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A Logic Of Discovery: Lessons From History And Current Prospects

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A LOGIC OF DISCOVERY:
LESSONS FROM HISTORY
AND CURRENT PROSPECTS

by

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ABSTRACT

This dissertation critically examines the philosophical literature on the problem of the logic of scientific discovery. The purpose of this examination is to bring the problem into sharper focus so that reasonable strategies might be developed for its solution.

The analysis of both the contemporary and historical literature on the topic reveals many misconceptions and faulty arguments scattered amongst the valuable contributions to the problem. With this in mind, a brief, but concise reformulation of the problem is presented to facilitate a more orderly assessment of putative contributions to the issue. Once the historical and contemporary literature has been assessed, the dissertation culminates in a proposal to approach the problem from the perspective of "scientific paradigms" and "normal science", concepts developed in T. S. Kuhn's: The Structure of Scientific Revolutions. It is argued that this approach avoids the pitfalls of earlier historical attempts at reconstructing a logic of scientific discovery, and also that "paradigms" are tacitly functioning as a presupposition of most contemporary analyses of scientific problem solving.

Chapter I begins by articulating the prevailing point of view on the problem, and traces this point of view to the early logical positivists. The positivists' position is explicated, and then attacked with four arguments which show the positivists' position to be unconvincing. A new framework is introduced for the purpose of clarifying the true nature of the problem. Within this framework a distinction is made between a "method for suggesting hypotheses" and a "logic for suggesting hypotheses", and it is suggested that an adequate analysis of the method might provide the foundation for a logic of discovery.

Chapter II examines the historical origin of the problem in the early nineteenth century. The views of William Whewell and John Stuart Mill are considered in the context of their famous controversy over the nature of induction. It is pointed out that, to a large extent, this controversy was really a debate about the logic of discovery under another name. The views of both philosophers are critically analyzed for their respective contributions to the problem.

Chapter III traces the problem through the later nineteenth century through the work of Charles Sanders Peirce. Peirce's theory of "abduction", as an inferential method of discovery, is examined and criticized. Peirce's general argument that scientists reason their way <u>to</u> hypotheses is accepted, but it is argued that "retroduction" fails to capture this reasoning as a form of inference.

Chapter IV examines and criticizes the many arguments of Norwood Russell Hanson on the subject. Despite the many valuable suggestions put forth by Hanson, it is found that Hanson failed to provide a method for distinguishing the class of <u>plausible</u> hypotheses from the larger class of <u>possible</u> ones.

Chapter V introduces the notions of 'paradigm' and 'normal science'. Earlier arguments, and three contemporary approaches are examined from this point of view. It is shown how the Kuhnian conceptions of 'paradigm' and 'normal science' are presupposed by these approaches; and it is suggested that the proper analysis of these concepts might aid in future research on this problem.

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CHAPTER I

OUTLINE OF THE PROBLEM

1. The Prevailing Point of View: the Legacy of Positivism

In Experience and Prediction, Hans Reichenbach introduced the terms "context of discovery" and "context of justification" into the philosophical analysis of scientific methodology. In brief, the context of discovery is concerned with the origins of scientific hypotheses (i.e., hypothesis generation), and the context of justification is concerned with the acceptability of proof (i.e., hypothesis confirmation) of hypotheses once they have been put forward. Since the time Reichenbach introduced these terms, most of the work in the philosophy of science has been focussed largely on problems arising in the context of justification. Unfortunately, problems associated with the context of discovery have been comparatively neglected by most philosophers of science. This neglect however has not been entirely fortuitous. Following the views set down by Reichenbach, Carnap, Popper, and others, many modern philosophers have regarded the context of discovery (either tacitly or explicitly) as the exclusive domain of historians, sociologists, and psychologists. In effect, many philosophers have accepted the view, stated most clearly by Popper, that the discovery of scientific hypotheses is not a philosophical problem at all. Popper states, for example, that:

> The initial stages, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man -- whether it is a musical theme, a dramatic conflict, or a scientific theory --may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.[1]

Elsewhere, even more pessimistically, Popper continues:

> My view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains 'an irrational element', or 'a creative intuition' in Berson's sense ... There is no logical path ... leading to these ... laws. They can only be reached by intuition, based upon something like an intellectual love (Einfülung) of the objects of experience.[2]

The further extent to which Popper's rather dismal conclusions about the logic of discovery have been absorbed by contemporary philosophers is reflected throughout the secondary source literature on the subject. Indeed, many of these writers are non-positivists who continue to reflect this particular point of view toward the problem. To wit, Richard Rudner writes:

> To the context of discovery, on the other hand, belong such questions as how, in fact, one comes to latch on to good hypotheses, or what social, psychological, political or economic conditions will conduce to thinking up fruitful hypotheses. In short, the issues or questions appropriate to the context of discovery are, themselves, substantive issues or questions in the social sciences. They are questions to be answered by the sociology, or the psychology, or the history of science rather than by the philosophy of science.[3]

Similarly, Braithwaite says:

> The solution of these historical problems involve the individual psychology of thinking and the sociology of thought. None of these questions are our business here.[4]

These statements express a widely prevailing point of view towards the <u>context of discovery</u> as a whole; it has come to be part of the "received opinion". Indeed, scarcely a student gets through a course in the philosophy of science without having digested this precious nugget of "wisdom" that: "there is no logic of scientific discovery". This point of view toward the problem of discovery is part of the legacy bequeathed to us by positivism. Against this background therefore, it is not surprising that discussion of this topic has been neglected in contemporary philosophical literature. What _is_ surprising, however, is the extent to which the positivist's conclusions about a logic of discovery have been accepted without serious challenge or critical reflection. That there should be so much premature closure on the question of a logic of discovery is not only uncritical, but it strikes me as being peculiarly unphilosophical.

When one considers the totality of problems in the philosophy of science, there is little doubt that the problems associated with the context of justification are of the first order of importance in clarifying the structure of scientific knowledge. The genesis of these problems is the history of epistemology itself; thus, it must be admitted, that the emphasis in the philosophy of science has not been misdirected. On the other hand, when a philosopher (or group of philosophers) suggests that "X is not a philosophical problem" it all too often simply means that he is not interested in that problem. In the case of _discovery_, in particular, it is quite clear that much of the neglect and disinterest has been brought about by an uncritical acceptance of this part of the positivist's legacy. In the section to follow, I will attempt to show that the true nature of the problem has been obscured by some confusions, misunderstanding, and, in some cases, by simple bad arguments. The purpose of the analysis will be to show that there are no good reasons for not studying the _context of discovery_ with an eye toward rational reconstruction, and to show that the positivists' position on this issue is without foundation.

I would like to make it clear at the outset that my use of

Reichenbach's distinction between the _context of discovery_ and _context of justification_ is primarily for exegetical purposes; it is a useful way of focussing on the problem area. However, it should not be inferred from this that I am relying upon this distinction in an uncritical way. Indeed the _raison d'être_ of the following discussion is to bring out and expose many of the unspoken assumptions, and unwarranted conclusions, which are usually associated with this distinction (i.e., I want to expunge from its use many of the associated ideas which are usually read into that distinction). Thus, my use of the distinction between the _context of discovery_ and _context of justification_ in subsequent parts of this essay will not prejudge the issue about a logic of discovery, as has all too often been the case in the writings of contemporary philosophers. The neutral use of this distinction has been long overdue.

Secondly, it should also be pointed out that it is possible to distinguish several different claims about a logic of discovery which the positivists themselves rarely bothered to distinguish. Indeed, it is the conjunction of these distinguishable claims that has come to represent the positivist view, as though there were just one claim being made. The positivist tradition has run together several different claims about discovery, each one of which tends to feed off the others, yet no one of which is convincing when considered in isolation.

In the discussion which follows I will attempt to separate, and analyze, each of the distinct claims which the positivists have run together in forming their view about a logic of discovery.

2. Some Common Confusions Within the Positivist's Position

(2.1) The De Facto Argument

Consider for a moment the meaning of the statement: "there is no logic of scientific discovery". What precisely is being claimed in that statement? The positivists themselves are not entirely clear about this, but frequently either or both of the following is intended: (A) the process of discovering scientific hypotheses does not, as a matter of fact, conform to any existing or familiar logical system; or (B) no logical system, familiar or otherwise, could be constructed such that the discovery of hypotheses is an adequate interpretation of that logic (i.e., one cannot construct a logic which captures the discovery of hypotheses). The first claim, or intended meaning, I call the "de facto claim" since it purports to describe the current state of affairs on the issue about a logic. The second claim, or implied meaning, I call the "in principle claim" since it denies the possibility of constructing a logic of discovery. Unfortunately, these two distinguishable claims are easily (and frequently) run together in discussions about the logic of discovery.

The first claim is true, but it is also trivial, since it is noncontroversial even amongst proponents of a logic of discovery. The de facto statement that "there is no logic of discovery", simpliciter, is uncontested. The first claim however, does not entail the second, and to suggest that it does is a simple non sequitur. One can admit the fact, as some modern proponents do, that there is no logic of discovery and deny the impossibility of constructing one. It may simply be a contingent matter of fact that no one has successfully constructed a logic of discovery. However, the positivist's position frequently employs the truth of the *de facto* claim to serve as a tacit premise for accepting the stronger "in principle" claim as well. Many modern writers have inadvertently accepted this bad argument more or less uncritically, and the stronger claim has since taken on the status of an eternal verity.

There is a second argument that can be found in the literature which might be considered a corollary to that analyzed above, which appears to trade off the same confusion. This argument suggests that because there is no logic of scientific discovery (i.e., the *de facto* claim), it follows that there are no *philosophical* problems, as such, in the context of discovery. Or to put it another way: if there is no logic involved, then there are no philosophical problems in this context either. Recall R. Rudner for example (above) discussing the question how "one comes to latch on to good hypotheses ...?", and then answering that these "are questions to be answered by the sociology, or the history of science *rather than by the philosophy* of science". And similarly Popper says: "The initial stages, the act of conceiving or inventing a theory seems to me neither to call for logical analysis nor to be susceptible of it ... it is irrelevant to the logical analysis of scientific method".

To see the weakness in this particular argument, or this version of the argument, one need only consider the question: "Was there a logic of deduction before Aristotle formulated his syllogistic logic?". Surely, most of the philosophers prior to Aristotle were employing some form of logic in their writings. One might say that Aristotle merely articulated some of the already existing rules of inference in a more formal and systematic way than had been considered possible theretofore. But however one decides on the question of whether or not there was a pre-aristotelian logic, it ought, at any rate, to be admitted that Aristotle's attempt to formulate his logic (i.e., what he took to be all the valid forms of inference) was a philosophical endeavour _par excellence_. No one to date, it is true, has formulated an adequate logic of discovery (this is simply the _de facto_ claim again), but along lines analogous to those suggested above, one might argue that a logic of discovery is still in its "pre-aristotelian state" -- and the search for one is philosophically challenging. Thus, it does not follow that because there is no logic, there is no philosophical problem.

(2.2) _The Psychological Argument_

A second claim which can be seen at times in the positivist's position is the assertion that the context of discovery, taken as a whole, contains _only_ questions of a psychological (or sociological) nature. According to this view, the discovery of scientific hypotheses is a more or less random affair depending primarily on the psychological states of individual scientists. This view is usually advanced because it is assumed that the facts about discovery are of purely psychological, sociological, and historical interest -- there is no task for logic to perform in this context. That is, the context of discovery is alleged to be the exclusive domain of empirical investigation. For example Rudner says: "To the context of discovery belong such questions as ... what social, psychological, political or economic conditions will conduce to thinking up fruitful hypotheses ..."; and these are questions to be answered by disciplines other "than the philosophy of science".[5] Similarly, Carl Hempel writes: " ... while the process of invention by which scientific discoveries are made is as a rule psychologically guided and stimulated by antecedent knowledge of specific facts, its results are not logically determined by them."[6]

One must admit, I think, that whatever else philosophy and logic may be, they are not empirical fact-gathering disciplines in the sense that sociology, psychology and history are. However, to assert that the context of discovery is _exclusively_ the domain of empirical investigation is a _conclusion_ which requires the support of argument; it is not an _a priori_, nor obvious, truth. As N. R. Hanson has argued, "even if these philosophers are correct that there are no logical dimensions to the discovery of hypotheses, then that fact should come as the conclusion of some argument and not as its preamble."[7]

The context of discovery, taken as a whole, obviously includes much that is of peculiar interest to psychologists and other empirical disciplines; the positivists, and others, have been particularly aware of this. However, the point at issue here is whether or not the process of discovering scientific hypotheses is _purely_ (or just) an empirical question. The problem of a _logic_ (as such) of discovery can be properly viewed as a part, or sub-set, of the problems within the context of discovery taken as a whole.[8] In effect, to simply assume that the context of discovery is exclusively a matter for empirical investigation, as is implied by the statements of Popper, Hempel, and Rudner, is to beg the philosophical question at issue.

(2.3) _The D-N Model as the Arch Paradigm of Rational Reconstructions_

Perhaps because of the positivist's exclusive concern with the "context of justification", there has been a widespread tendency, extending beyond the positivists themselves, to _identify_ the concepts of 'rational reconstruction' and 'logic' with what goes on in the "context of justification" alone. Indeed this association is quite understandable when one considers the following two points: (1) the deductive-nomological model is the most familiar "rational reconstruction" (along with, perhaps, Tarski's and Carnap's) that we have at the present time, and these are reconstructions in the "context of justification"; and (2), our concept of 'logic' is almost always associated with 'proof' which is, again, the concern of the "context of justification".

However, it must be recognized that the over-identification of these concepts (i.e., 'rational reconstruction' and 'logic') with what goes on in the formal structure of the "context of justification" alone, is both dangerous and misleading. It is dangerous because the context of discovery is not concerned with establishing the truth of scientific hypotheses (as is "justification"), but rather with the reasoning employed to suggest those hypotheses in the first place; and it is misleading to suppose that 'rational reconstruction' and 'logic' are concepts which can only apply to the products of scientific research, and not to the process (or "logic-in-use") of scientific reasoning prior to testing. As Abraham Kaplan has pointed out:

> The distinction between discovery and justification, and between their respective contexts, is valid and important. I suggest, however, that the limitation of logic to the context of justification stems from confusing this distinction with the one I have made above between logic-in-use and reconstructed logic. Because our reconstructions have occupied themselves with justifications, we have concluded that there is no logic-in-use in making discoveries.[9]

The predominance and general familiarity of the deductive-nomological model of scientific theories has been functioning, at least tacitly, as the arch-paradigm for all rational reconstructions.

Indeed, the phrases "logic of science" and "rational reconstruction" are used almost exclusively in connection with the D-N model per se; and in many instances the respective concepts are used interchangeably. Thus, when the question is raised as to whether the context of discovery might be rationally reconstructed, or indeed, whether there might be a logic of discovery, philosophers have tended to expect, if not require, a replication of the reconstructions found in the context of justification, such as the D-N model, in particular. This line of thinking however, whether it be explicitly stated or merely tacitly assumed, is misguided and unreasonable. To appreciate just how this line of thinking is misguided, it is helpful to consider the following points.

First, even according to the positivist's own conception of a 'rational reconstruction', the possibility of reconstructing a model for discovery is not ruled out.[10] And this can be said despite the fact that their sole purpose was to reconstruct the "context of justification". In general, by the term "rational reconstruction", they meant an abstract descriptive model which makes manifest the form of reasoning involved in a given method. For example, Popper describes 'rational reconstruction' as follows:

> I refer to the possibility of adopting ... what may be called the method of logical or rational reconstruction, or perhaps the 'zero method'. By this I mean the method of constructing a model on the assumption of complete rationality on the part of all the individuals concerned, and of estimating the deviation of the actual behaviour of people from the model behaviour, using the latter as a kind of zero co-ordinate.[11]

It is important to recognize that the reconstructed model is an idealization designed to capture the reasoning in certain (prespecified) types of tasks, and that the model will change as the nature of _the task changes_. The deductive-nomological model is a rational reconstruction designed to capture the proof procedure of scientific hypotheses once they have been put forward; this is just one (though important) type of rational endeavour. The context of discovery, on the other hand, is concerned with a _different task_, namely, the initial suggestion of scientific hypotheses. Thus, it would be unreasonable to expect, much less require, a replication of the logical models we have for the context of justification. Because the nature of the tasks are so different, at least _prima facie_, there is good reason to suppose that the reconstruction will be correspondingly different as well. For example, a reconstruction for _discovery_ need not employ deductive logic, as does the D-N model for _justification_. It might be possible to construct alternative models of reasoning for both context; thus, it is intellectually dangerous to become overly enamoured with one particular model, from one context.

Secondly, when the positivists assert that "there is no logic of scientific discovery", and mean by that "that discovery has no logic of the type found in the context of justification (i.e., the H-D model)", they are saying nothing of which many earlier philosophers in the nineteenth century were unaware. Indeed, as the following chapters of this essay will show, Whewell, Peirce, and later Hanson understood the problem of discovery to be precisely that of reconstructing an _alternative_ model for the context of discovery. They were not looking for the same type of solution to the problem which the positivists' position seems to require. Thus, to hold the view that "there is no deductive logic of discovery, therefore, there is _no_ logic of discovery", is simply to manifest insensitivity, or ignorance, about what the problem of the logic of discovery has been historically. The subsequent historical analysis of the problem will help to demonstrate (among other things) the inappropriateness of the positivists' conception of the problem.

3. The Basic Philosophical Problem

The basic philosophical question of the logic of scientific discovery is whether or not there is a logical method for generating or suggesting plausible hypotheses in science, as distinguished from the question of whether there is a logical method for justifying and confirming those hypotheses. The problem, therefore, is to show that there is a rational method for suggesting untested hypotheses in science, and that this method is sufficiently rational to qualify as logic. The present essay will be primarily concerned with the first part of the problem (i.e., whether there is a rational method). And I shall argue that there are rational methods for suggesting plausible hypotheses in certain restricted (though important) areas of scientific investigation; but more of this later, however.

This is a very brief, thus bold, statement of the basic problem. However, like most problems in philosophy, much more needs to be said in order to bring the problem into sharper focus. Indeed, the purpose of this essay is to put the problem into a clearer and more manageable perspective than it has been heretofore. Toward this general end therefore, it attempts:

(1) To reformulate the problem in such a way that it is clear and congenial to modern readers, while at the same time does not trivialize any claims for or against a logic of discovery.

(2) To analyze and assess contributions from the recent history of the problem (starting with the nineteenth century) against the background of this reformulation of the problem.

and (3) To point out what remains to be done in order to accomplish a satisfactory solution to the problem, and (in the last chapter) to suggest a new and untried approach to the problem which appears promising.

In examining these particular objectives, it can be seen that no definite solution to the complete problem is offered. However, given the sparse and chaotic state of the literature in this context, such basic groundwork is not only useful for understanding the problem, but it appears necessary in order to recognize and assess putative solutions.

4. A General Framework

It has already been pointed out that much contemporary thinking about the problem of the logic of discovery (or PLD)[12] has been influenced by the positivist's approach to this problem. And, it has been argued, this particular approach is not the most neutral nor productive approach. In this section therefore, I should like to make several points which help distinguish the present approach from that of the positivist's and which will also serve as a general framework within which the remaining discussion of the problem will take place.

First, it should be recognized that there are at least two features of the PLD which are quite similar to the problems encountered in the logic of confirmation. The first is the fact that both problem areas are investigations into scientific method, as such, and neither area is concerned with the contingent psychological thought processes of individual scientists. A second feature which both problem areas share in common is that they are both concerned with rationally reconstructing their respective parts (or aspects) of scientific method, with an eye toward showing (or revealing) the rational structure of that aspect. On these two points at least, the context of justifiand (3) To point out what remains to be done in order to accomplish a satisfactory solution to the problem, and (in the last chapter) to suggest a new and untried approach to the problem which appears promising.

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A second general point which should be kept clearly in mind when focussing on the PLD, is the basic distinction between the <u>context of justification</u> and the <u>context of discovery</u>. N. R. Hanson has expressed this difference most succinctly, and in terms that are faithful to both problem areas. He has distinguished between:

(1) reasons for accepting an hypothesis H

from

(2) reasons for suggesting H in the first place.[13]

The latter (i.e., (2)) is the locus of philosophical concern in the <u>context of discovery</u>. The present essay accepts this way of distinguishing the two problem areas since it accurately points to the true center of interest in the PLD.

