from recent writers in philosophy of science in order to see how they responded to the challenge brought by the theory ladenness of observation.

The first of these writers was Kuhn. Kuhn brought the problem of the theory-ladenness of observation to the surface of philosophy of science. His handling of it had a great deal of impact on philosophers who followed him.

Kuhn's concern with observation grew out of his study of the history of science. The evidence was overwhelming, he believed, that the model of scientific growth by steady accumulation was inaccurate. It seemed to him that a regular feature of science was the periodic rejection of much that had been considered "scientific," including observation reports. The conclusion appeared unavoidable that the foundation of objective observation reports so long presupposed by empiricist philosophers was faulty. The foundation of observation reports appeared to him to have more to do with consensus among the community of scientists than with objectivity in the sense of giving a true account of the world.

Kuhn did not explicitly conclude that science itself was irrational. Instead, he continued to describe the rationality of science in terms of the relation of justification between theories and observation statements. The problem of philosophy of science, according to Kuhn, was to determine the sense in which that relation still held, given the loss of objectivity brought by the theory-ladenness of observation.

Science, he said, could continue justifying its theories on the basis of observation reports so long as the theories with which the observations were laden were not in dispute. When those theories were in dispute, however, observation reports could no longer function as evidence. During such periods science was left with "persuasion" and "conversion" as means of making decisions. Revolutions, or periods of major change, were treated as irrational by Kuhn.

Kuhn's work can be seen as an attempt to define the limits of philosophy of science, given the theory-ladenness of observation and the loss of objectivity it entails. Philosophy of science, he concluded, retained the capacity to illuminate the rationality of science when theory remained stable, but it became mute when theory changed.

Kuhn accepted the theory-ladenness of observation and concluded accurately that there was something wrong with the traditional empiricist notion of the objectivity of observation. Consensus on the content of observation reports did not seem to be supportable, given the history of science. But instead of trying to find another interpretation of the objectivity of observation, he concerned himself with describing the implications of the loss of the traditional sense of objectivity. He rejected the problem of discovery and thereby blocked at least one avenue that would have been more fruitful.

Nagel attempted to solve Kuhn's problem. He accepted the claim that observation was theory-laden, but he argued that theory-ladenness did not relativize knowledge or lead to circularity in testing in the ways that Kuhn thought. He held that even if there were no inherent differences between observation and theory terms and statements, observation terms and statements were nonetheless more stable. This relative stability provided all the foundation that was required for the testing or justification relation to remain a viable way of characterizing the rationality of science, he said, with the single proviso that the observational evidence chosen to justify theory not be laden with that particular theory.

In a sense I believe that Nagel is right in his assertion that observation terms and statements are relatively stable in comparison to theory terms and statements, although in particular cases this may not be true. Unlike Nagel, however, I can place that assertion in the context of the theory-observation distinction. That is, theory and observation are distinct because of levels of generality and generality is sometimes related to stability although not always.

Nagel's difficulties came from the same source as Kuhn's. He assumes that the objectivity of observation must be based on consensus on the content of observation reports. When forced to admit the theory-ladenness of observation he resorts to "common-sense" as a source of theory or prior knowledge that satisfies the consensus requirement and provides "relative" stability. This gives him a sense of objectivity but only insofar as common sense is objective. Should common sense change he would have no more insight into that period of change than Kuhn did into revolutions in science. Nagel's solution to Kuhn's problem amounts to shifting the basis of consensus from the scientific community to the community at large.

This is not an attractive solution for two reasons. First, it does not tell is how specifically scientific observation reports (that have no common sense corollaries) achieve any reasonable sense of objectivity. With theory-ladenness admitted, there would appear to be no basis for consensus on the content of observation reports. The second reason for rejecting this solution is that Nagel himself implicitly rejects it. He grants that circularity in the testing relation is still possible, although avoidable. Within science relative stability does not solve the problem of the objectivity of observation. As long as the basis of objectivity is the content of observation reports, the admission of theory-ladenness will raise the issue of meaning-dependence and circularity. And as long as the evidence of observation is even occasionally circular, it can not be genuine evidence because the objectivity of observation is in doubt.

Nagel's treatment of science is interesting for one more reason. He raises the issue of the function of theory in science but he can find no way to account for it within the parameters of the problem of justification. His "use" criterion fails because, as he sees, theories sometimes report observations. The problem of objectivity as well as the problem of theories results from the isolation of objectivity in observation. If he had examined the problem of discovery the interrelations between theory and observation would have become the working context of his philosophy of science. Instead, the separation of theory and observation become a presupposition. Consequently, the function of theory in reporting observations was as much a problem as the objectivity of observation.

In short, Nagel tried to solve Kuhn's problems but he retained the source of those problems in his assumptions about observation. And like Kuhn, he avoided the one route that offered relief from the difficulties raised by theory-ladenness.

This attempt to solve the problems raised by theory-ladenness failed, but others have concluded implicitly that those problems are unsolvable. The article by Thrane supported this conclusion. Like Kuhn, Thrane say that theory-ladenness was incompatible with a consensus-on-content interpretation of the basis of objectivity in observation. But unlike Kuhn, he chose to develop a concept of vision that was theory-free. Without theory, he thought, consensus on the content of observation would be achievable. His article is interesting because it establishes conditions that must be met for theory-free observation. The most important condition is that the object of observation must be something of which we are not aware. This is so because to be aware of something is to be aware of it as something determinate. Since determinateness is the province of theory or knowledge, indeterminateness is critical to theory-free vision. Thrane's argument for the possibility of perceiving something without being aware of it fails, but he detects failure in an even more important sense. Even if it were reasonable to talk of an object of vision of which we are not aware, it would be useless for philosophy of science, he says. Why? Because an object of vision that is so radically indeterminate cannot be specified as content in an observation report. In other words, his theory aims at a ground for consensus on observation reports but the conditions required for consensus are incompatible with content.

Thrane's article helps to point out that the problems of philosophy of science discussed here were not created by the theory-ladenness of observation, but were implicit in the separation of theory from observation. The principle that consensus on the content of observation reports was the basis of objectivity required incompatible presuppositions. It required indeterminateness for consensus and determinateness for content.

I have described the independence of the evidence of observation as a pseudo-problem. It arises, as we have seen for two reasons. First, the concept of the objectivity of observation reports and the theory-ladenness of observation seemed to threaten that concept. And second, the concept of theory in its relation to observation was left unexamined. Theory was examined in philosophy of science to be sure, but the sense in which it might be said to contribute to observation was left unspecified. I have offered discovery as the approach to correct this situation because discovery in science _is_ the discovery of theories. If we examine that process we find both what theories contribute to observation and how they make that contribution. What they contribute is the selection and connection of the data-energy from the environment and not the data itself. How the contribution is made is through constant contact with the environment. This tells us both that theory is not a dilution-factor and that it does not spring _ex nihilo_ from the mind of the scientist. It is empirical both in process and product and fully compatible with an objective account of observation.

The independence of observational evidence is a pseudo-problem based on faulty presuppositions about the objectivity of observation and the failure to analyze the discovery of theory. Nagel's work showed us how the latter results in the treatment of theory as the "free creation" of the mind of the scientist and the concomitant inability to integrate the function of theory into philosophy of science.

The meaning-dependence of observation terms on theory was shown to be a pseudo-problem on similar grounds. In a common sense sort of way it is surprising that such a problem should plague self-avowed empiricists. If a relation of meaning dependence should arise for empiricists it should have been the other way around, with the assumption that theory terms were meaning-dependent on observation terms. But it was to illuminate the reasons behind this construal of the problem that the demarcation criterion was introduced. The empiricists made presuppositions that were shielded from examination. They assumed that science was different from metaphysics because of its dependence on observational evidence. Observation became their criterion of the real and was doubly protected from inclusion in the program of philosophy of science. To examine it would appear to be an exercise in metaphysics or an incursion into science. Consequently, the only recognized source of meaning in science was theory and the arrow of meaning-dependence was clearly established.

But, again, to correct this problem we need only examine the discovery of theories to find that meaning determination is the process of science itself. "Theory" and "observation" are logical distinctions which illuminate that process but they do not refer to separate or separable sets of terms. It is only when theory and observation are artificially separated that dependence appears as a problem.