On the question of the origin of scientific hypotheses, or where they come from, the H-D account must, according to its own principles, remain silent.[14] The two contexts simply have different methodological interests; and of these different interests, Hanson frequently pointed out that:

> H-D accounts begin with the hypothesis as given, as cooking recipes begin with the trout. Recipes however, sometimes suggest, "First catch your trout". The H-D account is a recipe physicists often use after catching hypotheses. However, the conceptual boldness which marks the history of physics shows more in the ways in which scientists <u>caught</u> their hypotheses than in the ways in which they elaborated these once caught. To study only the verification of hypotheses leaves a vital part of the story untold -- namely, the reasons scientists had for thinking their hypotheses would be of one kind rather than another.[15]

In general then, the present discussion considers the <u>context of justification</u> to be concerned with rationally reconstructing the method of justifying or confirming scientific hypotheses <u>once they have</u>

been put forward; whereas the context of discovery is concerned with rationally reconstructing the reasons which initially suggest scientific hypotheses.

A third general point which forms part of the present approach to the PLD, and which distinguishes this approach from the positivists', is that of making a clear distinction between 'having a method (simpliciter) for some process' and 'having a logic for a given process'. It should be clear, in the present framework at least, that not all methods need amount to logics strictly so-called, and conversely, not all logics specify the structure of a single method. The positivists however, in the context of justification, frequently write as though the rational reconstruction of the method of confirmation is tantamount to providing a logic, strictly construed, of confirmation. But this is a confusion, since many methods (e.g., planning a community, or building a bridge) are clearly not logical in the strict sense.[16] Perhaps the reason the positivists often confused this distinction was because they assumed from the outset that the method of confirmation is intrinsically rational; and they saw their function as simply formalizing that inherent rationality.[17]

Interestingly, as was mentioned earlier, the positivists made just the opposite assumption about the context of discovery -- they assumed it was non-rational. However, in both the context of justification and the context of discovery it has not been sufficiently recognized that the question of whether a method exists is independent of the question of whether a logic exists. It should be noticed, for example, that for any method, whether it be for confirming hypotheses or suggesting them, it is possible to ask "Does the method conform to a pattern of reasoning which we might reasonably call a logic?" Thus the present essay shall adopt the point of view that the PLD is really a twofold problem (i.e., has two phases). There is, first, the problem of establishing that there is a general _method_ for suggesting plausible hypotheses. (As will be shown later, most of the history of the problem focussed on this area). And secondly, there is the problem of showing that this general method (if there is one) has a _logic_.[18] Both of these problems are but parts of the general PLD.

There are two additional points about this way of describing the problem which are worth noting here. First, in so far as the positivists have relegated the _context of discovery_ to the realm of "psychology" and "intuition", they have, in effect, denied that there is any _method_ (as such) in the _context of discovery_. Their position is that the _context of discovery_ cannot be rationally reconstructed because there is no _method_ there to reconstruct. Hence it can be seen that even the first part of the PLD (i.e., showing that there is a method) is a legitimate philosophical problem in itself. Despite this, and at least partially because of the dominance of the positivist tradition, philosophers like Peirce and Hanson have had a difficult time being heard on this question. Secondly, it might also be noted in this connection, that the establishment of a clearly articulated method for suggesting plausible hypotheses would be an important first step toward providing a logic of discovery. Once the method is understood, then it becomes possible to formulate in a clear fashion the question: "Does this method conform to a pattern of reasoning which we might reasonably call a _logic_?" And once the method has been clearly characterized, perhaps in terms of certain principles and rules, then the problem of formulating a logic, strictly so-called, becomes that much easier.

The foregoing points constitute the general parameters within which the present essay approaches the PLD; in addition to the few remaining definitions, the subsequent investigation in later chapters should help to fill-out and develop this framework still further.

5. Some Working Definitions and Related Distinctions

In discussing the PLD, as in most discussions, it is important to have a clear and consistent understanding of the central terms used in the discussion. The explication of the PLD, in particular, requires a clear understanding of three of its major terms, and these are: discovery, hypothesis, and logic. In this section I should like to offer three working definitions which not only provide the relevant meanings of these terms, in particular, but which also contribute to an understanding of the problem as a whole.

(5.1) Discovery

Of the three terms to be clarified here, 'discovery' is perhaps the only one which departs somewhat sharply from ordinary usage when it is used in connection with the PLD. When the "layman" (and by "layman" here, I mean simply anyone not considering the discovery issue) uses a locution of the form "X discovered Y", the 'Y' can range over such diverse things as: physical objects, sets of physical objects, properties, relations, concepts, theorems, laws, explanations, proofs, etc.; in short, various things that are regarded as matters of fact. But when philosophers or scientists engaged in arguments about the logic of discovery use the term 'discovery', the objects (or referents) of 'discovery' are usually hypotheses. The difference between this more technical use of the term and the nontechnical (or "layman's") use is significant. The nontechnical use of 'discovery' has matters of fact, or objects, as its referents, whereas the technical use (which is of concern here) has hypotheses which account for facts as its referent.[19] Hypotheses are not physical facts in the same sense that spatio-temporal objects, or states of affairs are facts. From the point of view of ordinary discourse then, it can be seen that the object of discovery in the debate about the logic of discovery, is somewhat unusual.

From the point of view of logic, there is yet another peculiarity of the term 'discovery' in this context, and it derives from the requirement of acceptability. Normally, for something to be a bona fide scientific discovery it must be accepted as true; but in connection with the PLD this requirement is inapplicable because there is no truth claim being made. The claim in this context is more like saying this or that particular explanation or description is highly plausible (prior to testing). Hence the requirements (or criteria) of acceptability for suggested hypotheses need not be the same as those for matters of fact.

There is a sense in which even well established scientific theories, of any kind, are never completely accepted as finally true. Due to the open-ended character of scientific theories, a characteristic which safe-guards objectivity, it is always possible for the theory to become discredited or abandoned if future tests should reveal sufficient negative data (e.g., when "black swans" begin showing up!). Mario Bunge, in fact, attempted to use the open-ended character of science as an argument against the possibility of a logic of discovery. He attempts to do this in the following way:

> Is there an infallible technique for inventing scientific hypotheses that are likely to be true? In other words, is there a method, in the Cartesian sense of a set of "certain and easy rules" leading us to state factual truths of a wide extension?... such an art was never actually invented. What is more, it may be argued that it will never be invented unless the definition of science is radically changed; indeed, scientific knowledge, as opposed to revealed wisdom, is essentially fallible, that is, susceptible of being partially or even totally refuted. The fallibility of scientific knowledge and consequently the

> impossibility of establishing golden rules leading us straight-forward to final truths, is but the complement of that verifiability we had found as the core of science. That is, there are no infallible rules _guaranteeing_ in advance the discovery of new facts or the invention of new theories, thereby securing the fruitfulness of scientific research; certainty is to be found in the formal sciences alone.[20]

The argument can be summarized in three steps as follows:

(1) A logic of discovery ("_ars inveniendi_") is an infallible method for inventing true scientific hypotheses.

(2) Scientific statements are essentially fallible; i.e., no scientific statements are final truths.

(3) Therefore, there is no logic of discovery.

There are several crucial deficiencies in Bunge's argument, not the least of which is the fact that he begins (in the first sentence) by raising the question of whether a method can be developed for producing "hypotheses that are _likely to be true_", and then his argument turns to show that no method can be developed to produce statements or theories that are _certainly_ (i.e., infallibly) _true_ -- which, of course, was not the question. In effect, his argument fails to answer his own question -- the question which is of interest here.

Aside from this, however, it can also be seen that Bunge is construing the PLD in the classical "Cartesian" or "deductive" sense, a sense which (as indicated earlier) fails to capture the nature of the problem when it is properly conceived. The classical "Cartesian certainty" which Bunge is discussing has little or no place in the modern conceptions of scientific method, let alone in the context of the PLD.

But perhaps the most illustrative point which Bunge's argument brings out, and the point which is central to the present discussion, is that the logic of discovery problem is not one of establishing truth (as Bunge correctly rejects as impossible), but it is a problem of suggesting hypotheses "that are likely to be true" (the question Bunge never answers). Bunge's discussion is illustrative of the confusion between the two senses of 'discovery' which I have been attempting to distinguish and characterize, viz., the nontechnical (or "layman's") use and the technical (or "logic of discovery") use. These two uses of the term "discovery" are logically and epistemically distinct. And confusions such as Bunge's are bound to ensue when the distinction is not observed.

In sum, the working definition (or concept) of 'discovery' which will be used in the remainder of this essay will be what I have dubbed the "technical use" of 'discovery' as opposed to the "layman's" or nontechnical use. In short, it simply means the construction of plausible scientific hypotheses which are alleged to account for facts or data. Thus, even a plausible hypothesis which, with further tests and examination, turns out to be false could qualify as a discovery. 'Discovery', in this context, does not have facts as its object (or referent) in the same physical sense as the layman's use does. While this definition of 'discovery' departs somewhat from its usage in ordinary discourse, it does, I think, capture the intended meaning which has been the most pertinent for the PLD.

(5.2) Hypothesis

Since hypotheses are the object of discovery in the PLD, it is necessary to have a clear and stable understanding of what the term 'hypothesis' means in this context. Although there has been some discussion, and confusion, in modern literature about the precise nature of hypotheses,[21] these particular problems need not impinge upon our grasp of the PLD as such.

In general, most of the recent discussion about hypotheses centers around the criteria which good hypotheses should meet. For example, there appears to be at least a prima facie agreement amongst philosophers that good hypotheses should be: consistent, testable, projectable, and simple. Yet the meanings of these terms are themselves unclear, and function as the source of several separate discussions. These discussions however, usually arise in the context of justification because of special problems which they pose there. Fortunately, for the purpose of understanding the PLD in particular, there is a standard, or core meaning of 'hypothesis' which is perfectly adequate, and poses no special problems in this context. In the present context, by the term 'hypothesis' it is sufficient to understand that it is simply: a proposition or descriptive model which is tentatively advanced as a proposal to accept this proposition as true, or this model as applicable, with the express purpose in mind of attempting to confirm or disconfirm it.

Perhaps the most interesting and important characteristic of hypotheses is that their epistemological status is more like a conjecture than an established truth. As Wartofsky points out:

> Hypotheses are tinged with tentativeness, with the conscious qualifier, "What if such and such were taken to be the case?" as against the assertion, "Such and such is the case".[22]

The amount of confidence which can be placed on any hypothesis is, of course, contingent upon the amount and type of empirical evidence in its support. The phrase "fruitful hypothesis" does not mean that a given hypothesis is true, but rather something like "it fits very closely with available evidence", or that "it is rich in explanatory power".

This much having been said about hypotheses, I think it can be seen that the meaning of 'hypothesis' in this context is quite straight-forward and presents no special difficulties for the PLD as such. In short, the standard meaning of 'hypothesis' (as underlined on page 21) is all that is meant in this context.

(5.3) Logic

It has already been pointed out that the PLD consists of two distinguishable problem areas: first, that of establishing that there is a rational method for suggesting scientific hypotheses, and second, establishing that this method has a logical structure, in the strict sense. While much of this essay examines the question of whether a rational method exists (historically, a formidable task in itself), it is equally important to have a clear idea of what is meant by 'logic', since it is both the focal point and ultimate desideratum of the question, "Is there a logic of scientific discovery?". In this section therefore, I shall suggest a working definition of the term 'logic' which captures the formal features normally associated with the term by modern philosophers, and which is also faithful to the basic problem.

Stephen Kőrner points out that:

> Traditionally, the task of logic has been conceived as that of providing criteria of correctness for inference by making explicit the rules which are conformed to by correct inference and violated by incorrect ones; or by characterizing in a general way those propositions which state that one

> proposition follows from another; and by systematizing these rules and propositions as fully and efficiently as possible.[23]

The "rules" which Körner refers to here are known more simply as inference rules. Other philosophers normally demand that logic must also contain formation rules as well. The presence of such rules is typically regarded as a necessary condition for a system to be regarded as a logical system, in the strict sense. Thus, the present essay shall maintain this condition as a central feature of the working definition of 'logic' to be employed here.

It should be recognized, however, that no one logical system is so universal as to be useful in every type of situation involving reasoning. Indeed, there are many types of logical systems, both inductive and deductive, which are designed to be used in only certain types of problem situations.[24] (Moreover, there are coherent formal systems, particularly in pure mathematics, which do not have any foreseeable application in any problem area). Thus, owing to the diversity inherent in the many types of logical systems no attempt shall be made here to identify all the necessary and sufficient conditions for designating a system 'logical'. Rather, what will be suggested here (i.e., for present purposes) is that the two necessary conditions referred to by Körner (above) can be usefully, and fairly, employed as both the necessary and sufficient conditions for regarding a system as logical. Such a conception of 'logic' has the advantage of capturing the formal features of logic which the pre-analytical intuitions of philosophers would normally require, and is at the same time, broad enough to include the various types of inductive and deductive systems which historical philosophers working on the PLD may have had in mind.

Hence, the following two conditions, or criteria, will serve as the working definition of the term 'logic' throughout the present analysis of the PLD:

(1) That there exist at least one repeatable rule or procedure which functions as an inference rule.

and

(2) That there be at least one statement serving as a formation rule (i.e., specifying which statements or formulae are "well formed").

Any system which possesses these two characteristics shall be said to qualify as a "logical" system in the present essay.

I should also like to make it clear that, in general, the normal, or standard meaning of the terms 'inference rule' and 'formation rule' are intended to apply here. For example, by 'inference rule' I mean that there exist some pre-specified and repeatable procedure or operation which enables one to pass from one premise or judgement (or indeed, sets of these) to another.[25] And by the term 'formation rule', I simply mean some statement (or set of statements) which specify what general class of formulae are to be regarded as "well formed" (i.e., which formulae are syntactically coherent).[26]

Having given this working definition of 'logic' for the present context, let me draw attention to a few points which might inadvertently be overlooked in this connection. First, inductive rules of inference such as those found in the calculus of probability (e.g., Bayes' theorem), or even various tests of statistical significance, shall qualify as inference rules, despite the fact that they are non-effective rules of inference. Similarly, this definition of logic leaves open the possibility of including systems which might employ various heuristic devices as rules of inference. Such possibilities must be included in order to avoid any *a priori*, or unreasonable, conceptions of what is to count as a logic. In connection with the PLD, in particular, Wesley Salmon has been especially sensitive to this point, and he admonishes us as follows:

> Turning to the problem of a logic of discovery for empirical science, we must be careful not to pose the question in an unreasonable way. To suggest that there might be a mechanical method that will necessarily generate true explanatory hypotheses is a fantastic rationalistic dream. Problems of discovery completely aside, there is no way of determining for certain that we have a true hypothesis. To make such a demand upon a logic of discovery is obviously excessive. Not since Francis Bacon has any empiricist regarded the logic of science as an algorithm that would yield all scientific truth.[27] (Underlining is mine).

A second point which should not be overlooked in connection with a logic of discovery concerns the scope, or breadth, of applicability of such a logic. Since the time of Aristotle's Syllogistic there has been a tendency amongst some philosophers to think that logic must be universal in scope, and must apply in all manner of situations. However, it is now more clearly understood that there may be no such logic, and that logics typically have a certain restricted domain of applicability. Thus in connection with the PLD, in particular, it might turn out that a given candidate for a logic of discovery is only applicable to a certain field, or area, of scientific investigation. As R. D. Carmichael (a mathematician) has astutely observed:

> In several places, I have met the term logic of discovery but seldom or never the notion of *logics* of discovery. It is conceivable that the logic of discovery is not one in the sense of something indivisible, but that it is relative to the field of investigation or the point of view so that one should not speak of a logic of discovery in any absolute sense, but only of such a logic as relative to a given discipline or a given goal of investigation.

> The usual failure to divide the problem into the parts thus suggested has, I believe, been a chief hindrance to the development of the logic of discovery. The fact that the logic of demonstration is a unit, being the same whatever the field of investigation, has led to a too ready acceptance of the view that a logic of discovery should also be a unit.[28]

With respect to the PLD in particular, one must guard against the assumption that a candidate for a logic of discovery in organic chemistry (say) must necessarily be applicable in physics and sociology as well. As was suggested earlier, what may be a rational method for suggesting plausible hypotheses in one area, may turn out to be totally fruitless in another.

In general, while 'logic' is here being defined in terms of inference rules and formation rules, the nature of the PLD in particular requires that one not lose sight of the following: (a) one cannot justifiably demand that the rules of inference be <u>just those rules</u> which are already familiar from other branches of logic, since such a demand might rule out the possibility of a logic of discovery on <u>a priori</u> grounds; (b) the rule(s) of inference need not be <u>effective</u> (i.e., deductive) in character; and (c) the scope (or range of applicability) of such a logic need not be universal, but may be applicable only to certain restricted areas of scientific investigation.

I think the foregoing definition of 'logic' is sufficiently clear so as to provide at least a fair and consistent meaning to the term throughout this essay. However, as was pointed out earlier, the basic, or most primitive problem for the PLD concerns the question of whether or not there is a <u>methodology</u> of discovery; and it is only after this question has been answered satisfactorily that the question of a <u>logic</u>, as such, becomes appropriate.

It should be understood that 'method' is a far broader concept than is 'logic', and cannot be characterized in terms of formal rules. It is sufficient for present purposes to understand by 'method' any orderly practice or procedure for accomplishing some task which is <u>not</u> characterized by an appeal to formal rules, such as 'logic' was, (i.e., there is an absence of formal rules). In short, by the term 'method' I mean simply what a standard dictionary definition would suggest. (See, for example, footnote 16).

As the PLD has been thus far described, the first order of business is to determine whether the method(s) of suggesting plausible hypotheses can be adequately <u>reconstructed</u>, and then secondarily to address the question of whether the reconstructed method(s) might be captured by a set of rules.

6. The PLD in Perspective

While philosophy is to some extent a speculative enterprise, it is important, at some point, to make an attempt to keep one's feet on the ground. I should like therefore to make a few brief comments about the relevance of the PLD for the philosophy of science in general.

For the purpose of gaining an untinted perspective on the PLD, it is sometimes helpful to ignore the "received opinion" (discussed earlier) for a moment, and to consider the problem in a fresh light, that is, in a relatively naive way. If one looks at the day-to-day scientific enterprise, for example, at least one of his surface impressions will be that the suggestion and formation of hypotheses is not an unusual occurrence. Indeed, the designing and testing of experiments never begins <u>until</u> such an occurrence has taken place. He would also notice, depending on the particular science he was observing, that the hypotheses are usually of some general type, and that a whole host of other possibilities are never seriously entertained. There are very few journals on palmistry, black magic, or horoscope lying around laboratories. With just these few petty observations one's intuitions would perhaps suggest to him that something more than "wild guessing" was going on in the suggesting and formation of these hypotheses. The phenomenon of formulating plausible hypotheses in science is simply too common, and the "guesses" too good, to suggest that nothing more than "flashes of genius" and "irrational creative intuition"[29] are operative here. Thus, contrary to the "received opinion" at least the *prima facie* case would seem to favour the presence of some rational consideration in the discovery of hypotheses. The serious student of scientific method ought not to minimize the importance of these surface impressions because of a prevailing dogma to the contrary.

This is not to suggest that one is faced with an exclusive choice between "wild guessing" and "logical method", but it is to suggest that there appears to be some kind of rational method at work in this phase of scientific investigation. And it is the business of the PLD to attempt to uncover that method, and to examine its inner workings very carefully.

Moreover, if Braithwaite is correct in asserting that:

> The business of a philosopher of science is primarily to make clear what is happening in scientific thinking[30]

then philosophers have been unduly advised to limit their attention to the thinking that goes on after a hypothesis has been proposed. As N. R. Hanson has observed, "There have been virtually no serious analyses of the concept of discovery by philosophers of science at all."[31]

In addition to increasing our understanding of scientific method as a whole, study of the "context of discovery" could have some practical benefits as well. For example, David Bakan, a methodologist in psychology, has suggested that our lack of understanding about the "context of discovery" has hindered progress in the social sciences in particular. He writes:

> There is nothing intrinsically wrong with the emphasis upon the testing of hypotheses. It is an important part of the total investigatory enterprise. What I do wish to point out, however, is that by the time the investigatory enterprise has reached the stage of testing hypotheses, most of the important work, if there has been any, has already been done. One is tempted to think that psychologists are often like children playing cow-boys, they emulate them in everything but their main work, which is taking care of cows. The main work of scientists is thinking and making discoveries of what was not thought beforehand. Psychologists often attempt to "play scientist" by avoiding the main work.[32]

Hanson has also made similar statements about the "hard" sciences as well.[33]

The next three chapters of this essay will examine the history of the PLD. Over and above the intrinsic historical interest which these discussions might possess, the primary purpose of including them here is twofold: (a) to help set off the PLD as a distinct methodological problem with characteristics of its own; and (b) to see what insights might be gleaned from these thinkers for the purpose of bringing an adequate reconstruction of discovery closer to a reality. Hopefully, the present formulation of the problem will serve as guide in showing what to look for.

FOOTNOTES

1 *Logic of Scientific Discovery*, (New York, 1968), p. 31.

2 *Ibid.*, p. 32.

3 *Philosophy of Social Science*, (New Jersey, 1966), p. 6.

4 *Scientific Explanation*, (Cambridge, 1968), pp. 21-22.

5 *Op. cit.*, p. 6

6 "Studies in the Logic of Confirmation", *Aspects of Scientific Explanation*, (New York, 1965), p. 5.

7 "Anatomy of Discovery", *Journal of Philosophy*, LXIV (June 8, 1967), pp. 321-352.

8 It is worth pointing out here, though the point will be developed in later chapters, that it might well be the case that certain general types of discoveries follow a logic, while others might not; thus there would remain ample problems of interest in the context of discovery for both the empirical sciences and formal sciences to investigate.

9 *The Conduct of Inquiry*, (San Francisco, 1964) p. 14.

10 The concept of a 'rational reconstruction', or *rationale Nachkonstrucktion*, was used by Carnap in *Der logische Aufbau der Welt* (Berlin and Leipzig, 1928); and it was developed further by Reichenbach in *Experience and Prediction* (Chicago, 1938). Popper also gives a brief analysis of it in *The Poverty of Historicism*, (London, 1957), p. 141.

11 *The Poverty of Historicism*, ibid., p. 141.

12 Henceforth, I shall simply use the letters "PLD"to replace the more cumbersome phrase "problem of the logic of discovery".

13 "The Idea of a Logic of Discovery", *Dialogue*, IV (1965-66), p. 50. "Is There a Logic of Scientific Discovery?", *Current Issues in the Philosophy of Science*, (eds.) Feigl and Maxwell (New York, 1961), p. 31.

14 The hypothetico-deductive (or H-D) account of scientific method is obviously broader than "positivism" *per se*; these remarks are directed at any theory which holds that the H-D model is an adequate description of the *total* methodological picture.

15 "Is There a Logic of Scientific Discovery?", *ibid.*, p. 31. Also in "Notes Toward a Logic of Discovery", *Perspectives on Peirce*, (ed.) R. J. Bernstein (Yale Press, 1965), pp. 52-54.

16 By the term 'method' here, I mean nothing more than a straightforward dictionary definition of that term. For example, Webster's says: "Method: (1) An orderly procedure or process; regular way or manner of doing anything ...; (2) Orderly arrangement, ...; more generally, orderliness and regularity or habitual practice of them in action." Later, I shall define 'logic' in terms of formal rules, and 'method' can be understood in terms of informal rules or procedures (i.e., the absence of formal rules) as Webster's suggests.

17 Hilary Putnam once commented that he did not understand all the peculiarly negative remarks about the logic of discovery, since nobody has ever successfully demonstrated that there is a logic of confirmation either. "Illinois Symposium on the Philosophy of Science", University of Illinois, 1968.

18 A working definition of the term 'logic' will be suggested in the next section.

19 For an excellent linguistic discussion of the differences in 'discovery' referred to here, see G. L. Farre's paper, "On the Linguistic Foundations of the Problem of Scientific Inference", Journal of Philosophy, LXV (1968), 779-794. See also N. R. Hanson's last paper "The Anatomy of Discovery", Journal of Philosophy, LXIV (1967), 321-352.

20 Bunge, M., Metascientific Oueries. (Springfield, Ill., 1959), pp. 66-69.

21 Marx Wartofsky has said: "No term in science suffers a greater ambiguity than does hypothesis. One could make up a list of contradictory statements about hypotheses and their status and use in scientific discussion which would make the scientific community look like something on the other side of Alice's looking-glass." Conceptual Foundations of Scientific Thought (New York, 1968), p. 183.

22 Ibid., p. 184.

23 The Philosophy of Mathematics (New York), p. 115.

24 With respect to scientific method, in particular, all of these logical systems have been employed for the context of validation (e.g., the work of Carnap, Hempel, et al). None of these logics, however, have been employed for the context of discovery, nor were they designed for that purpose.

25 This meaning of 'inference rule' concurs with most standard discussions on the topic; see for example: C. S. Peirce, Collected Papers, eds., C. Hartshorne, P. Weiss (Harvard, 1931-35), 2.26, 6.497; see also R. D. Carmichael Logic of Discovery (Chicago, 1930), p. 3.

26 This notion of a 'formation rule' is very clearly laid out in Wesley C. Salmon's The Foundations of Scientific Inference (Pittsburg, 1967), p. 28.

27 Ibid., pp. 112-113.

28 Logic of Discovery (Chicago, 1930), p. 9-10.

29 These are Popper's phrases, see The Logic of Scientific Discovery, op. cit., p. 32.

30 Scientific Explanation (Cambridge, 1968), p. 368.

31 "An Anatomy of Discovery", Journal of Philosophy, op. cit., p. 322.

32 On Method: Toward a Reconstruction of Psychological Investigation, (San Francisco, 168), p. 44.

33 See chapter IV of this essay.

CHAPTER II

SOME NINETEENTH CENTURY VIEWS ON THE PROBLEM

> Deductivism in mathematical literature and inductivism in scientific papers are simply the postures we choose to be seen in when the curtain goes up and the public sees us. The theatrical illusion is shattered if we ask what goes on behind the scenes. In real life discovery and justification are almost always different processes, and a sound methodology must make it clear that they are so.
>
> P. B. Medawar.[1]

1. Whewell and Mill

Although Reichenbach introduced the terms "context of discovery" and "context of justification" into modern discussions of scientific methodology, the distinctions to which those terms refer can be found in a very clear form in the work of Charles Sanders Peirce[2] (1839-1914). Perhaps owing largely to the work of Peirce many contemporary philosophers of science have become aware of the methodological contributions made by William Whewell (1794-1866). Peirce, who knew Whewell's work intimately, cites Whewell as making more substantial contributions to the philosophy of science than John Stuart Mill.[3] And, as one might suspect, the distinction between hypothesis formation (i.e., the context of discovery) and "justification" can already be seen in the work of Whewell, albeit not so clearly as with Peirce. Thus we have, as it were, a direct historical link between contemporary literature which employs the "discovery-justification" distinction and the work of Whewell. No doubt, if one looks further back he would find still other philosophers who held this distinction in some form, but the historical link to the present day distinction would be anything but direct.

Whewell's position on the role of hypothesis formation in scientific discovery can be seen perhaps most clearly in his famous controversy with John Stuart Mill over the nature of induction in science. During the course of this controversy several important points are made by both philosophers which help to illuminate some modern positions on the problem of the logic of discovery. But even more instructive than the positive insights contributed by this controversy, are the several difficulties and tensions which develop in each of their respective positions. Some of these difficulties are, interestingly, carried into modern discussion on the PLD as well.

Whewell published his views first in two major books, the History of the Inductive Sciences (1837), and The Philosophy of the Inductive Sciences, Founded Upon Their History (1840). For Mill, these two books were to serve as a foil against which he would direct his attack for the purpose of explicating his own views on the nature of induction in A System of Logic (1843).[4] The books by both authors went through several later editions, and the controversy continued here in the form of published replies and subsequent rebuttals. C. J. Ducasse observed that "The great popularity quickly attained by Mill's System of Logic ... stood in the way of a general recognition of the merits of Whewell's theory of the nature of scientific knowledge and the process of discovery."[5]

In general terms, Whewell's views were a rather strange admixture of Baconian inductivism and Kantian a priorism, a mixture which was at odds with the traditional British empiricism of his contemporaries - particularly Mill. Although the many disagreements between Whewell and Mill can be described as a clash between rationalism (of the Kantian sort) and traditional British empiricism,[6] of particular interest here is how this clash manifested itself in the form of a disagreement over the nature of induction and the role of hypotheses in scientific discovery. Both Whewell and Mill thought that their respective views of induction described the process of scientific discovery. Whewell characterized his own work as being a "philosophy of discovery", and his rather unorthodox conception of induction as "Discoverer's Induction". Mill, on the other hand, tended to minimize and belittle the importance of hypotheses (Whewell's Kantianism) and maintained that his view of induction described "the operation of _discovering_ and _proving_ general propositions"[7] in science.

Before proceeding directly to the disagreement between Whewell and Mill, and the views expressed there, it is important to understand some very basic ideological differences which separated Whewell and Mill. To begin with, Whewell's interest in scientific method was not simply an epistemological interest, but it was also a practical interest; practical in the sense of wanting to provide certain maxims or rules which might guide the working scientist in his search for new truths. This practical side of Whewell's interest in methodology is a reflection, I think, of the influence which Francis Bacon had on his work. Whewell, like Bacon, called his tables "Inductive tables". Despite Whewell's rather lengthy critique of Bacon's view of induction, the Baconian influence on Whewell's view of what a sound methodology ought to accomplish should not be under-estimated. Whewell's work in scientific methodology continued to share Bacon's concern with the procedural problems encountered by active scientists in their search for new truths. Both Whewell and Bacon viewed science as an ongoing _activity_, and not simply the collected results or product of finished research. An "organon", after all, as was the _Novum Organon_, is not knowledge, but it is a set of rules which one must follow if one wants to do something; and in this case the something is to advance science. As one commentator on the Novum Organon aptly points out: "Bacon seeks for rules which are the invention of invention".[8] In short, Bacon's interest in scientific method was just as much concerned with what we now refer to as the "context of discovery" as it was with other aspects of scientific endeavour (e.g., proof). This same concern about problems which arise in the context of discovery can be seen throughout Whewell's work as well. It would appear that this Baconian strain in Whewell's work had been lost sight of, or even forgotten, by many later commentators who choose to concentrate on the more obvious, and perhaps more interesting, Kantian elements in Whewell's thought.

It is significant that one of Whewell's later volumes was entitled Novum Organon Renovatum.[9] Like the Organon of Bacon, it too sought general rules for successful scientific investigation. Perhaps it was this mutual interest in finding general procedural rules that could explain Laurens Laudan's puzzlement when he observed that "Whewell was unable to break completely with the view that Bacon was an important methodologist".[10]

In the introduction to the Novum Organon Renovatum Whewell tells us, in fairly general terms, what his purpose is in studying the methods of science:

> My object at present is not to relate the history, but to present the really valuable results of preceding labours: and I shall endeavour to collect, both from them and from my own researches and reflections, such views and such rules as seem best adapted to assist us in the discovery and recognition of scientific truth; or, at least, such as may enable us to understand the process by which this truth is obtained. I would present to the reader the Philosophy and, if possible, the Art of Discovery.[11]
>
> (my underlining)

In Whewell's particular discussion of Newton's Rules of Philosophizing this dual interest in the context of discovery and the context of justification again becomes visible. Whewell criticized Newton's Rules, not on the grounds that the inference (Whewell called it an "inductive" inference) to universal gravitation had no rational license, or was not valid, but on the grounds that the rules (taken together) which Newton used, were not general enough to underwrite other inductive conclusions. But why, one might ask, should Whewell be disappointed in a set of rules (considered to be valid) which did not underwrite <u>other</u> inductive inferences unless he, Whewell, was concerned to find a set of rules which would aid the scientist in different, future, discoveries? Or, to put this query in another way, Whewell recognized (or considered) the conjunction of Newton's Rules to be valid in the case of inferring universal gravitation, yet he was still critical of, and disappointed by these rules. Whewell was aware of the rather obvious point that any set of rules (let alone Newton's) which are designed to license <u>just one</u>, and only one inference, can hardly be regarded <u>as rules</u> (qua rules) at all; and Whewell did not choose to criticize the rules in this way. What is being suggested here is that one plausible interpretation of Whewell's disappointment in Newton's Rules is that Whewell did not see them as aiding in any way the general problems encountered in the context of discovery.[12]

As will be shown presently, Whewell had an unorthodox understanding of the term 'induction', and as R. E. Butts points out: "For Whewell, the logic of induction is the logic of discovery, as much as the logic of proof".[13] In fact, as Butts also suggests, one cannot make sense of Whewell's claim that the inductive tables display the logic of induction unless the term 'induction' be understood in Whewell's peculiar dual-purpose manner. Whewell's view of induction, albeit an unorthodox one, tends to support, rather than detract from the general point being made here, viz., that Whewell was not prepared to accept as adequate any set of methodological rules (be they Newton's or Mill's) which did not attend to the context of discovery as well as the context of proof. And it has also been suggested here that Whewell's tacit criteria of adequacy for methodological rules find their origin in one of the overarching themes in Bacon's Novum Organon.

There are several good reasons for calling attention to this practical side of Whewell's methodological thought, not the least of which is the fact that it is often overlooked in favour of Whewell's more obvious Kantianism. Whewell freely admitted that he was greatly influenced by Kant. But more pertinent to the present discussion, however, is the fact that Whewell's practical interest in scientific methodology (in particular, the context of discovery) is what lies behind his basic disagreement with Mill's view of induction and its role in scientific discovery. Should this difference be lost sight of, the debate between Whewell and Mill could all too easily collapse into a mere verbal dispute about the meaning of the word 'induction'. In reality, the dispute about induction is the focal point of a clash between two different philosophies of scientific method.

Mill, as is well known by now, was much more concerned with the formal aspects of science than he was with its practical problems; in particular, Mill wanted to clarify the notion of "proof". In contemporary parlance, one would say that he was primarily interested in "the context of justification". Mill's System of Logic was intended, among other things, to accomplish at least two basic tasks: to clear up much of what he considered to be psychologistic confusions in scientific method, and to provide a proper analysis of the "reasoning" which goes to make up the "proof" of any given proposition. In the Preface to the first edition Mill states his purpose thus:

> To cement together the detached fragments of a subject never yet treated as a whole; to harmonize the true portions of discordant theories, by supplying the links of thought necessary to correct them from the errors with which they are always more or less interwoven; ... [14]

In the introduction to the same work, he emphasizes his intention to clarify the formal aspects of reasoning in particular:

> Our object, then, will be to attempt a correct analysis of the intellectual process called Reasoning or Inference, and of such other mental operations as are intended to facilitate this: as well as, on the foundation of this analysis, and pari passu with it, to bring together or frame a set of rules or canons for testing the sufficiency of any given evidence to prove any given proposition.[15]

In the body of the text, Mill goes on to criticize Whewell most strongly on the question of proof. He says, for example:

> Dr. Whewell's theory of the logic of science would be very perfect if it did not pass over altogether the question of Proof.[16]

As one might suspect, Mill's charge here is considerably over-stated; Whewell's notion of the "Consilience of inductions" is a requirement that a hypothesis must be able to explain more than just the initial puzzling data, and Whewell introduced this requirement specifically for the purpose of testing hypotheses (i.e., for purposes of "proof").

There are in fact several instances where both Whewell and Mill unjustly criticize one another; moreover, there are also times when the one is insensitive to legitimate criticisms by the other. All of these oversights and disagreements become more understandable however, if viewed from the perspective of a basic clash between the final desideratum (or "ideology") of each philosopher. It is reasonable to expect a certain amount of insensitivity and disagreement between any two disputants whose ultimate purposes do not correspond. In sum, Whewell considered the proper analysis of scientific method to include both the antecedent circumstances (i.e., the context of discovery) and an appropriate account of proof; whereas Mill's attention was directed almost exclusively toward the analysis of scientific proof, or justification.[17] Whatever problems Whewell found in the context of discovery, Mill regarded as either unworthy of serious (separate) investigation or simply resolvable by employing his "canons" of induction properly.

2. The Methodological Debate

While perhaps it is true to say that Whewell and Mill differed on what it was they wanted to accomplish in their respective analyses of scientific method, it would be an over-simplification to suggest that that was the sum of the matter. A more complete picture of the Whewell-Mill controversy would point out that Whewell and Mill were defenders of entirely different epistemologies: they had differing views regarding the ingredients of knowledge, and how we come to have it. Their respective analyses of scientific method can, in fact, be regarded as simple extensions (or manifestations) of their more general epistemologies. Their views on scientific method are just one part of a connected and coherent system of thought. However, an exhaustive examination of these two systems would be far beyond the scope of this essay. The difficulty of the present task, therefore, as it is with any partial analysis, is to examine the views of Whewell and Mill on the problem of the logic of discovery (in particular) without distorting or giving false impressions about the remainder of their systems. Actually, the controversy between Whewell and Mill over the nature of scientific "Induction", is really a debate about the"logic of discovery" under another name. Whewell considered "Induction" to consist in much more than the straight-forward procedure which Mill propounded in his Methods. Whewell considered the word "Induction" to be a general term which referred to the total process of scientific investigation.

It is particularly instructive to view this debate against the background of two of the distinctions made in chapter I of this essay, viz.: (1) Context of discovery versus context of justification, and (2) having a method versus having a logic. It becomes progressively more clear that Whewell was quite sensitive to the first distinction (i.e., (1)), but not sensitive enough to the second; whereas Mill, on the other hand, was aware of the second distinction, but quite oblivious to the first. Each philosopher was correctly aware of the other's deficiencies, but neither was sensitive enough to his own.

Perhaps the key to understanding Whewell's general view of science, and how it progresses, is to understand his Inductive Tables. For Whewell, an inductive table is basically a schematic diagram of the relationships between the established propositions of a well developed science. The propositions are arranged in a hierarchical pyramid with particular (low level) observation statements at the bottom, and progressively more general statements ascending toward the top. Thus, the highest order law-like statements appear at the top of the table, subsuming all of the less general statements which serve as support for the facts than it was introduced to account for (i.e., his "Consilience of inductions"), this still fails to answer the formal objection raised by DeMorgan. For DeMorgan, and indeed most other logicians of the nineteenth century, the essence of induction was simply the accurate generalization of particulars; but in Whewell's view the essence of induction "consists in the suggestion of a conception not before apparent".[23] The difficult step in an induction, according to Whewell, is not so much checking the accuracy of a generalization once made, but in "colligating" all the known facts under a new conception such that a generalization is then possible. The process of moving up the pyramid of the inductive tables is far more important and fundamental in Whewell's view of induction. Thus, it can be seen that Whewell's concern with the discovery of "new conceptions" (or "colligating ideas"), which reflects his practical interest in methodology, is built right into his view of induction. I would suggest that it is this view which helps clarify Whewell's interesting, but rather impervious, response to DeMorgan's criticism:

> After having invited your criticism of my Novum Organon, I ought not to omit to thank you for it. I have been as I expected that I should be, instructed and interested by it. Nor can I deny that it is in a great manner just. My object was to analyze, as far as I could, the method by which scientific discoveries have really been made; and I call this method Induction, because all the world seemed to have agreed to call it so, and because the name is not a bad name after all. That it is not exactly the Induction of Aristotle, I know; nor is it that described by Bacon, though he hit very cleverly on some of its characters, erring much as to others. I am disposed to call it Discoverers' Induction; but I dare not venture on such a novelty, except in the indirect way in which I have done. With such a phraseology I think my formulae are pretty near the mark, and my Inductive Tables a good invention. But I do not wonder at your denying these devices a place in Logic; and you will think me heretical and profane, if I say, so much the worse for Logic.[24]

While this response clearly shows that Whewell was not nearly sensitive enough to DeMorgan's formal objections to his view of induction in the Novum Organon, it also shows that Whewell considered DeMorgan (and later Mill) to have missed the importance of his most fundamental point, namely: induction requires the introduction of a "Colligating" idea or "conception" prior to the testing of the generalization. For Whewell, the confirmation of an induction is but half of the process. M. R. Stoll, in her lengthy study of Whewell's view of induction, makes this point by observing that:

> Whewell's criticisms of the logics of Aristotle and Mill rest mainly on his conviction that these two men were so concerned to discover a method of proof for inductions that they misrepresented the process of induction itself. Whewell insisted that the important thing is, not to present the inductive conclusions when found, but to consider how they are discovered. In proposing their formal canons, both Aristotle and Mill assumed that all the elements of the discovery are ready at hand. Whewell maintained that in so far as they made this assumption, Aristotle and Mill forgot that the essence of induction lies in the discovery of the elements which are presented in the proof.[25]

A serious question could be raised here as to just who it was that really "misrepresented the process of induction itself". DeMorgan and Mill felt that Whewell was stretching the meaning of "induction" to include more than was proper to the process. On the other hand, Whewell felt that they (and others) were not perceptive enough to see all that was really involved in the process of induction. Perhaps the fairest assessment of this part of the dispute would be to point out that neither of these views of induction represents the complete process with a fair distribution of emphasis. If it is true that Whewell's view of induction is wanting in formal rigour in the confirmatory stage of induction, it is also true that most of Whewell's contemporaries were not receptive enough to the preconditions and ingredients of induction which he pointed out. Again, Whewell's methodological interest in the process of discovery over-rides his interests and perceptions of _induction_, as such.