Falsification theory offered little that was new, as the problem of the non-rejection of theories showed. Non-rejection was a problem only because of related assumptions about the basis of objectivity and the program of philosophy of science. Observation provided the basis of objectivity because of its content and this in turn was the foundation of the rationality of science. The admission of the theory-ladenness of observation forced the falsificationists toward a conventionalist position, but this appeared (at least to Lakatos) the only alternative to irrationalism.

That is, falsificationism did not represent a significant philosophical advance because it retained the unsupportable content interpretation of the basis of objectivity in observation, and because it continued to restrict the program of philosophy of science to the testing relation and its implications. The sophisticated provisos added by Lakatos, e.g., that a theory not be rejected until another is found with "corroborated excess empirical content over its predecessor," _is_ significant for understanding science, but it has the flavor of an _ad hoc_ addendum. The program of philosophy of science as justification provided no context within which this insight fits. Justification in general has become too fragmented, resembling a patchwork more than a program, as a result of the damage to the concept of objectivity brought by theory-ladenness.

Discovery, on the other hand, provides a context within which the peaceful co-existence between theories and anomalies constitutes no problem for the rationality of science. The objectivity of observation, discovery revealed, is not based on content.

This is why Hanson's philosophy of science represents an alternative to the traditional empiricist program. Instead of making assumptions about observation, he proposed to examine observation as the starting point of philosophy of science. The issue is discovery, he said, and the way to enlighten discovery is by determining how theories are built into our appreciation of observation, facts and data. He began with the assumption that theories and observation are intimately related, and with the further assumption that the way to understand the rationality of science was through the investigation of that relation.

The fruit of Hanson's approach can be seen in his contribution to the understanding of theories as the context or background against which observational details make sense alongside other data. He also characterized theory as an empirical part of science. But, equally important in his analysis of observation which attempted to accommodate its complexity and depth. He was able to bring these insights to bear on particular issues in philosophy of science. He was able to show, for instance, that the so-called "functionally _a priori_" character of some of the laws of classical physics is a psychological issue with little relevance to the epistemology of science, giving new meaning to the charge of "psychologism." He also showed that many problems thought to be peculiar to quantum mechanics fit the relations between theory and observation that he developed in _Patterns of Discovery_.

Hanson's philosophy of science mapped the discovery direction that I have followed here. He began by examining the interrelations between theory and observation with the result that he was able to specify the function of theory in scientific observation as well as its empirical character. The theory-ladenness of observation appeared to be threatening from the justificationist's perspective in part because that perspective offered no analysis of the relations between theory and observation. Theory input into observation was admitted in some cases (such as in the works of Nagel and Kuhn treated here) without any specification of its actual meaning or sense. Whatever its meaning it appeared to conflict with objectivity based on the content of observation reports. By following Hanson's lead I have been able to show that it is possible to develop a concept of observation that is both objective and compatible with theory input. The function of theory, I have argued, can be characterized as selective and connective which makes it both empirical and a non-diluting factor in perception.

I have further attempted to develop a theory of truth that is appropriate for this approach to scientific knowledge. That theory has elements of correspondence as well as coherence and pragmatism. One of the most attractive parts of the theory is that it avoids too great a dependence on coherence or conventionalism. Lakatos described Popper as courageous for his willingness to proceed on an essentially conventional foundation, having found no reasonable sense of correspondence. The lack of a correspondence element was due to the faulty and unsupportable assumption that objectivity was to be founded on observation reports. To proceed as he did seemed the only path open, given the dictates of the justification program of philosophy of science, but a more prudent course would have been to seek another ground for or interpretation of objectivity.

The coherence or conventional aspect of the theory of truth developed here is not a retreat position but instead has a functional role in the philosophy of discovery. All discoveries are contextual and understanding how a discovery was made requires an appreciation of the history of the problem that stimulated research in the first place. Harold Brown recommends the analysis of discovery in science based on its historical context but he has explicitly given up any sense of correspondence between scientific knowledge and the world. Instead he calls any proposition "true" that is part of a body of scientific knowledge ([1], pp. 152-155). He overtly embraces historicism and relativism as that which is possible for philosophy of science.