In fairness to DeMorgan however, it ought to be pointed out that he did recognize Whewell's positive contribution to the analysis of scientific discovery, but DeMorgan declined to recognize this as a contribution to induction _per se_. In commenting on Whewell's example of Kepler's elliptical hypothesis, DeMorgan writes:

> Again, Dr. Whewell, very properly pointing out that the elliptic induction, upon planet after planet, is a very trivial matter compared with the discovery that the _ellipse_ of all possible curves, is the one which one planet actually takes, -- introduces the determination of the ellipse as a part of the induction, and as the most important part. We admit that it was the most important part of the _discovery_. But we demur to this part of the discovery being called induction.[26]

Among other things, DeMorgan implies here that Whewell has simply confused the processes of discovery and induction as being, in effect, one and the same process. In fact, Whewell's use of these terms _is_ often ambiguous, particularly when one considers what the Inductive Tables are intended to represent; but it appears that Whewell's point about the "Colligation of Facts" as being necessary for induction is still somewhat more subtle than DeMorgan seems to recognize. Leaving aside for a moment any alleged confusions between the processes of _discovery_ and _induction_, Whewell is holding the view that even the standard nineteenth century conceptions of induction (especially those espoused by Mill, and Herschel) require the "Colligation of facts" by "introducing an appropriate conception" as an integral part of induction itself, else there would be no fact or proposition to be proven by inductive generalization. Whewell regarded this as a logical point supporting his view of _induction_ generally and not simply a case of special pleading for his analysis of scientific discovery;[27] in developing this point Whewell says:

> Hence in _every_ inference by Induction, there is some Conception _superinduced_ upon the Facts: and we may henceforth conceive this to be the peculiar import of the term _Induction_. I am not to be understood as asserting that the term was originally or anciently employed with this notion of its meaning; for the peculiar feature just pointed out in Induction has generally been overlooked. This appears by the accounts generally given of Induction.[28]

Both Whewell and Mill considered induction, however that term be construed, to be the process by which all significant scientific discoveries are made; it is natural therefore to find their discussion of induction focussing on specific scientific discoveries from the history of science. (This might also explain the occasional conflation of the terms 'discovery' and 'induction' and the resulting ambiguity.) In their published replies to one another the example which is discussed in the greatest detail is that of Kepler's discovery of the elliptical orbit of Mars. Indeed, this discussion is most illustrative of the fundamental clash between their respective methodological interests, and their views on scientific induction generally. In this exchange, Whewell wanted to make two distinguishable points: (1) that every induction involves the introduction of a conception, supplied by the mind, and superimposed on the data; and (2) that this superimposition of an appropriate conception is the most difficult and important step in scientific discovery. Mill, on the other hand, either denies or attempts to make trivial both of Whewell's points, and goes on to emphasize the point that induction is "that transition from known cases to unknown which constitutes Induction in the original and acknowledged meaning of the term."[29]

In the Philosophy of Inductive Sciences, and again later in the Novum Organon Renovatum, Whewell explicated his view of scientific induction; and this was not only the beginning of the controversy with Mill but also the beginning of two different philosophies of science:

> Induction is familiarly spoken of as the process by which we collect a General Proposition from a number of Particular Cases: and it appears to be frequently imagined that the general proposition results from a mere juxta-position of the cases, or at most, from merely conjoining and extending them. But if we consider the process more closely, as exhibited in the cases lately spoken of, we shall perceive that this is an inadequate account of the matter. The particular facts are not merely brought together, but there is a New Element added to the combination by the very act of thought by which they are combined. There is a Conception of the mind introduced in the general proposition, which did not exist in any of the observed facts . . . The same is the case in all other discoveries. The facts are known, but they are insulated and unconnected, till the discoverer supplies from his own stores a Principle of Connection. The pearls are there, but they will not hang together till some one provides the String. The distances and periods of the planets were all so many separate facts; by Kepler's Third Law they are connected into a single truth: but the Conceptions which this law involves were supplied by Kepler's mind, and without these, the facts were of no avail.[30]

It is Whewell's emphasis here on the necessity to introduce a mental "Conception" which marks the most significant departure from his contemporaries' view of induction, and the point Mill strongly objects to. In effect, what Whewell is doing is to point out that hypotheses (i.e., his "Conceptions") play the most important role in scientific discovery, and that these hypotheses, or "Conceptions", are not simply seen in the data itself. Whewell developed this point further (staying with the same example) by pointing out that:

> We know how long Kepler laboured, how hard he fought, how many devices he tried, before he hit upon this Term, the Elliptical Motion. He rejected, as we know, many other 'second extreme Terms', for example, various combinations of epicyclical constructions, because they did not represent with sufficient accuracy the special facts of observation. When he had established his premise, that 'Mars does describe an Ellipse about the Sun', he does not hesitate to guess at least that, in this respect, he might convert the other premise, and assert that 'All the Planets do what Mars does'. But the main business was, the inventing and verifying the proposition respecting the Ellipse. The Invention of the Conception was the great step in the discovery; the Verification of the Proposition was the great step in the proof of the discovery.[31]

Nowhere in the writing of Whewell is the distinction between, what we now call, the "context of discovery" and the "context of justification" so eminently clear. Whewell's phrase "the Invention of the Conception" (or hypothesis) corresponds to his movement up the Inductive Tables, which is the order of discovery; and the phrase "Verification of the Proposition" corresponds to his downward movement in the Inductive Tables, which is the order (or context) of proof. It is in the context of discovery in particular that the introduction of an appropriate mental Conception is required, according to Whewell, and this phase is just as much a part of induction as is the proof phase. He says that:

> . . . for our purpose, it is requisite to bear in mind the feature we have thus attempted to mark; and to recollect that, in every inference by induction, there is a Conception supplied by the mind and superinduced upon the Facts.[32]

From the point of view of the logic of discovery, it is of particular interest to note that Whewell maintains that the "Conception" (or hypothesis) which is introduced is the product of skillful guessing by a trained and disciplined mind. In several places throughout his work Whewell remarks that the process of hitting upon the right conception is one of informal trial and error: "the mind, in order to be able to supply this element (i.e., the Conception) must have peculiar endowments and discipline."[33] And that:

> . . . in truth, we must acknowledge, before we proceed with this subject, that speaking with strictness, an Art of Discovery is not possible; - that we can give no Rules for the pursuit of truth which shall be universally and peremptorily applicable; -- and that the helps which we can offer to the enquirer in such cases are limited and precarious. Still, we trust it will be found that aids may be pointed out which are neither worthless nor uninstructive.[34]

In effect then, Whewell's position with respect to hypothesis formation contains the following three theses:

(1) That the introduction of a conception (or hypothesis) is a required element in discovery, and that this conception is supplied by the mind of the discoverer.

(2) That there is no "Art of Discovery" per se, but it is possible to formulate, or point out, certain "aids" to this process.

and

(3) The process of hitting upon the right conception is one of skillful trial and error by a trained mind.

Whewell considered these theses to be integral elements in the process of discovery, and, a fortiori, induction itself. Kepler's discovery of the elliptical orbit of Mars, Whewell argued, was just one of many examples which demonstrated these theses.

In a section of A System of Logic entitled "Of Inductions Improperly So Called" Mill attacks Whewell's account of Kepler's discovery from several fronts. It is possible, in fact, to distinguish at least ten different criticisms all of which are intended to serve the dual purpose of explicating Mill's own view of the process of induction while discrediting Whewell's view at the same time. Mill's criticisms fall roughly into two groups, each group directed at one of two separate themes which Mill finds throughout Whewell's account. The first set of objections is directed at Whewell's view of the origin and importance of the "Conception" (or hypothesis) introduced in the inductive process. The second group of objections is directed at Whewell's alleged failure to recognize that the central feature of all induction is generalizing from known cases to unknown cases, and that this feature constitutes the essence of induction "properly so called".

Where Whewell had emphasized that the great step in Kepler's discovery was bringing all the known facts together "under the conception of an ellipse", which was a new point of view never before suggested, Mill begins his attack by arguing that this part of Kepler's discovery was of comparatively trivial significance, and that Kepler merely reported what he saw. Mill suggests that the introduction of the conception of an ellipse was really little more than piecing together what he had seen, like:

> A navigator sailing in the midst of the Ocean discovers land: he cannot at first, or by any one observation, determine whether it is a continent or an island; but he coasts along it, and after a few days finds himself to have sailed completely around it: he then pronounces it an island. Now there was no particular time or place of observation at which he could perceive that this land was entirely surrounded by water; he ascertained the fact by a succession of partial observations, and then selected a general expression which summed up in two or three words the whole of what he so observed. But is there anything of the nature of an induction in this process? Did he infer anything that had not been observed from something else which had? Certainly not. He had observed the whole of what the proposition asserts.[35]

We would do well to disregard for the moment Mill's denial here that this phase of Kepler's discovery involved any induction (this will be considered as part of Mill's second objection), and focus our attention on Mill's assertion that Kepler simply saw the ellipse in the data. It is precisely this thesis of "seeing the ... in the data" which Whewell was trying to deny as a characterization of scientific discovery, and Mill, on the other hand, later asserts as a feature which is presupposed by his "Canons of Induction". The issue here is the origin of the conception which is introduced: Whewell says that it is added to the facts, or "superinduced", by the mind of the discoverer, and Mill argues that the appropriate conception is simply read right off the data of observation. For Mill the origin of Kepler's "Conception" of an ellipse was likewise the result of what he saw; Mill says:

> According to Dr. Whewell, the conception was something added to the facts. He expresses himself as if Kepler had put something into the facts by his mode of conceiving them. But Kepler did no such thing. The ellipse was in the facts before Kepler recognized it; just as the island was an island before it had been sailed around. Kepler did not put what he had conceived into the facts, but saw it in them.[36]

Mill's straightforward empiricist account of the origin of the "Conception" is eminently clear here. For Whewell, such an account is deceptively simple, and indeed, it passes over altogether the essential difficulty in scientific discovery. Thus Whewell replies to Mill's account by saying:

> There is a difference between Mr. Mill and me in our view of the essential elements of this Induction of Kepler, which affects all other cases of Induction, and which is, I think, the most extensive and important

> of differences between us. I must therefore venture to dwell upon it a little in detail.
>
> I conceive that Kepler, in discovering the law of Mars' motion, and in asserting that the planet moved in an ellipse, did this; -- he bound together particular observations of separate phases of Mars by the notion, or, as I have called it, the _conception_, of an _ellipse_, which was supplied by his own _mind_. Other persons, and he too, before he made this discovery, had presented to their minds the facts of such separate successive positions of the planet; but could not bind them together rightly, because they did not apply to them this conception of an _ellipse_. To supply this conception, required a special preparation, and a special activity in the mind of the discoverer. He, and others before him, tried other ways of connecting the special facts, none of which fully succeeded. To discover such a connection, the mind must be conversant with certain relations of space, and with certain kinds of figures. To discover the right figure was a matter requiring research, invention, resource. To hit upon the right conception is a difficult step; and when this step is once made the facts assume a different aspect from what they had before: that done, they are seen in a new point of view; and the catching this point of view, is a special mental operation, requiring special endowments and habits of thought. Before this, the facts are seen as detached, separate, lawless; afterwards, they are seen as connected, simple, regular; as parts of one general fact, and thereby possessing innumerable new relations before unseen. Kepler, then, I say, bound together the facts by superinducing upon them the _conception_ of an _ellipse_; and this was an essential element of his Induction.[37]

Thus Whewell continues to defend his view that the right "conception" is not simply _seen in the data_ of observation, as Mill argues.

In several places Whewell had painstakingly pointed out the historical fact that no one before Kepler's discovery, including Kepler himself, had been able to put the observed facts together in the correct pattern, and to do so required the skillful guesswork of a disciplined mind. Thus Mill found himself in the position of having to account for this historical fact which Whewell had emphasized time and again. And on this point, we find Mill in curious agreement with Whewell:

> Having stated this fundamental difference between my opinion and that of Dr. Whewell, I must add, that his account of the manner in which a conception is selected suitable to express the facts appears to me perfectly just. The experience of all thinkers will, I believe, testify that the process is tentative; that it consists of a succession of guesses; many being rejected, until one at last occurs fit to be chosen. We know from Kepler himself that before hitting upon the "conception" of an ellipse, he tried nineteen other imaginary paths, which, finding them inconsistent with the observations, he was obliged to reject. But, as Dr. Whewell truly says, the successful hypothesis, though a guess, ought generally to be called, not a lucky, but a skillful guess. The guesses which serve to give mental unity and wholeness to a chaos of scattered particulars are accidents which rarely occur to any minds but those abounding in knowledge and disciplined in intellectual combinations.[38]

Mill's agreement here with Whewell's view about the need for "skillful guesses" is curious indeed, and not nearly so insignificant as Mill may have considered it to be. To agree, as Mill does, that the formation of a hypothesis requires "skillful guessing" is, if not contradictory, certainly at odds with his view that it is simply "seen in the data". There is a significant difference between simply "seeing" a pattern and "guessing" at it. On the one hand, Mill says that Kepler simply _saw_ the _ellipse_; that the "conception" was simply "the sum of the different observations", and on the other hand he holds that the conception of an ellipse required "skillful guessing" on the part of Kepler. It does not appear that Mill can have it both ways, at least not without considerably more argument than he offers. Indeed the tension which can be seen in Mill's position here, is precisely the point at which the modern hypothetico-deductive account of scientific method departs from the classical inductivists account (e.g., that espoused by Mill).

Hypothetico-deductive theorists hold the general view that the framing of a hypothesis is an independent problem from its subsequent confirmation; whereas the classical inductivist accounts (e.g., Bacon and Mill) recognize no independent problem here and proceed as though the appropriate conceptions just stand out in the data. Whewell was attempting to call attention to the necessity of framing hypotheses which are not, strictly speaking, part of the observation data itself; Mill, on the other hand, was ambiguous about this point, and in any case considered it trivial.

Although Whewell did not choose to criticize Mill's position _via_ the tension pointed out here, he continued to maintain his view that the appropriate conception is not simply _read right off the data_. In fact, Whewell was perhaps the first to recognize that "Mill's Methods" were seriously deficient on this point; and he argued that the "Methods" took for granted (or presupposed) the very thing which he was attempting to characterize as the most difficult step in scientific discovery. Whewell argued that Mill's methods require that the observed phenomena be reduced to manageable _formulae_ which contain all the relevant predicates such that the _Methods_ can then determine which predicate is operating as a causal factor; but the crucial problem, of course, is how _to know which predicates to consider as relevant_. Sir John Herschel criticized Mill's _Methods_ in the same way, saying that "the difficulty in physics is to find such predicates, not to perceive their force when found."[39] In more recent times this deficiency in Mill's Methods, as methods of discovery, has become more widely recognized.[40]

With the advantage of hindsight in our favour, it is clear now that Whewell was making a very acute observation with respect to the role hypotheses (or "conceptions") play in scientific discovery; and he saw that there was a serious methodological problem in arriving at these hypotheses. Unfortunately, Mill's criticisms of Whewell's general view of induction, as such, were regarded as so devastating by Mill's contemporaries that Whewell's positive insights on methodology were swept aside in the aftermath as well. Whewell's methodological views were left aside until Charles S. Peirce, in the United States, gave them the serious attention which they deserve.

The second main criticism which Mill brought against Whewell was directed at Whewell's unorthodox conception of induction; and the Kepler example was as good as any for Mill to make his point. Mill contended, correctly I think,that up until the point where Kepler discovered that an elliptical orbit would satisfy the available data, no induction properly so called had been performed by Kepler. Mill stated that:

> The only real induction concerned in the case, consisted in inferring that because the observed places of Mars were correctly represented by points in an imaginary ellipse, therefore Mars would continue to revolve in that same ellipse; and in concluding ... that the position of the planet during the time which intervened between two observations, must have coincided with the intermediate points of the curve.[41]

Mill continues his line of criticism here by developing a point originally made by DeMorgan:

> Nearly all the definitions of induction, by writers of authority, make it consist in drawing inferences from known cases to unknown; affirming of a class a predicate which has been found true of some cases belonging to the class; concluding, because some things have a certain property, that other things which resemble them have the same property ... It can scarcely be contended that Kepler's operation was an Induction in this sense of the term ... There was not that transition from known cases to unknown which constitutes Induction in the original and acknowledged meaning of that term.[42]

It must be admitted that although Whewell's general theory of induction does take into account the need for proof of a discovery once made[43] (i.e., moving down the hierarchical pyramid for what Whewell calls "The Consilience of Induction"), his discussion of Kepler's discovery disregards this aspect of induction altogether: indeed, Whewell implies that at the point where Kepler introduced the new conception, an <u>induction</u> had been performed. It was this latter implication which prompted Mill to conclude:

> Dr. Whewell calls nothing Induction where there is not a new mental conception introduced, and everything induction where there is. But this is to confound two very different things, Invention and Proof. The introduction of a new conception belongs to Invention: and invention may be required in any operation, but is the essence of none.[44]

Thus Mill's objection to Whewell's view of Kepler's <u>induction</u>, <u>qua</u> induction, is in order. Mill was also correct in calling attention to Whewell's cursory account of the formal problems in proof procedures. Whewell's attention was focussed on the problems of forming plausible hypotheses (or conceptions); whereas Mill's attention had been focussed on the problems of proof and confirmation of those hypotheses once found. Each had something valuable to contribute to the other's position and to the analysis of scientific method generally, but each of them appears to have been too preoccupied with his own ideological interests to appreciate the significance of the other's criticisms. While these ideological differences (i.e., those outlined in the first section of this chapter) resulted in a certain insensitivity to valid criticisms, they also made possible some genuine advances in the analysis of scientific method. Marion Rush Stoll aptly summarizes the result of the controversy:

> Whewell attempted to analyze the process of discovery; Mill was concerned with the problems of proof. Each had something to contribute to inductive logic. Unfortunately, however, Whewell's contribution was almost forgotten in the enthusiasm with which Mill's views were received. Even at Cambridge, Mill's influence was much greater than that of any indigenous thinker. The fact is, of course, that Mill's _System of Logic_ was very widely read, and that its clarity and specious simplicity made such books as Whewell's appear laboured and unfruitful. The limitations of Mill's _Four Methods_ were not generally recognized, and Whewell's criticisms of their inadequacies were not appreciated by his contemporaries.[45]

One could go still further and point out that Whewell's criticisms of Mill rested on, among other things, distinguishing between the "context of discovery" and the "context of justification" -- a distinction which is regarded as fundamental in modern theories of scientific method.

Before moving on however, it is interesting to look, for a moment, at the views of Whewell and Mill in isolation from the controversy with one another. While Whewell's view contains a clear cut distinction between the process of _discovery_ and the process of _proof_, he tended to confuse and muddle this distinction by calling the conjunction of these processes by one name: _induction_. This turned out to be a rather unfortunate choice of words since his contemporaries already had an established opinion about the essential ingredients of induction. This choice of words may also have been one reason why Whewell's genuine contributions to the analysis of scientific method were left unheralded.

From the point of view of the problem of the logic of discovery however, Whewell's position contains an interesting tension which is even more instructive than his distinguishing between discovery and proof. In several places throughout Whewell's work we find him stressing the point that scientific discovery is generally "no accident", and that it requires the "skillful guess-work" of a "trained and disciplined researcher"; yet, Whewell holds, that "an Art of Discovery is not possible".[46] It would appear that in Whewell's effort to show that the important "new Conceptions" in science are not simply seen in the data (i.e., his effort to distinguish between discovery and justification) he slightly overstated his case. Whewell was attempting to call attention to the fact that the process of discovering a new conception was methodologically different from the process of proving it, yet in doing this Whewell says things about the former process which he never actually argues for. Whewell nowhere presents an argument for his view that "an Art of Discovery is not possible". Indeed, it is curious that a man who recognizes that scientific discovery requires "training", "discipline", and "skill", should also say that "an Art of discovery is not possible". For, on the contrary, it would appear that if the guesses (to hypotheses) are "not accidental", and "not lucky guesses", but instead require "training" and "discipline", then it would seem that an Art of Discovery is not only possible, but indeed plausible. This is not to suggest that there is any contradiction in Whewell's position here, but it does point to an interesting tension which exists in a methodology which says that scientific discoveries are non-accidental (i.e., they are "skillful"), yet there is no Art, or logic, to scientific discovery. There is something unhappy, or at least incomplete, about a view which holds that a certain process requires skill and training, and at the same time leaves the impression that it is a more or less serendipidous process. If the discovery of scientific hypotheses is not accidental, and not luck, then it would seem that an Art, or logic, to it might at least be possible.[47] Thus Whewell, who holds that discovery is non-accidental, appears to have overstated his case when he stated that "an Art of Discovery is not possible". And this overstatement resulted in the tension described here. Indeed this same sort of ambiguity, or tension, can be seen in many modern accounts of the problem as well; and attention will be called to it again in due course.

From the point of view of the logic of discovery, Whewell's overstatement here is particularly unfortunate because it is just such an "Art", if you will, that the Discovery problem is concerned to learn more about. As it is, Whewell tells us that hypotheses are required, that these hypotheses are not part of the observational data, and that the formation of them requires 'skill' and 'training'; but Whewell left the problem here in its unfinished state.

Mill's position on the analysis of scientific method also contains some internal difficulties which were not brought out sufficiently in the controversy with Whewell. Mill, it will be recalled, considered his canons of induction to be both the method of discovery and the method of proof in science. He was able to hold this view because, for him, both the generative act (i.e., the act of forming the initial idea or hypothesis) and the act of proof, were founded on the straightforward empiricist principle of simply looking and seeing. Perhaps the main difficulty with such an account, at least from the methodological point of view, is that it cannot account for new ideas which may involve theoretical terms. By definition theoretical terms refer to entities which cannot be seen (at least at the time of their inception), and such terms have always played a central role in scientific theories. Newton's gravitational force could not be _seen_ in the required sense of 'seeing', nor could atoms be _seen_ prior to the introduction of electron microscopes. In fact, many scientific theories contain propositions and hypotheses which contain references to entities which cannot be seen. Indeed, C. S. Peirce went so far as to say that hypothetical reasoning "is the only kind of argument which starts a new idea".[48]

Since the time of Mill, and of Peirce, there have been many modern empiricists (e.g., C. J. Ducasse, M. Cohen, and G. Ryle) who have made the point that no amount of looking, or even _repeated looking_, will in itself bring you to knowledge of causal laws.[49] The use of hypotheses which contain references to entities or relations which are not _known_ nor _seen_ is now recognized as an integral part of scientific theorizing and practice. This later point, in fact, is what Whewell was trying to persuade his contemporaries to appreciate. However, there is perhaps no clearer way to understand the seriousness of the deficiency in Mill's type of inductivism than to see how it effects working scientists in the field. Peter B. Medawar, for example, a well known Microbiologist and Nobel Prize winner for Medicine, 1960, states that:

> We scientists often miss things that are "staring us in the face" because they do not enter into our conception of what might be true, or, alternatively, because of a mistaken belief that they could not be true. In our earlier work on immunological tolerance (_Philos. Trans. Roy. Soc._, B, 239 (1956): pp. 357-414), my colleagues R. E. Billingham and L. Brent and I completely missed the significance of observations which, rightly construed, would have led us to recognize an altogether new variant of the immunological response (the "graft against host" reaction) which now plays a very important part in the theory of tissue transplantation. The "facts" were before us, and if induction really worked we should not have been obliged to wait several years for their elucidation, which was hit upon independently by M. Simonsen and by Billingham and Brent themselves (see Billingham's Harvey Lecture on

<u>The biology of graft-versus-host reactions.</u> Harvey Lectures, Series 62, New York, 1968).[50]

Thus Mill's methods not only fail in helping us to frame hypotheses (indeed, the use of hypotheses is trivialized by Mill), but the methods also provide no guarantee that significant facts and relations will not go overlooked. Despite the deficiencies of Mill's methods however, they do play a limited role in helping to frame hypotheses in the descriptive sciences where taxonomic classification is the primary objective. But too much emphasis should not be placed on this limited usefulness, because, as was pointed out above, and as Einstein states here:

> There is no inductive method which could lead to the fundamental concepts of physics ... in error are those theorists who believe that theory comes inductively from experience.[51]

It is both odd and unfortunate that an influential methodologist such as Mill, who claimed to have a method for scientific discovery, should actually have had so little to contribute to the problem directly; whereas Whewell, on the other hand, who denied that there were any rules for scientific discovery, identified the true source of the problem, and provided us with a modest beginning for its solution. We have learned, however, that a simple "look and see" procedure, such as Mill's, will not do; and from Whewell we have learned that it is necessary to "introduce a Conception", and that the appropriate conceptions are engendered by "training" and "skill" in the respective discipline. Anything resembling a standard procedure, or logic, to this process is still some way off however.

FOOTNOTES

1 P.B. Medawar, Induction and Intuition in Scientific Thought (London, 1969), p. 26.

2 Collected Papers of Charles Sanders Peirce, C. Hartshorne and P. Weiss, eds., (Harvard, 1931-1935), see in particular ms. C. 1901 on "Abduction".

3 William Whewell's Theory of Scientific Method, R. E. Butts, ed., (Pittsburgh, 1968), P. vii. All further citations from Whewell in my text are from this volume.

4 J. S. Mill, A System of Logic (London, 1965), p. v.

5 "Whewell's Philosophy of Scientific Discovery", The Philosophical Review, LX (Jan. 1951), p. 56.

6 Ibid., p. 56.

7 Mill, op. cit., p. 186.

8 Science, Folklore, and Philosophy, H. Girvetz, G. Geiger, et al, eds., (New York, 1966), p. 177.

9 This work was one of three volumes which comprised the third edition of his Philosophy of the Inductive Sciences; they appeared from 1858-1860.

10 "Theories of Scientific Method from Plato to Mach", History of Science, VII (1968), p. 163.

11 Whewell, op. cit., pp. 191-192.

12 In a paper entitled "Whewell on Newton's Rules of Philosophizing" from The Methodological Heritage of Newton, R. E. Butts points out that there is ample room to interpret Newton's Rules as "discovery rules", as opposed to "inference rules", but Whewell, unfortunately, did not see them from this point of view -- else he would have been more charitable.

13 Whewell, op. cit., p. 20.

14 Mill, op. cit., p. iii

15 Mill, ibid, p. 7.

16 Mill, ibid., p. 199.

17 E. W. Strong develops the point about Whewell and Mill often speaking past each other in his paper "Whewell and Mill: Their Controversy About Scientific Knowledge", Journal of the History of Philosophy (1955), p. 210. See also R. E. Butts, op. cit., p. 20.

18 Whewell, op. cit., p. 165.

19 For a more exhaustive development of this point, see R. E. Butts, op. cit., p. 17.

20 Whewell, ibid., p. 165.

21 For an excellent summary of the numerous criticisms brought against Whewell's view of induction see R. E. Butts, ibid., pp. 24-29.

22 Augustus DeMorgan, "Review of Whewell's Novum Organon Renovatum", The Athenaeum, no. 1628, pt. 1 (jan. 8, 1859), pp. 42-44.

23 Whewell, op. cit., p. 110.

24 Whewell, ibid., Letter to DeMorgan, p. 110.

25 Marion Rush Stoll, Whewell's Philosophy of Induction (Lancaster, Pa., 1929), p. 98.

26 Whewell, op. cit., p. 140.

27 Whewell, ibid., p. 139.

28 Whewell, ibid., pp. 141-142.

29 Mill, op. cit., p. 199.

30 Whewell, op. cit., pp. 140-141.

31 Whewell, ibid., p. 142.

32 Whewell, ibid., p. 144.

33 Whewell, ibid., p. 20.

34 Whewell, ibid., p. 192.

35 Mill, op. cit, p. 191.

36 Mill, ibid., p. 193.

37 Whewell, op. cit., p. 278.

38 Mill, ibid., p. 195.

39 Butts, op. cit., p. 287.

40 See for example, Irving Copi, Introduction to Logic, (New York, 1961), pp. 394-399. Also see the paper by E. W. Strong, op. cit., pp. 229-231.

41 Mill, op. cit., p. 192.

42 Mill, ibid., pp. 198-199.

43 R. E. Butts provides a lucid analysis of other, more serious, difficulties with Whewell's view of inductive proof in Whewell's Theory of Scientific Method (Pittsburgh, 1968), pp. 22-29.

44 Mill, op. cit., p. 200.

45 M. R. Stoll, op. cit., p. 99.

46 M. R. Stoll, ibid., p. 192.

47 This discrepancy can also be seen to persist throughout Whewell's critique of Newton's Rules of Philosophizing. On the one hand, Whewell is disappointed in Newton's Rules because they provide no maxims or rules for future discoveries, and on the other hand, he holds the view that no such rules are possible.

48 Collected Paper, op. cit., p. 5.171.

49 W. Dray, Laws and Explanation in History (Oxford, 1964), pp. 90-93.

50 P. B. Medawar, op. cit., p. 52.

51 A. Einstein, The Method of Theoretical Physics (Oxford, 1933). Quoted from N. R. Hanson's Patterns of Discovery (Cambridge, 1965), p. 119.

CHAPTER III

A LATER NINETEENTH CENTURY VIEW: CHARLES S. PEIRCE

Charles Sanders Peirce (1839-1914) continued the line of enquiry suggested in the work of Whewell, namely: what is the precise nature of the process which enables man to introduce the appropriate "Conception", or hypothesis, in science? Whewell had argued (correctly I think) that hypotheses are <u>not</u> arrived at through a straightforward inductive procedure of the type Mill thought possible, and that the correct hypotheses are arrived at by something more akin to "intelligent guessing". To this much, Peirce was in full agreement with Whewell,[1] but what was needed in order to amplify Whewell's thinking was a more thorough investigation into what made some guesses "intelligent" guesses and others not. For this purpose Peirce introduced a third type of reasoning which he distinguished from Deduction and Induction, one which he calls "Retroduction". The term "retroduction" was taken from Aristotle's ἀπαγωγή, but Peirce often calls it "abduction", and sometimes simply "hypothesis". Peirce claims that: "The first starting of a hypothesis and the entertaining of it, whether as a simple interrogation or with any degree of confidence, is an inferential step which I propose to call <u>abduction</u>".[2] And of the three basic types of reasoning Peirce says:

> If we are to give the names of Deduction, Induction, and Abduction to the three grand classes of inference, then Deduction must include every attempt at mathematical demonstration, whether it relates to single occurrences or to "probabilities", that is to statistical ratios; Induction must mean the operation that includes an assent, with or without quantitative modification, to a proposition already put forward, this assent or modified assent being regarded as the

> provisional result of a method that must ultimately bring the truth to light, while Abduction must cover all the operations by which theories and conceptions are engendered.[3]

In commenting on this threefold division of reasoning in Peirce, A. W. Burks points out that:

> All three (types of reasoning) are based upon the idea of an hypothesis. Abduction invents or proposes hypotheses; it is the initial proposal of an hypothesis because it accounts for the facts. Deduction explicates hypotheses, deducing from them necessary consequences by means of which they may be tested. Induction tests or establishes hypotheses; as a believer in the frequency theory of probability Peirce used the phrase "evaluate them". "Abduction is the process of forming an explanatory hypothesis. It is the only logical operation which introduces any new idea; for induction does nothing but determine a value, and deduction merely evolves the necessary consequences of a pure hypothesis" (5.171).[4]

Although Peirce's views on induction, indeed logic in general, underwent considerable revision and development during his productive life, there is one point which Peirce never changed his mind about, namely: that induction is primarily a method of justification and proof, whereas abduction is a method of discovery and invention. "Abduction", Peirce says, "involves an original suggestion; while typical induction has no originality in it, but only tests a suggestion already made".[5] And, not surprisingly, the "original suggestion" which Peirce refers to here is nothing other than a hypothesis. Peirce, like Whewell before him, also holds the view that the truly important properties, which are expressed as predicates in a hypothesis, are not part of (nor seen in) the observational data themselves.

> Any proposition added to observed facts, tending to make them applicable in any way to other circumstances than those under which they were observed, may be called a hypothesis. A hypothesis ought, at first, to be entertained interrogatively.

> Thereupon, it ought to be tested by experiments so far as practicable. These are two distinct processes, both of which may be performed rightly or wrongly ... But it is the first process, that of entertaining the question, which will here be of foremost importance.[6]

It is clear from this that Peirce is crystallizing, and making more succinct, some of the basic observations which were already noted by Whewell. Unlike Whewell, Peirce has not clouded the issue by calling _two_ process by _one_ name (i.e., "Induction"). The two contexts, that of _discovery_ and _justification_, are clearly demarcated by Peirce and given two distinct names. Moreover, where Whewell had said that the former context (or process) relies on "intelligent guesses" by someone skilled in the discipline, Peirce goes further and says that it involves an "inference" _viz._, "abductive inference".

However, given the long traditional association of the word "inference" being used only in connection with "deduction" and "induction", a legitimate question can be raised as to just what Peirce means by "inference" (e.g., in the phrase "abductive inference"). Although Peirce himself never offers a clear definition of 'inference', there are numerous passages in his analysis of _belief_, together with several examples of the different types of inference, which enable one to determine what Peirce meant by the term 'inference'. The essential ingredient of "inference" for Peirce appears to be, as A. W. Burks suggests, "deliberate and consciously controlled thinking". In Burks' perceptive analysis of Peirce's theory of Abduction, he points out that:

> Though Peirce's conception of logic as a study of habits of inquiry leaves room for the view that abductive discovery may be inference, it is actually in this theory of reasoning as normative that we find a positive justification for such a position. Reasoning, according to Peirce in his later period, is thinking which is deliberate and

> consciously controlled. "A proof or genuine argument is a mental process which is open to logical criticism" (2.26). A man is reasoning when he deliberately and consciously adopts a conclusion because he sees that it follows from the premises in accordance with a method or leading principle which he approves and which he consciously sees is applicable to the particular case ... . Hence, on Peirce's view, logic is the study of how one cognition ought to determine another cognition. It is clear that on the basis of this definition of reasoning the discovery of an hypothesis could be an inference, since it could be the deliberate determination of one cognition (the hypothesis) from another cognition (the data of the problem).[7]

The notion of deliberate and controlled thinking as a characterization of reasoning is extremely suggestive for the problem of the logic of discovery generally; but while this notion is rich it is also vague. What more is needed, at least from our present point of view, is a further understanding of how these so-called "controls" might be expressed as methodological principles (or logical rules) so that we could see how "one cognition" could be used to aid in the "determination" of another. That is, the nature of the inferential relationship between the data of the problem and the cognition of the hypothesis, needs to be articulated in more precise terms. However, in the absence of such an analysis, Peirce does provide us with some examples of historical scientific discoveries which he considers to be fine examples of the type of "controlled thinking" that he calls "retroduction" (or abduction).

It would appear that there is something more than coincidence in the fact that Peirce selected (our old friend) Kepler's elliptical hypothesis as a typical example of retroductive inference. Peirce had followed the controversy between Whewell and Mill, who were, after all, two of the most formidable methodologists of his generation and he (Peirce) wasted few words in commenting on their respective contributions.

Thus, with no inconvenience, we can see an example of what Peirce called Kepler's "retroductive reasoning", while getting his assessment of Whewell and Mill at the same time. Peirce states that:

> Whewell described the reasoning (of scientific method) just as it appeared to a man deeply conversant with several branches of science as only a genuine researcher can know them, and adding to that knowledge a full acquaintance with the history of science. These results, as might be expected, are of the highest value, although there are important distinctions and reasons which he overlooked. John Stuart Mill endeavoured to explain the reasonings of science by the nominalistic metaphysics of his father. The superficial perspicuity of that kind of metaphysics rendered his logic extremely popular with those who think, but do not think profoundly; who know something of science, but more from the outside than the inside, and who for one reason or another delight in the simplest theories even if they fail to cover the facts.[8]

Of Kepler's discovery in particular, Peirce states that:

> Mill denies that there was any reasoning in Kepler's procedure. He says it is merely a description of the facts. He seems to imagine that Kepler had all the places of Mars in space given him by Tycho's observations; and that all he did was to generalize and so obtain a general expression for them. Even had that been all, it would certainly have been inference ... But so to characterize Kepler's work is to betray total ignorance of it. Mill certainly never read the De Motu (Motibus) Stellae Martis, which is not easy reading. The reason it is not easy is that it calls for the most vigorous exercise of all the powers of reasoning from beginning to end.[9]

Peirce, at this point, offers a rather long and detailed exegesis of the reasoning involved in Kepler's discovery; reasoning which Peirce considered to be a paradigm of retroductive inference. But in so far as the general form of retroductive inference is what is of particular concern here, much of the detail of the example can be overlooked. The following sketch of Peirce's discussion illustrates those elements of Kepler's discovery which are (or at least appear to be) characteristic of retroduction generally. According to Peirce:

> What Kepler had given was a large collection of observations of the apparent places of Mars at different times. He also knew that, in a general way, the Ptolemaic Theory agrees with the appearances, although there were various difficulties in making it fit exactly. He was furthermore convinced that the hypothesis of Copermicus ought to be accepted ...[10]

What is noteworthy here is the fact that Kepler was not simply working from observation alone; he was working with observation in conjunction with previously accepted data and theories and difficulties. Hence this much of retroduction appears to agree with Whewell's view (as against Mill's) that the formation of a hypothesis involves much more than the data of observation. From the standpoint of "retroduction" itself, we see that it (i.e., retroductive reasoning) begins with: (1) a problem,[11] (2) some (at first stages) disconnected observations, and (3) some previously established theories which appear relevant to the question at issue. These constitute the basic elements (or, if you will, "components") with which retroduction works.[12]

Peirce continues his analysis of Kepler's reasoning thus:

> ... Now Kepler remarked that the lines of apsides of the orbits of Mars and of the earth are not parallel; and he utilized various observations most ingeniously to infer that they probably intersected in the sun. Consequently, it must be supposed that a general description of the motion would be simpler when referred to the sun as a fixed point of reference than when referred to any other point. Thence it followed that the proper times at which to take the observations of Mars for determining its orbit were when it appeared just opposite the sun -- the true sun -- instead of when it was opposite the mean sun, as had been the practice. Carrying out this idea, he obtained a theory of Mars which satisfied the longitudes at all the appositions observed by Tycho and himself, thirteen in number, to perfection. But unfortunately, it did not satisfy the latitudes at all and was totally irreconcilable with observations of Mars when far from apposition.[13]

After Peirce explains how and why Kepler continued to modify the various hypotheses which he entertained, Peirce picks out those characteristics of Kepler's reasoning which he considers to be the most illustrative of retroduction:

> At each stage of his long investigation, Kepler has a theory which is approximately true, since it approximately satisfied the observations ... and he proceeds to modify this theory, after the most careful and judicious reflection, in such a way as to render it more rational or closer to the observed fact ... .
>
> Thus, never modifying his theory capriciously, but always with a sound and rational motive for just the modification he makes, it follows that when he finally reaches a modification -- of most striking simplicity and rationality -- which exactly satisfies the observations, it stands upon a totally different logical footing from what it would if it had been struck out at random, or the reader knows not how, and had been found to satisfy the observation. Kepler shows his keen logical sense in detailing the whole process by which he finally arrived at the true orbit. This is the greatest piece of Retroductive reasoning ever performed.[14]

Thus we see that, for Peirce, retroduction is a kind of hypothetical reasoning, whereby theory and observations are brought together in such a way that the hypothesis can be regarded as the conclusion of a hypothetical argument. There is constant interplay between theory and observation which helps to suggest the next, most plausible, hypothesis. Indeed Kepler's reasoning, and presumably much retroductive reasoning, consists of several hypotheses each one of which becomes changed and modified as new data become available.[15] Retroduction, then, is a kind of reasoning, or argument, where a hypothesis serves as conclusion, and some combination of theory with observations serve as premises. One could in fact look at the form of retroduction as being just the reverse of modern hypothetico-deductive (H-D) form of explanation. Where the H-D account begins with a law or hypothesis and a set of initial conditions, and from these generates low-level observation statements, Retroduction, on the other hand, begins with a problem, some observations, and initial conditions, and from there must work back up to a hypothesis.[16]

One might rightly suggest, at this point, that the idea of "Retroduction" appears to be quite clear as far as it goes, but it also raises as many questions as it sets out to answer. One might ask, for example, whether or not retroduction might just as easily be regarded as a kind of "hypothetical guessing", as opposed to strict "reasoning", and thereby undermine the foundation of "retroduction" as a form of _inference_. Secondly, one might ask whether Peirce is not just making a psychological observation about the contingent way scientists may often think, i.e., in what sense can retroduction be considered a kind of _logic_, as against _psychologic_? And thirdly, but perhaps most importantly, one might point out that although it is helpful to suggest that the _form_ of retroduction is the reverse of "hypothetico-deduction", this is still far from being an adequate (or complete) characterization of retroduction as a _bona fide_ inference procedure. Hypothetico-deduction, after all, has many _rules_ (e.g., the identity theorem, _modus ponens_, etc.) which serve as the formal substance to its outward form. What analogous rules does "retroduction" possess which would give _substance_ to its form?