This is not a necessary course. In a recent article Theodore Kisiel attempts to formulate a logic of discovery which illuminates the rationality of science without abandoning the belief that scientific knowledge somehow makes objective contact with the world. The logic of discovery, he points out, begins with the problem to be solved, and problems are not man-made. They force themselves upon us and this suggests a sense of objectivity that he calls "peculiar" and more complex than the objectivity of atomically isolated data ([2], p. 405). This is compatible with the sense of objectivity to which I have attempted to give structure here. I.e., observation is treated as objective because the input from the world is both genuine and undiluted.

Kisiel further argues that the logic of discovery is more basic than traditional forms of reasoning such as deduction since these depend on discovery for their premises. This more basic form of rationality would be measured, he says, by the ability of the researcher to adapt to new and challenging problems ([2], pp. 403-404). This stands in marked contrast to measuring a student's ability to learn and apply rigid rules of inference.

Kisiel, like Brown, takes what might be called a contextual approach and for that reason could be on the same path to conventionalism. He avoids that course, however, by placing his analysis of discovery in a wider, human context with discovery characterized as a "form of life" that presupposes objectivity and precedes verification ([2], p. 409). His direction for philosophical investigation is aimed, in my terms, at fleshing out the coherence of discovery. I believe that this is potentially a very fruitful direction to take.

An implied problem that deserves consideration is the appropriateness of available metaphors for knowledge. If correspondence is the primary criterion of truth and consensus on observation reports the basis of objectivity, then the most likely metaphor for the relation between knowledge and the world will be picturing or mirroring. Hanson has pointed out some of the problems with this metaphor, but in a broader sense it fails by being too rigid and specific. It is possible for observation to yield genuine contact with the world without generating a foundation for consensus. In fact, it can be characterized as objective without any specification of content and this leaves open the possibility of a functional metaphor such as "tool-using."

The shift from a content metaphor to a functional metaphor may have other interesting implications as well.

Knowledge can be viewed as far more fluid and subject to change (of any depth) on the tool interpretation without the sense of threat that accompanied the content interpretation. We are not forced to make uncomfortable concessions, admitting for instance, that "knowledge" that once served us effectively and was called "true" is now considered false. Archaic knowledge was true because it manifested the three elements of correspondence, coherence and pragmatics. The fact that this was superseded represents no conflict for this theory of knowledge simply because the basis of our concept of objectivity is not content.

The emphasis on fluidity may also have implications for fields such as learning theory and psychology. Neurosis might, for instance, be characterized and treated as, in part, an epistemological illness--the inability to relinquish certain non-functional approaches to the world.

A similar psychological problem may be responsible to some small degree for obscuring the importance of theory-laden observation to epistemology. While it is clear at a common-sense level that our knowledge and attitudes influence what we are able to observe, it is also common-sense that many of those controlling factors are not empirically based in any obvious way. Attitudes and beliefs that are inherited from our culture, sub-culture and family may remain unchallenged for a lifetime and yet constitute a dysfunctional element in our lives. This would appear to call for a distinction between theory-laden observation (which I have described as empirical in all its aspects) and ego-laden observation.

The tool-functional metaphor may also facilitate a more practical approach to the assessment of knowledge claims. The process of determining the value of knowledge claims need no longer be restricted to an up or down truth determination. The discovery approach places knowledge itself, as Kisiel suggests, in the broader context of human life. The broader context allows for knowledge assessment based on notions such as "appropriateness." The picturing metaphor, on the other hand, restricts knowledge assessment to the corresponding relation between knowledge and its purported objects.

The discovery approach, however, places no restrictions on the level of scrutiny in knowledge assessment. The only philosophical difference between theory and observation, after all, is the level of generality. Any level, including the most basic observation is fair game.

It is my belief that the discovery approach to scientific knowledge can give new life to philosophy of science without sacrificing a basic commitment to empiricism. It provides a context that is broader than justification, and a viable interpretation of objectivity.

THE END

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The dissertation is therefore accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

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