All of these questions, and the possible variations thereof, are crucial to Peirce's case that retroduction is a form of _inference_, and, as such, may be regarded as a kind of _logic_ for the formation of scientific hypotheses. Peirce had anticipated some of these questions, and his answers to them continue to be pertinent to the present-day problem. To the first question, that retroduction ought not be considered a form of inference because it involves an element of random guessing, Peirce's answer is extremely interesting. For Peirce, a mode of reasoning is good or valid if from true premises it yields true conclusions as frequently as it professes to do so: either always, as deduction professes, or in a better than chance proportion of cases, as abduction and induction profess to do.[17] In "The Doctrine of Chances", Peirce expresses himself in the following way: The probability of a deductive conclusion is one; the probability of an inductive conclusion, while less than one, is significantly greater than chance. V. Thomas' statement of Peirce's reply to the question is concise and accurate:

> As for abduction, Peirce claims that we know by induction that abduction is valid. Although it is conceivable that all the hypotheses that ever occurred to anyone could have been false, we know by induction that some have been true. "A man must be downright crazy to deny that science has made many true discoveries" (C.P.5.172), and every discovery was originally an abductive suggestion. This success, Peirce contends, is not a matter of chance:
>
> "Think of what trillions of trillions of hypotheses might be made of which one only is true; and yet after two or three or at the very most a dozen guesses, the physicist hits pretty nearly on the correct hypothesis. By chance he would not have been likely to do so in the whole time that has elapsed since the earth was solidified". (C.P.5.172).[18]

In effect, Peirce's answer to the charge that abduction is not a form of inference, is to point out that when the number of successful hypotheses is contrasted with the number of possible hypotheses, one will see that the ratio is considerably higher than chance; and, according to Peirce, this kind of evidence ought to convince the rational man that the process of suggesting hypotheses is not merely guessing, and ought, therefore, to be considered inferential in nature. However, Peirce's argument here only establishes that retroduction is not a matter of chance; in order for the argument to establish that retroduction is a form of inference, as such, one would have to accept Peirce's rather vague and inadequate conception of 'inference' which has already been criticized above. In sum, Peirce has made a good case that retroduction is not a matter of chance, thus there is cause for the hope of constructing a logic, however, Peirce has not established retroduction as a form of inference.[19]

To the second question (i.e., that Peirce might be doing psychology, as opposed to logic) Peirce's answer involves his general theory of signs. In so far as an analysis of this part of Peirce's work goes beyond the scope of this essay, one or two statements from some noted scholars, who have looked at this question in Peirce, will have to suffice for the present. Ernest Nagel spoke to this question at some length in his address to the "Fifth International Congress for the Unity of Science":

> It is unnecessary on this occasion to speak of Peirce's contributions to formal logic (e.g., his improvements on Boole's work, his development of the logic of relations, etc.) ... . An adequate logical theory, according to him, must take into account the complicated properties and functions of signs in inquiry, and even the rules of formal logic were regarded by him as intimately related to the habits of action generated in the course of successful inquiry. Thus, the alleged facts of "consciousness" were dismissed by him as totally irrelevant to the question of the validity of the laws of logic ... . Accordingly, Peirce was one of the most pronounced foes of attempts to base logic upon inner feelings of certitude or other facts of individual psychology, and his conception of the nature and function of formal logic is incompatible with interpretations of inquiry in terms of a "mentalistic" theory of thinking.[20]

And in several places throughout Professor Hanson's work we find him warning that:

> When Popper, Reichenbach, Braithwaite, and others argue that there is no logical analysis appropriate to the intricate and mysterious psychological complex within which new ideas spark forth, they were saying nothing of which Peirce was unaware ... . Peirce was as capable as our ultra-modern contemporaries of detecting a genetic fallacy.[21]

And also that:

> Aristotle and Peirce thought they were doing something other than psychology, sociology, or history of discovery; they purported to be concerned with a <u>logic</u> of discovery: theirs was a philosophical inquiry about the formal structure of reasoning which constitutes scientific innovation and discovery.[22]

These statements should at least suggest caution to any premature charges of psychologism in Peirce's theory of abduction. Indeed, Peirce's own distinction between psychology and logic appears to be as sound as any found in contemporary literature on the problem.

However, to the third question, which asks whether there are any formal rules which provide <u>substance</u> to the outward form of "retroduction", Peirce provides no definite answer. Most of Peirce's energies in this area were taken up with clarifying the very <u>idea</u> of "retroduction", and also with arguing that it was a form of inference -- theses which have still to find their mark with most of the philosophical community. In fact, N. R. Hanson, a modern proponent of the logic of discovery, spent most of his work on the problem explicating Peirce's views and trying to make them more acceptable to a modern audience. Hanson succeeds, I think, in making Peirce's views on retroduction more understandable, indeed, more persuasive, but Hanson too fails to provide the formal machinery which is lacking in Peirce's account. Even if one agrees with Peirce and Hanson that the suggestion and formation of scientific hypotheses is a reasonable procedure, one still wants to ask: what, precisely is the procedure like, and what are the rules governing its inferences?

It should be noted at this point that Peirce does not have a distinction between "having a method" and "having a logic", as outlined in chapter I of this essay. Peirce tackles the problem of a logic of discovery directly, and does not address himself to the related problem of a method, simpliciter. In this chapter, I discuss Peirce's arguments in his terms, eventhough some of the earlier distinctions could have been useful to him.

In "Pragmatism and Abduction", Peirce attempts to offer a clear, but unfortunately brief, answer to the questions (above) about the rules governing 'its inferences'.

> Long before I first classed abduction as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis -- which is just what abduction is -- was subject to certain conditions. Namely, the hypothesis cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts or some of them. The form of inference, therefore, is this:
>
> > The surprising fact, C, is observed:
> > But if A were true, C would be a matter of course. Hence, there is reason to suspect that A is true.
>
> Thus, A cannot be abductively inferred, or if you prefer the expression, cannot be abductively conjectured until its entire content is already present in the premise, "If A were true, C would be a matter of course".[23]

Hanson, in fact, takes this particular notion or view of retroduction from Peirce, and later defends it as being essentially correct as a characterization of the form of the logic of discovery generally. Hanson's schema is somewhat clearer however.[24]

(1)	P	(some surprising phenomenon is observed)
(2)	$H \supset P$	(If H were true, it would explain P as a matter of course)
$\therefore$ (3)	H	(Hence, there is reason to think that H is true).[25]

While this provides some idea of what Peirce considered the form of retroduction to be, it remains quite far from providing a schematism which could safely be called a logical system.

It would not be far off the mark to characterize a large part of the modern problem of the logic of discovery as an effort to provide more formal substance to the form of reasoning which Peirce called "retroduction". Peirce had obviously benefited from both the contributions and the confusions in the debate between Whewell and Mill: all three philosophers had a mutual concern for the problem of suggesting reasonable hypotheses prior to their confirmation. One might also point out that modern discussions on the problem of the logic of discovery owe a similar debt to the advances made by Peirce -- this is particularly true of Hanson. Peirce had argued that the suggesting of scientific hypotheses is a rational affair, and as such, it should be "rationally reconstructable". In fact, the schema above is the result of Peirce's efforts at such a reconstruction; and despite the remaining problems with it, it would be simply rash for anyone to suggest now that scientific discovery is purely a matter of psychology: Peirce's arguments must be faced head-on.

For all this, however, there are other problems with Peirce's account of scientific discovery, which have not been adequately discussed in the literature, and which might be useful to look at here.

*****

It has already been pointed out that attempts at rationally reconstructing the context of discovery have been hampered by difficulties which have not been encountered in the <u>context of justification</u>, and the reasons for this are really quite simple. Reconstructionists in the context of justification (e.g., Reichenbach, Carnap, Hempel, <u>et al</u>) have assumed, quite understandably, that scientific justification must be a rational procedure; and they saw their task, <u>qua</u> philosophers of science, as clarifying the underlying logic of this process. On the other hand, efforts at reconstruction in the context of discovery (e.g., those of Hanson, Toulmin) have not been allowed to assume that this process is similarly totally rational, indeed, the contrary view is often dogmatically asserted. Discovery does not strike one as having the same <u>prima facie</u> rationality to it. Thus, most of the work in the area of discovery has been taken up with the fundamental preliminaries (e.g., showing that it is not just chance, nor mere guess-work); and the serious business of actually reconstructing this process in any rigorous way has unfortunately been long overdue. Peirce's work on the problem of the logic of discovery was no exception to this: he was primarily concerned with showing that the process of suggesting scientific hypotheses was a reasonable affair involving inference; and "retroduction" was the name of such inferences. (Again it should be noted that Peirce did not have, or at least employ, the distinction between "having a rational <u>method</u> of discovery" and "having a <u>logic</u> of discovery"). But for one reason or another Peirce did not develop retroduction any further than pointing out its most general structure (e.g., the schema above). He did not codify this "inference" procedure into any system of rules, such that it delineates the "logic of discovery". Thus his attempt at "rational reconstruction" is far from complete even if it is correct as far as it goes.

For the purpose of seeing just how far Peirce's efforts at "reconstruction" succeeded or failed, let us contrast his reconstruction with one that is perhaps the most widely known in contemporary philosophy of science, viz., the Hempel-Oppenheim model of scientific explanation (or more simply, the "D-N model").

While it is true that Hempel encounters some difficulties with his logic of explanation, he nevertheless makes clear what his proposals are. The models of explanation (e.g., the D-N, and statistical-nomological):

> ... are not meant to describe how working scientists actually formulate their explanatory accounts. Their purpose is rather to indicate in reasonably precise terms the logical structure and the rationale of various ways in which empirical science answers explanation-seeking why-questions.[26]

When Hempel speaks of a logic of explanation, he means the set of requirements which an argument must meet to be classed as a deductive-nomological explanation: his logic of explanation is an explication of the concept of deductive-nomological explanations. Similarly, Peirce's attempts at a logic of discovery may be construed as an explication of the concept of retroduction.

However, rational reconstructions (such as Hempel's) can be described and evaluated at two distinct levels. There is a structural, or syntactical, description of the concept under consideration, and there is also a logical (or semantical) description of it. For example, when Hempel describes the form of a D-N explanation in terms of the general model:

$$\begin{array}{ll} \text{Explanans} & L_1, L_2, L_3, \ldots L_n \text{ (law-like statements)} \\ & \underline{C_1, C_2, C_3, \ldots C_n} \text{ (initial conditions)} \\ \text{Explanandum} & \qquad E \end{array}$$

He is here describing the structure, or syntactics, of scientific explanation generally; and this general structure has corresponding conditions of adequacy (which will be discussed in a moment). Secondly, the concept of scientific explanation can be discussed and evaluated in terms of the logical rules which it employs, and in Hempel's case the rules are the standard rules of deductive logic such as those found in Principia Mathematica. It is on this second level, incidentally, which the now famous (or infamous!) paradoxes of confirmation are generated; but these difficulties leave the structural (or syntactical) description of explanation untouched. Thus, Hempel's rational reconstruction of scientific explanation is composed of two distinct claims: (1) that the D-N model is an accurate structural description of the concept of scientific explanation; and (2) that the logical rules of the Principia are sufficient mechanical tools (as it were) for making the required inferences. The conjunction of these two claims constitutes the foundation of Hempel's "rational reconstruction". Hempel has, in fact, faced separate criticism on both of these fronts. For example, the D-N model has been criticized on conceptual grounds (e.g., by Scriven, Donegan, Hanson, et al.) for not capturing all the peculiarities and nuances of explanation; and the model has also been criticized on formal grounds (i.e., on the second level) because it issues in paradoxes.[27]

Notwithstanding these difficulties, however, the point to be noted here is that rational reconstructions usually involve two distinguishable tasks: the first is what has been here called the structural or syntactical analysis; and the second task is that of providing the logical, or logical-rule-like, mechanics of the process.[28] These two task correspond to my earlier distinction between: describing the method, and providing the method with a logical structure, *vis a vis* rules.

Peirce's analysis of retroduction can be viewed as an analogue to Hempel's analysis of explanation. Peirce's proposal is to present a schema, or structural description, of scientific discovery. Where Hempel's model is deductive-nomological, Peirce's model is retroductive. Thus, from the point of view of the conceptual, or structural tasks, (and omitting the logical task) the following schemata:

Hempel's Reconstruction	Peirce's Reconstruction
Explanation:	Discovery:
$L_1, L_2, \ldots L_n$	P
$C_1, C_2, \ldots C_n$	$H \supset P$
E (explanandum)	H (tentatively adopt H)

represent similar undertakings. Both schemata are alleged to describe the outward form (or structure) of their respective constructs. Peirce's schema of retroduction is his structural model, or description, of the way in which typical scientific hypotheses are arrived at (i.e., his conceptual analysis of scientific discovery).

As with most models or constructs of hitherto vague concepts, either definitions or precise conditions of adequacy are needed to distinguish *bona fide* cases of the concept from erroneous ones. Hempel, again, is quite clear about specifying the conditions which an explanation must meet in order to be truly classed as "deductive-nomological". For Hempel, an argument from explanans to explanandum is deductive-nomological if and only if: (1) the explanans implies the explanandum; (2) the explanans contains at least one general law non-vacuously; (3) the explanans has empirical content; and (4) all sentences in the explanans are true.[29] To find similar conditions of adequacy for Peirce's model of retroduction one would be constrained to interpolate from some few passages throughout Peirce's work. At several places Peirce insists on the hypothesis having definite empirical content,[30] and at 5.189 he says, "The hypothesis (H) cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts or some of them".[31] Moreover, it should also be remembered that Peirce need not require that the hypothesis be _true_ (as in Hempel's condition (4)) because he construes retroduction, as a method of suggesting _reasonable_ hypotheses.

However, even if one were to reconstruct Peirce's conditions in a more rigorous way, those conditions would still be incomplete, (i.e., inadequate): they do not enable one to distinguish _legitimate_ retroductions from _illegitimate_ ones. That is, Peirce's schema of the form of retroduction, together with his conditions of adequacy, do not rule out the possibility of _unreasonable_ hypotheses being suggested in the same manner, i.e., these can also be suggested retroductively. Retroduction, after all, as a form of inference, is supposed to license some suggestions while ruling out others. While it is true that retroduction licenses only those suggestions which are empirically testable and which _could_ account for the problematic data, this still does not enable one to distinguish the plausible (or most reasonable) hypotheses from those which are _prima facie_ implausible. In typical investigative situations it is always possible to suggest a large class of hypotheses which might explain the data, yet only some subset of this class has initial plausibility. The class of _plausible hypotheses_ is not equivalent to the class of all _possible_ ones. Peirce's schema of retroduction does not take into account the fact that plausibility, or reasonableness, is not an intrinsic quality of sentences or hypotheses, but it is a _relation_ between sentences, or between hypotheses and the respective background conditions (e.g., existing sets of beliefs, accepted theories, etc.).

If there is to be a logic of suggestion (which is what retroduction purports to be) then reference must be made to the relation between the suggested hypothesis and background information. A gypsy and a scientist may both suggest hypotheses describing the occurrence of an eclipse cycle, but determining the reasonableness of their suggestions requires some knowledge of their reasons for suggesting the hypothesis, i.e., knowledge of the background conditions on which each suggestion is based. Peirce's conceptual analysis of retroduction fails to show precisely how this information fits into his logic of suggestion.

Thus far, the difficulties referred to here have been directed at Peirce's conceptual, or structural, analysis of retroduction. However, on the second level of Peirce's attempted reconstruction there is perhaps a more obvious shortcoming. Peirce's schema suffers from the noticeable absence of any formal, or logical rules. Where Hempel's analysis of scientific explanation relies heavily on the established rules of natural deduction, Peirce made no serious attempt to present any rule-like procedures for retroduction. This is particularly unfortunate since he considered retroduction to involve a third type of <u>inference</u>, over and above deduction and induction. Thus, even the most sympathetic interpretation of Peirce's view of retroduction must face the disappointing fact that he failed to offer any inferential rules for this concept, and <u>a fortiori</u>, the desired logic of suggestion.

Thus, on both the syntactic and semantic levels of reconstruction (to continue the comparison with Hempel), Peirce's work on retroduction contains serious shortcomings on both fronts.

Despite all the difficulties surrounding Peirce's discussion of retroduction, however, none of these difficulties are insurmountable, at least not in principle. The difficulties arise primarily because his conception was under-developed, not because his perception of the problem was unclear. Indeed, Peirce saw more clearly than any of his predecessors (and many of his successors) that discovery and justification are two distinguishable sides of the methodological coin.[32] Peirce's discussion of retroduction was his attempt at rationally reconstructing the methodological problem of suggesting plausible scientific hypotheses. In effect, his work on retroduction has provided the modern day methodologist with some of the conceptual tools for understanding both the nature of the task and the problems of formulating the logic of scientific discovery.

FOOTNOTES

1 Charles S. Peirce, Essays in the Philosophy of Science, Vincent Thomas, ed., (New York, 1957), pp. 206-207.

2 Peirce, ibid., p. 236.

3 Peirce, ibid., p. 237.

4 A. W. Burks, "peirce's Theory of Abduction", Philosophy of Science, 13 (1946), p. 303.

5 Peirce, op. cit., p. 248.

6 Peirce, ibid., pp. 235-236.

7 Burks, op. cit., p. 304.

8 Peirce, op. cit., pp. 206-207.

9 Peirce, ibid., p. 207.

10 Peirce, ibid., p. 207.

11 In this case (i.e., Kepler's case) the problem is a discrepancy, or anomaly, between what is actually observed and what ought to be observed if the current theory were taken to be absolutely correct. However, this should not be taken as the only way scientific problems are recognized to arise in Peirce's system; there are still the "how to accomplish it" and other types of problems which have a claim to the title "scientific problem".

12 These elements can be viewed (correctly I think) as a natural outgrowth of Peirce's pragmatism which holds the view that "all enquiry begins with a problem", and also from his anti-cartesianism which holds that "knowledge is never acquired in isolation from our previous experience and biases".

13 Peirce, ibid., p. 208.

14 Peirce, ibid., pp. 208-209.

15 In Patterns of Discovery, (Cambridge, 1965) pp. 78-79, N. R. Hanson amplifies this point with an italicized statement: "The move of treating observed physical phenomena as approximations to mathematically 'clean' conceptions developed after Kepler into a defining property of physical inquiry".

16 In his paper "The Idea of a Logic of Discovery", _Dialogue_, 4, 1966, N. R. Hanson also contrasts retroduction with the H-D account, but Hanson restricts retroduction to _anomalies_ (i.e., conflicts with accepted theory). I do not think this restriction necessary, nor do I think it can be found in Peirce.

17 I am here following V. Thomas' analysis _op. cit._, p. xiv.

18 Thomas, _ibid._, p. xv.

19 Peirce's argument provides evidence for the view that there is _some method_ of discovery at work, if not a _logic_, and perhaps Peirce should have argued for this weaker claim.

20 "C. S. Peirce, Pioneer of Modern Empiricism", _Philosophy of Science_, 7, 1940, pp. 72-73.

21 N. R. Hanson, _Perspectives on Peirce_, ed. by R. J. Bernstein (Yale Press, 1965), pp. 43-44.

22 N. R. Hanson, "The Idea of a Logic of Discovery", _Dialogue_, 4, 1965-66, pp. 49-50.

23 Collected Papers, _op. cit._, Vol. V, 5.189, p. 117.

24 _Patterns of Discovery_, (Cambridge, 1965), p. 86.

25 The '$H \supset P$' is not intended as a translation of the _subjunctive_ (i.e., "were" and "would"), it is simply a rendering of the proposition that: P if H. The subjunctive conditional was Peirce's original way of stating the relationship, however, this latter way is a reformulation contributed by Gerd Buchdahl and N. R. Hanson.

26 C. Hempel, Aspects of Scientific Explanation and Other Essays in the _Philosophy of Science_, (New York, 1965), p. 412. This quote also demonstrates the point (made above) that scientific explanation was assumed to have an underlying "logical structure" (as opposed to mere psychological structure) right from the beginning of the undertaking. Similar assumptions have been _verboten_ in the context of discovery.

27 See Israel Scheffler's excellent discussion of these paradoxes in his _Anatomy of Inquiry_, (New York, 1967), pp. 225-295.

28 The situation here is quite like that of designing an experiment in the social sciences: there is the task of designing the experiment, and also that of applying the proper statistical procedures. Distinguishable sets of problems characterize each of these tasks.

29 C. Hempel, _op. cit._, in "Studies in the Logic of Confirmation"

30 Peirce's discussion on this point clearly anticipates Popper's "falsifiability" as a demarcation criterion.

31 Collected Papers, _op. cit_., Vol. V, p. 117.

32 See the quote by P. B. Medawar at the beginning of Chapter II of this essay, p. 33.

CHAPTER IV

NORWOOD RUSSELL HANSON

1. General Perspective

In this chapter I will examine the views of N. R. Hanson on the PLD as an historical development of Peirce's general point of view. In particular, I will examine Hanson's contributions from the perspective of the general framework introduced in chapter I, and attempt to determine the extent to which Hanson might (or might not) have brought the PLD closer to a solution.

From the point of view of the history of the philosophy of science, Peirce's work on the logic of discovery problem laid dormant for the half century following his death in 1914. About the time of Peirce's death, the new developments which emanated from the Vienna Circle captured the center of interest of most philosophers of science. Logical positivism not only offered new approaches to old problems, but it also had the more profound effect of establishing a new network of questions which circumscribed (or implicitly defined) the field of the philosophy of science. In the terminology of T. S. Kuhn, positivism established a new "paradigm" of enquiry for the philosophy of science. The primary concern of the philosophy of science now centered on epistemological issues (e.g., justification, explanation, confirmation, etc.) almost to the total exclusion of procedural problems in methodology: wherein lies the problem of the logic of discovery. The PLD belonged to the "old paradigm" which the positivist's revolution left behind in order to pursue the logic of confirmation. Thus, from about 1908 to 1958 the PLD, as a methodological problem, was all but forgotten in the philosophy of science. While it is true that Karl

Popper published his Logic der Forschung in 1934, what he means by 'discovery' in this book is clearly epistemological in that it attempts to establish conditions of scientific proof (vis-a-vis "falsification"). Under the title The Logic of Scientific Discovery, Popper clearly says:

> In this book I intend to give a more detailed analysis of the methods of deductive testing. And I shall attempt to show that, within the framework of this analysis, all the problems can be dealt with that are usually called 'epistemological'.[1]

Thus, the nineteenth century conception of a "logic of discovery" which was concerned with the suggestion of hypotheses prior to testing, underwent a radical change in meaning and purpose in the new positivist paradigm.

Against this chorus of positivist influence, Norwood Russell Hanson, in 1958, once again took up Peirce's original methodological concern. He began publishing papers which attempted to explicate and develop the underlying rationale of retroduction as a logic of discovery.[2] Hanson's work, which is rich with historical examples, had the effect of helping to refurbish philosophical interest in the history of science in general, and the PLD in particular. In the later part of the twentieth century many philosophers of science have begun (once again) to exploit the history of science for a broader understanding of what goes on in scientific thinking. The work of Hanson, Kuhn, Polanyi, and Feyerabend to mention a few, has recently begun to function as a kind of counter-revolution to the strict formal reconstruction of the early positivists.[3] Part of the purpose of the present essay is, in fact, an attempt to show that there is something good and something ill in both camps, and that there is reason for optimism when the strengths of both approaches are conjoined. But more of this later.

Perhaps because Peirce's approach to the logic of discovery problem had been left aside for so long, Hanson found it necessary to buttress his arguments for a logic of discovery with repeated polemics against what he considered to be hypothetico-deductive establishment. Part of Hanson's task was to reestablish the methodological problem of discovery as a serious problem in the philosophy of science, and much of his effort was directed toward this end. Consider, for example, the beginnings of three typical articles by Hanson:

> Suppose Charles Sanders Peirce were alive today. He would now see some of his ideas being rejected just as energetically as they were fifty years ago. The historian of science in him always caught fire when contemplating the context within which new discoveries were conceived . . . [4]

and

> Is there a logic of scientific discovery? The approved answer to this is "No". Thus Popper argues . . . [5]

and thirdly

> In 1924 -- that was the year I was born, so I remember it well -- a distinguished mountain climber named Mallory was often asked: "Why climb Everest?" His resonant and memorable response was always: "Because it's there!"
>
> Some people ask me: "Why agonize about discovery?" My timid response is usually 'Mallorian': I say: "Because it's there!" A concept unanalyzed is a concept unknown.[6]

While much of Hanson's work consisted of arguments supporting the basic idea that 'discovery is a central and important concept in the philosophical analysis of science, the present essay takes this point as given, and will concern itself primarily with examining Hanson's positive suggestions for a logic of discovery. This task, however, is more easily said than done since Hanson's conception of 'logic' sometimes takes the form of Wittgensteinian "conceptual analysis", and at other times he means the more familiar development of formal inference systems.[7] In point of fact, Hanson did not have a singular approach to the logic of discovery problem, but continued to develop several different ideas on the subject right up until his death in 1967. In each case, however, he credits Peirce with providing him with the basic schematism for approaching the problem.

In what follows I will attempt to examine, and then to evaluate, three general lines of argument which Hanson continued to stress. These particular arguments appear to be the most relevant to our present concern of rationally reconstructing the process of scientific discovery; and they are also the three lines of argument which Hanson himself considered to be his most substantive contributions to the problem. In one of his last papers on the subject, Hanson summarizes these three lines of argument by giving each a distinguishable title:

> In earlier papers I sought to distinguish between reasons for accepting particular H's (hypotheses) and reasons for entertaining H-types. But with even this little laid bare it already appears that different ingredients have long been lumped into the idea of a 'Logic of Discovery'. Each of these is present in Peirce, somewhere. And each of them has figured somewhere in my own past scribblings. I will dub these ingredients:
>
> (A) 'Logic of Discovery' <u>à la Patterns of Discovery</u>.
> (B) 'Logic of Discovery' <u>à la Is there a Logic of Discovery?</u>
> (C) 'Logic of Discovery' <u>à la Retroductive Inference</u>.
>
> This, for no reason better than that the conceptual strands later to be intertwined are best known to me in these terms.[8]

There is also a very rough chronological order to Hanson's emphasis with each of these lines of argument, or "ingredients" as he called it, but it is easier to consider the ordering of these themes (i.e., A,B,C, above) in terms of what could be considered logical priorities. In "A", for example, Hanson was attempting to show that there existed certain recurrent patterns in ways of organizing data, that typified many significant scientific discoveries. While in "B", Hanson was trying to establish that these patterns of reasoning (from A) constituted a unique and distinguishable type of rational inference (i.e., distinguishable from deduction and induction). And in "C" Hanson was trying to schematize in as formal a way as possible, this unique type of inference (from B) which leads to scientific discovery. Thus, it will be appropriate to begin this examination of Hanson's views with Patterns of Discovery, and to consider the others in due course.

2. 'Logic of Discovery' à la Patterns of Discovery.

In Patterns of Discovery Hanson makes two points, in particular, which are pertinent to the reconstruction of a logic of discovery. The first point, which is by far the most general, results from the epistemological thesis advanced in the book (viz., that scientific observation is "theory-laden").[9] The second pertinent point has to do with his analysis of what he takes to be the typical "patterns of inference" in discovering hypotheses. Hanson's general point will be considered here first.

In the introduction, Hanson urges the reader to look at the history of science in such a way that the current unsettled nature of quantum mechanics be regarded as the normal state of affairs in any research discipline: he considers elementary particle physics as paradigmatic of all physical enquiry. Thus, he suggests, "Particle theory will be the lens through which these perennial philosophical problems will be viewed".[10] Hanson's reason for doing this is that he believes that to approach the study of science, and its history, through already "polished systems" throws no light whatever on the methodological problems of science when viewed as an investigative enterprise. Indeed, he argues that to approach the history of science through the study of "finished" research (e.g., planetary mechanics, optics, electromagnetism, etc.) leads to distortion and error. He says, for example:

> The continuity that historians like Tannery, Duhem and Sarton taught us to look for breaks down abruptly when one supposes Einstein, Bohr, Heisenberg and Dirac to be different kinds of thinkers from Galileo, Kepler and Newton. But this is wrong. These are all physicists: that is, natural philosophers seeking explanation of phenomena in ways more similar than the dichotomy 'classical-modern' has led philosophers of science to imagine.[11]

Thus, in order to understand science, Hanson recommends that we study the history of science through the problems that the investigator himself was confronted with prior to solution. From this methodological point of view, Hanson urges: "The issue is not theory-using, but theory-finding; my concern is not with the testing of hypotheses, but with their discovery".[12] He criticizes the "hypothetico-deductive" (H-D) account from this point of view, saying that:

> Physicists rarely find laws by enumerating and summarizing observables. There is also something wrong with the H-D account, however. If it were construed as an account of physical practice it would be misleading. Physicists do not start from hypotheses; they start from data. By the time a law has been fixed into an H-D system, really original physical thinking is over. The pedestrian process of deducing observation statements from hypotheses comes only after the physicist sees that the hypothesis will at least explain the initial data requiring explanation.[13]

However, in fairness to the H-D account, it should be pointed out that their concern is more with the epistemological issues in science than it is with the methodological ones; thus, theirs is not so much "an account of physical practice", as it is an account of putative physical proofs. Hanson's general disagreement with the H-D theorists is not so much a dispute about specific issues,[14] as it is a disagreement over the assessment of what is most important in scientific thinking. However, when one looks at science from Hanson's "problem solving" point of view it is easy to see why discovery plays such a central role, and why he would say:

> I should argue strongly that this was so, that the history of scientific progress is not a history of increasingly refined laboratory techniques but a history of changing conceptions. Something was looked at in a new way, the priority of some principle of nature was challenged, ...[15]

The hypothetico-deductive account of science cannot account for these important conceptual changes. Thus, Hanson suggests, the problems faced in modern particle physics lay bare the sterility of the H-D account in broadening our understanding of scientific method. What is needed for the solution of these problems is the discovery of fruitful hypotheses. Similarly:

> Kepler did not begin with the hypothesis that Mars' orbit was elliptical and then deduce statements confirmed by Brahe's observations. These latter observations were given, and they set the problem -- they were Johannes Kepler's starting point. He struggled back from these, first to one hypothesis, then to another, then to another, and ultimately to the hypothesis of the elliptical orbit. Few detailed accounts have been given by philosophers of science of Kepler's achievements, although his discovery of Mars' orbit is physical thinking at its best.[16]

With such examples as these, Hanson attempts to reveal the many advantages in studying scientific progress from the perspective of Peirce and himself. When science is viewed from the investigator's point of view, prior to solution, a concomitant appreciation for the discovery problem naturally emerges.

Thus, when one indulges Hanson, and looks at science through the "lenses" of unsettled research disciplines, such as particle physics, a new array of methodological problems comes to the forefront. The conceptual problems of the working scientist are no longer seen as the construction of proofs (or falsifications) of <u>given</u> hypotheses, but rather the problems are the construction of the hypotheses themselves. In his only text book, Hanson warns the student to "Remember all this when, in your reading, you find yourself hip-deep in justifications of induction and the foundations of probability theory".[17]

Given this perspective of science as an investigative, problem-solving, activity, let us look closer now at Hanson's analysis of these problems and what he thinks is required to solve them. In general, Hanson sees the scientist's problem (as it has been in elementary particle physics) as being a confrontation with <u>anomalous</u> data, for example: how to reconcile this (e.g., any) persistently recalcitrant data with the already existing body of accepted fact and theory? For Hanson, anomalies of this sort are the paradigmatic scientific problems; in some places Hanson quotes Aristotle's pronouncement that "All knowledge begins in astonishment".[18] Compare, for a moment, this view with T. S. Kuhn's distinction between "normal science" and "science in crisis".[19] Kuhn argues that in "normal science" the problems can be characterized as, more or less, straightforward "puzzle-solving", and only in "crisis" situations does the scientist confront the "anomalies" of the Hansonian sort, where the data is seen to actually conflict with accepted theory. In Kuhn's language, Hanson treats most (if not all) problem situations as the "crisis-type" where bona fide "anomalies" are the rule rather than the exception. Whatever examples, from the history of science, that Hanson asks us to consider, he treats as the resolution of some "anomaly"; and this in turn results in a cessation of that Aristotelian "astonishment" he speaks of.

> One must compare the conceptual perplexities of contemporary physicists with those of Galileo, Kepler, Descartes and Newton when they were creating physics. A Galileo grappling with acceleration, or a Kepler considering a non-circular planetary orbit, or a Newton reflecting on the particulate nature of matter and light -- these do not differ essentially from cases of a Rutherford entertaining 'Saturnian' atoms, or a Compton proposing a granular structure for light, or a Dirac suggesting a positive electron, or a Yukawa wrestling with the idea of a 'meson'. This is frontier physics, natural philosophy.[20]

In several places Hanson explicitly says that it is the resolution of anomalies which is constitutive of the physicists' problems, for example:

> Usually he (the physicist) encounters some anomaly; he desires an explanation of it. It cannot follow obviously from any obvious premise cluster; for, in such a case, it would not be anomalous -- it would not constitute a perplexing occasion for further enquiry.[21]

Also:

> A Logic of Discovery should concern itself with the scientist's actual reasoning which
> (C) proceeds retroductively, from an anomaly to
> (B) the delineation of a kind of explanatory H which
> (A) fits into an organized pattern of concepts.[22]

In general, Hanson sees the primary methodological problem in science, as being the construction of "new conceptions", or "hypotheses", which can render the anomalies non-problematic, or non-anomalous. And he argues that what is typically needed to accomplish this is a change in conceptions, or a change in "conceptual Gestalt".[23] For the scientist, old data now become "seen as" something new and different from what the accepted theory had taught him to anticipate: the scientist thus imposes a new order (or arrangement) of intelligibility onto the anomalous data. "Physical theories" he argues:

> provide patterns within which data appear intelligible. They constitute a 'conceptual Gestalt'. A theory is not pieced together from observed phenomena; it is rather what makes it possible to observe phenomena as being of a certain sort, and as related to other phenomena. Theories put phenomena into systems. They are built up 'in reverse' -- retroductively. A theory is a cluster of conclusions in search of a premise. From the observed properties of phenomena the physicist reasons his way toward a keystone idea from which the properties are explicable as a matter of course.[24]

In order to explicate this idea of a Gestalt-shift, which plays such a central role in Hanson's system as a whole (let alone anomalies and discovery), Hanson devotes an entire chapter to perception and observation which involve these Gestalt-shifts. Many of his basic examples are common objects of study in the psychological literature on 'perception'; and Hanson uses them quite effectively to demonstrate his point that the resolution of anomalies in science typically involves such conceptual shifts. In effect, Hanson is using a psychological phenomenon to explicate a conceptual point. Hanson cites Wolfgang Köhler's Gestalt Psychology[25] as a rich source of such examples. The famous drawing of the Goblet-and-Faces is one such example.

> In Kohler's drawing of the Goblet and Faces we 'take' the same retinal /cortical/ sense-datum picture of the configuration; our drawings might be indistinguishable. I see a goblet, however, and you see two men staring at one another. Do we see the same thing? Of course we do. But then again we do not. (The sense in which we do see the same thing begins to lose its philosophical interest).[26]

Another even older example is: —

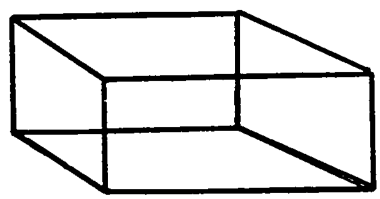

Fig. 1

Hanson says of this:

> Do we all see the same thing? Some will see a perspex cube viewed from below. Others will see it from above. Still others will see it as a kind of polygonally-cut gem. Some people see only criss-crossed lines in a plane. It may seem as a block of ice, an aquarium, a wire frame for a kite -- or any of a number of other things.[27]

And of the Bird-Antelope:

Fig. 2

Hanson says:

> . . . Here, some people could not see the figure as an antelope. Could people who had never seen an antelope, but only birds, see an antelope in fig. 2?[28]

Hanson insists that such problems are not simply "psychologist's tricks", but are problems at the "very frontier of observational science".[29] He uses these problems (or better, this phenomenon) to analyse the perceptual datum of scientific problems in the history of science.

> Let us consider Johannes Kepler: imagine him on a hill watching the dawn. With him is Tycho Brahe. Kepler regarded the sun as fixed: it was the earth that moved. But Tycho followed Ptolemy and Aristotle in this much at least: the earth was fixed and all other celestial bodies moved around it. Do Kepler and Tycho see the same thing in the east at dawn? . . . The same configuration is etched on Kepler's retina as on Tycho's.
>
> . . . People, not their eyes, see. Cameras, and eye-balls, are blind . . . That Kepler and Tycho do, or do not, see the same thing cannot be supported by reference to the physical states of their retinas, optic nerves or visual cortices; there is more to seeing than meets the eyeball.[30]

With several such historical examples, Hanson shows what he means by a "conceptual Gestalt", and how it figures into the resolution of anomalies which is, for him, the essence of discovery.[31] In one of his latest papers, Hanson summarizes this view of discovery put forth in Patterns of Discovery:

> In Patterns of Discovery 'explaining X' is represented as 'setting X into a conceptual framework'. Discovery is thus characterized as 'the dawning of an aspect of X' such that X is at last seen as part of a more comprehensive and comprehensible pattern; earlier, X might have been anomalous in seeming not to fit any intelligible organization of ideas. Now, the factual details of discovery constitute a subject matter for psychology -- wherein words like 'intuition', 'insight', 'hunch', 'in a flash', etc., are descriptively associated with the phenomenon to be investigated. But that such spectacular reorganizations of concepts do occur is a matter of profound epistemological importance. Patterns of Discovery traced some philosophical implications of such sudden coagula in the data of scientific perception. There, retroduction was remarked as the grounding discoverers give to individual anomalies -- thereby rendering them non-problematic, nonanomalous, explained.[32]

Perhaps one cautionary note about Hanson's view of discovery should be made clear here: While Hanson's general epistemological position on observation and perception takes the findings of Gestalt psychology quite seriously, his analysis of scientific discovery, as such, does not rest on such findings. Hanson's point about discovery is a conceptual point (cf. "conceptual Gestalt"), the perceptual examples were used only to explain, or illustrate, his position on scientific discovery.[33]

Before proceeding to the second point about discovery to be found in Patterns of Discovery (i.e., the more specific point about 'inference' mentioned earlier), it might be well to consider some questions about Hanson's view thus far, while it is freshly before us.

It is perhaps possible to raise many different objections to Hanson's view of discovery, but from the point of view of reconstructing a logic of discovery, there is one difficulty in particular, which, I think, deserves close attention. The difficulty issues from Hanson's view that discovery consists in the resolution of anomalies.

Before examining this difficulty, however, it is helpful to consider for a moment what it means to be confronted by a serious anomaly in science, or, in Kuhn's language a "crisis situation". It means that the investigator is confronted with data which do not conform to the physical laws which are, for the moment, regarded as true. As Hanson would put it, the data do not fit with the available conceptual scheme, or "Gestalt". A parallel situation in formal logic would be where a certain theorem (cf., the anomalous data) either: contradicts some other theorem or axiom within the system, or, may not be rich enough to express the desired relations in question (cf., the empirical laws of the theory). This implies that there is something amiss with the system, and the system itself needs to be revised or abandoned. In physics, for example, the anomaly would require a change in fundamental laws, thereby providing a new paradigm or "conceptual Gestalt". Hanson's view of discovery calls for just such conceptual changes as these; that is, discovery consists in resolving anomalies through a "Gestalt-shift" of this magnitude.

I should now like to point out what I think is the primary difficulty with this view of discovery in general, and Hanson's programme in particular.

Since "Gestalt-shifts" (for Hanson) consist in bringing a totally new perspective to bear on anomalies, it is extremely difficult to see how one (e.g., Hanson) could ever know in advance that a given logical system was going to be rich enough to handle all possible epistemological anomalies of the magnitude Hanson has in mind. His notions of "conceptual Gestalt" and "Gestalt-shifts" are, in effect, alternative frameworks, or models, for organizing the empirical data of our experience; and Hanson's corresponding logic (of discovery) would need to be able to anticipate transformationsfor all such alternative frameworks. One cannot always know today what sort of epistemological problems (i.e., anomalies) our theories might engender tomorrow (cf., Hume). Short of a transcendental deduction of some sort, it is difficult to see how anyone could claim that a given logical system will handle changes of this magnitude.[34]

Hanson does not discuss this problem, and it is difficult to guess what he would say. However, I have been suggesting that these difficulties are inherent in his view that discovery consists in the resolution of anomalies in "crisis situations". And they are, moreover, what motivates my suggestion (to be discussed later) to restrict the PLD to the domain of "normal (paradigm-based) science", where anomalies of Hanson's sort are not at issue.

So much then for the resolution of anomalies view which Hanson puts forth in Patterns of Discovery.

A second point in Patterns of Discovery, which is relevant to the task of constructing a logic, has to do with two particular patterns of reasoning which Hanson takes to be quite common in the history of science. Indeed, Hanson suggests[35] that it was examples such as these from the history of science which inspired his thinking about good reasons for suggesting hypotheses.

These two patterns of reasoning, from data to hypothesis, are not regarded by Hanson as logically distinct: they both involve the introduction of a new entity (e.g., space-occupying entities) to render the observed phenomenon intelligible. These two patterns will be treated separately here, however, because one of these "patterns of reasoning" has some interesting features which the other does not have. It would appear in fact, that one of these patterns would be a stronger candidate for logical reconstruction than the other.

Before examining these patterns, however, it is worth noting that neither of these involve the type of radical Gestalt-shift which Hanson had argued for earlier. In effect, what these two patterns amount to is the addition of a new entity into the older conceptual scheme; but there is no sense in which the old (or already available) conceptual scheme is thrown out and replaced by a new one. In these two patterns the old conceptual scheme, or paradigm, is extended to cover the new puzzles. Thus, these specific historical examples which Hanson regards as involving typical patterns of reasoning in discovery, are themselves counter-instances to his general view about conceptual (or "Gestalt") change.[36]

In classical celestial mechanics Hanson finds an example of reasoning to a hypothesis, which he regards as exemplary of a type of reasoning common in scientific discovery.[37] In 1846 V. J. Leverrier had accurately predicted the existence of a then unseen planet, Neptune. Using Newtonian mechanics as his theoretical model, when Leverrier observed the aberrations in the orbit of Uranus his first thought was not to regard the aberrations as falsifying Newtonian mechanics, but rather, he regarded them as an indication of more Newtonian forces at work, i.e., forces effecting the orbit of Uranus. Thus he postulated the existence of the unseen Neptune to account for the strange orbit of Uranus. In this case Leverrier was correct; and as Hanson remarks, this prediction "raised classical mechanics to its highest pinnacle".[38] Later, when Leverrier observed the precessions of Mercury at perihelion, he found himself in a similar situation as before, with Uranus. And again, as Hanson suggests: "aberrations in the perihelion of Mercury made Leverrier uncomfortable; but to have scrapped celestial mechanics then would have been to refuse to think about the planets at all".[39] Thus, Leverrier reasoned, as he had before, that there must be another planet, Vulcan, which accounts for the observed aberrations in the orbit of Mercury. Although this latter hypothesis turned out to be false (though not discovered until 1915 through Einstein's General Theory), Hanson points out that Leverrier's reasons for suggesting it were perfectly good ones. The perihelions of Mercury could be explained quite naturally if the hypothesis were true, i.e., if the described planet Vulcan existed.[40]

Consider, for a moment, an abbreviated form of the reasoning in this example: e consists of background information (e.g., statements of the known facts and theories of astronomy) and a statement of the fact that deviations in the orbit of Uranus were explained by postulating the existence of the planet Neptune; P is a statement describing the recalcitrant and puzzling phenomenon, the perihelions of Mercury. On the basis of e and P, it is reasonable to suggest H: a statement asserting the existence of a new planet (Vulcan) which stands in similar relations to Mercury as Neptune to Uranus. This form of reasoning is a development of the schematism taken from Peirce which Hanson considers so important. For instance, Hanson's schema might look like this:

e	(Background information)
P	(Puzzling phenomenon observed)
$\underline{H \supset P}$	(H could explain P as a matter of course)
H	(It is reasonable to suggest H)

The background information, e, is a necessary part of the explanans, because H alone does not explain the phenomenon, in any accepted sense of 'explain'. Thus, the recognition of the necessity to include background information is a step in the right direction. This collateral information may help to articulate (or unpack) the <u>relation</u> of 'reasonableness' which is alleged to exist between H and P.

In fact, however, Hanson always leaves this background information ill-defined, and only tacitly brings it in whenever his argument seems to require it. But leaving the set of background statements so ill-defined can only lead to difficulty in working out a logic of suggestion. A logic must focus on sentences which are stated explicitly; anything less leads to arbitrary choices by logicians, and thus trouble. Analogous difficulties would arise in deductive logic if logicians claimed that their theorems followed from axioms without specifying the rules of inference.

Although this particular shortcoming is serious, in that it fails to show how this pattern of reasoning employs a logic in any acceptable sense of 'logic', this type of analysis (i.e., one which makes use of background information) ought to be marked as the most viable candidate for a logic thus far.

Another pattern of reasoning, which Hanson regards as typical in some areas of scientific research, comes from modern elementary particle physics. Again, however, the pattern has to do with the reasoning involved in the introduction of "new entities". For example: Wolfgang Pauli's postulation of the neutrino (1923), Paul Dirac's postulation of the positron (identified by Anderson 1932), and Yukawa's postulation of mesons (1935), can all be considered as exemplars of this pattern which Hanson finds in modern particle physics. Because the entities in this area of investigation cannot be directly seen, or as Hanson says, they are "radically unpicturable", the evidence for them is necessarily indirect. Thus Hanson argues:

> If microphysical explanation is even to begin, it must presuppose theoretical entities endowed with just such a delicate and non-classical cluster of properties (e.g., unpicturable ones). In general, if A, B, and C, can be explained only by assuming some other phenomenon to have properties α , β and γ , then this is a good reason for taking this other phenomenon to possess α , β and γ. In macrophysics any such hypothesis is tested by looking at the other phenomenon to see if it has α , β , and γ . With elementary particles, however, we cannot simply look. All we have to go on are the large-scale phenomena A, B, and C (ionization tracks, bubble-trails, scintillations, etc.)...
>
> The cluster of properties α , β and γ may constitute an unpicturable conceptual entity

> to begin with. . . . This does not matter: . . . The whole story about fundamental particles is that they show themselves to have just those properties they must have in order to explain the larger-scale phenomena which require explanation.[41]
>
> (underlining is mine).

Hanson is here pointing out, and sanctioning, a form of reasoning which, on the surface at least, appears to be unduly ad hoc. The sole justification for the introduction of the new entity is that if it (the entity) did exist it would solve the problem. One is reminded of the Ptolemaic astronomers adding a new epicycle every time an observation did not fit with the theory. There are differences however: epicycles were a common, indeed integral, feature of the Ptolemaic theory, and could thus be expected to occur; whereas the introduction of new (and different) elementary particles could not be expected in advance, moreover, their presence comes as something of a shock to the existing theory. This is not to suggest that there is anything methodologically wrong with employing ad hoc hypotheses, indeed, I would argue that in "normal science" (to use Kuhn's phrase) it is often a reasonable thing to do. However, there are many different types of ad hoc hypotheses, and different reasons for introducing them, and not all these could be legitimately accommodated by one system in either science or logic.[42]

Despite these particular ad hoc features in the reasoning to hypotheses in elementary particle physics, it would be well to look at Hanson's discussion of the neutrino hypothesis in particular, since this is his most detailed discussion. He says of this that "The formation of the 'neutrino concept' provides a paradigm example of how observation and theory, physics and mathematics, have been laced together in physical explanation".[43] He quotes Fermi stating that: "The existence of the neutrino has been suggested . . . as an alternative to the apparent lack of conservation of energy in beta disintegrations. It is neutral. Its mass appears to be either zero or extremely small . . . etc."[44] And then Hanson comments on this, saying:

> Our concepts of the properties of the neutrino are determined by there being gross phenomena A, B, C, which defy explanation unless an entity exists having the properties α, β and γ -- just those which the neutrino has. The neutrino idea, like those of other atomic particles, is a retroductive conceptual construction out of what we observe in the large;" . . .[45]

Thus, Hanson envisages the introduction of new elementary particle hypotheses as fitting into the <u>retroductive schema</u>, for example: some anomalous phenomenon is encountered, P, which in this case is the apparent lack of energy conservation in beta disintegration; if neutrinos existed, H, this phenomenon would be explained as <u>a matter of course</u>, $H \supset P$; therefore it is reasonable to suggest H.

Hanson's specific argument to the effect that the neutrino hypothesis followed this pattern, and that it was the reasonable thing to suggest (e.g., retroductively suggest) is as follows:

> Accept the hypothesis (of Pauli): with every β particle another particle also leaves the nucleus, carrying the difference in energy. If this particle is construed (following Fermi) as having the properties: velocity c, hence mass = 0 and in no case greater than 1/500th an electron mass, charge neutral, magnetic moment = 0 (or very small), then the continuous spectrum of the β-ray will be explicable as a matter of course, and the energy principle still holds.
>
> Yes, but why accept this concept of the neutrino? It cannot be observed in the Wilson chamber, nor has it ever been directly detected by any other means.

> Besides, such a particle seems unlikely and unsettling. So why accept the neutrino?
>
> Because if you do, the continuous β-ray spectrum will be explained as a matter of course, and the energy principle will remain intact. What could be a better reason?[46]

This, Hanson suggests "is a paradigm example" of how elementary particle hypotheses are arrived at.

There are two points to be noted about this example which are particularly worthy of attention. First, Hanson is claiming here that the neutrino hypothesis was arrived at through a type of reasoning which is quite common (if not characteristic) of the reasoning done in particle physics in general. Thus, the postulation of a new entity (e.g., the neutrino) is not to be regarded as an unusual _ad hoc_ pattern of reasoning, but rather, it is a _typical_ pattern of reasoning in this area of investigation. Hanson is suggesting, in effect, that there is a _method_, however loosely defined, for reasoning toward hypotheses in sub-atomic particle physics. Hanson's suggestion comports with the general distinction, introduced earlier, between "having a method" of discovery, and "having a logic" of discovery. At most, Hanson is pointing toward a _method_ of discovery (for particle physics), and has said nothing here about a _logic_, as such, of discovery. However, it should be noted that, at least in my view, rational _methods_ are the sorts of things which might eventually become candidates for a logic of discovery in the required sense.

The second point I wish to call attention to is the fact that Hanson's example is not that of superimposing a whole new "conceptual Gestalt" on the data, but rather, it is an example of extending the old (available) Gestalt to cover the puzzling phenomenon (e.g., the apparent lack of conservation of energy in beta disintegrations). It is important to recognize that the existing theory, together with its backlog of information, played an integral part in the reasoning to the hypothesis. More generally, these background conditions have a definite influence on the method of reasoning used to suggest a hypothesis, and I shall argue (in chapter V) that within each discipline, these conditions provide the general parameters for a logic of discovery.

What has preceded in this section are the major points about discovery which Hanson made in Patterns of Discovery. In retrospect, it is fair to say that Hanson has in fact pointed to the existence of certain recurrent patterns of reasoning in the formation of scientific hypotheses. Indeed, in these patterns, in particular the ones which bring in relevant 'background information', Hanson has made a clear improvement over Peirce's rather simple schema. In general, however, these patterns of reasoning exist as little more than that -- patterns. And an existing pattern in X does not amount to a 'logic of X'. But where there exists a pattern, there is, correspondingly, more reason for optimism about a logic.

3. 'Logic of Discovery' à la Is There a Logic of Discovery

A second, and completely different, line of argument which Hanson devoted considerable attention to, concerned the question of whether the patterns of reasoning involved in discovery constitute a unique and distinguishable kind of inference. Peirce, it will be recalled, argued that retroduction was a third type of inference, as distinct from induction and deduction. However, this question was not directly raised by Hanson in Patterns of Discovery in any detail. In several of Hanson's later papers this question occupies the center of attention.[47]

Generally speaking, Hanson wanted to distinguish the 'logic of discovery' from the 'logic of justification' by distinguishing between:

(1) reasons for accepting a hypothesis H, from

(2) reasons for suggesting H in the first place.

This distinction does capture the different areas of concern between the two contexts, i.e., 'discovery' and 'justification'. However, in a view which Hanson attributes to Herbert Feigl,[48] it is denied that there is any logical difference between these two sets of reasons. Thus, Hanson wanted to refine the distinction so that the differences could be brought into sharper focus. For example:

> What would be our reasons for accepting H? These will be those we might have for thinking H true. But the reasons for suggesting H originally, or for formulating H in one way rather than another, may not be those one requires before thinking H true. They are, rather, those reasons which make H a plausible type of conjecture. Now, no one will deny some differences between what is required to show H true, and what is required for deciding H constitutes a plausible kind of conjecture. The question is: Are these logical in nature, or should they more properly be called "psychological" or "sociological"?[49]

Feigl's answer to Hanson's question is particularly interesting because it denies that the differences are logical ones, while at the same time (Feigl's view) does not imply that the differences are psychological or sociological. In effect, Feigl's answer denies the narrow dichotomy set up in Hanson's question. Let us look, then at Feigl's view, and then examine Hanson's responses to it. Hanson says:

> Or one might urge, as does Professor Feigl, that the difference (in reasoning) is just one of refinement, degree, and intensity. Feigl argues that considerations which settle whether H constitutes

> a plausible conjecture are of the same type as those which settle whether H is true. But since the initial proposal of a hypothesis is a groping affair, involving guess-work amongst sparse data, there is a distinction to be drawn; but this, Feigl urges, concerns two ends of a spectrum, ranging all the way from inadequate and badly selected data to that which is abundant, well-diversified, and buttressed by a battery of established theories.
>
> . . . Insofar as scientists have reasons for formulating types of hypotheses (as opposed to hunches and intuitions), these are just the kinds of reasons which later show a particular H to be true . . . (This) has no logical import for the differences between proposing and establishing hypotheses.[50]

This is a strong argument (of Feigl's) and it must be faced if Hanson wants to maintain his view that retroduction, and the logic of discovery generally, involves a unique type of inference. (While Peirce did offer such arguments, Hanson tries to construct his own, independently of Peirce).

Thus, Hanson refines his initial distinction to make his point clearer. He says that the original distinction (of his) had been stated in an "objectionable way", and that it "must be reset in the following, more guarded language. Distinguish now:

(1') reasons for accepting a particular, minutely specified hypothesis H, from

(2') reasons for suggesting that, whatever specific claim the successful H will make, it will, nonetheless, be a hypothesis of one kind rather than another".[51]

Hanson now places the emphasis for the distinction on the suggesting of hypothesis kinds, as opposed to reasons for suggesting the particular H that will eventually be successful. That is, a logic of discovery is concerned with the reasons a scientist has for suggesting types of hypotheses, whereas the logic of confirmation is concerned with the reasons a scientist has for accepting a particular (single) hypothesis as true.

As Hanson says "The issue is whether, before having hit a hypothesis which succeeds in its predictions, one can have good reasons for anticipating that the hypothesis will be one of some particular kind".[52] In order to support his distinction that these two sets of reasons are of different kinds, Hanson, as usual, illustrates his point with examples from the history of science.

> Could Kepler, for example, have had good reasons, before his elliptical-orbit hypothesis was established, for supposing that the successful hypothesis concerning Mars' orbit would be of the noncircular kind? He could have argued that, whatever path the planet did describe, it would be a closed, smoothly curving, plane geometrical figure. Only this kind of hypothesis could entail such observation-statements as that Mars' apparent velocities at 90 degrees and at 270 degrees of eccentric anomaly were greater than any circular-type H could explain. Other kinds of hypotheses were available to Kepler: for example, that Mars' color is responsible for its high velocities, or that the dispositions of Jupiter's moons are responsible. But these would not have struck Kepler as capable of explaining such surprising phenomena. Indeed, he would have thought it unreasonable to develop such hypotheses at all, and would have argued thus.[53]

But Hanson's interlocuter, in this case Feigl, could still respond by saying: Yes, Kepler could have had good reasons for his elliptical hypothesis, but "logically Kepler's reasons for entertaining a type of Martian motion other than uniformly circular were his reasons for accepting that as astronomical truth . . .

> Even after other inductive reasons confirmed the truth of the latter hypothesis, these early reasons were still reasons for accepting H as true. So they cannot have been reasons merely for proposing which types of hypothesis H would be, and nothing more".[54]

Hanson counters this objection, indeed, the whole Feigelian position, by pointing out that the reasons used in suggesting _types_ of hypotheses employ _analogical_ thinking, whereas confirmation does not. For example, Leverrier's hypothesis than an unseen planet Vulcan explained the abberrations in Mercury's orbit was essentially an analogy from the Uranus/Neptune hypothesis. Also, Kepler later (in the _Harmonices Mundi_) hypothesized that the orbit of Jupiter was also noncircular, call this "H'". Hanson says:

> The reasons which led Kepler to formulate H' were many. But they included this: that H (the hypothesis that Mars' orbit is elliptical) is true. . . . But such reasons would not establish H'. Because what makes it reasonable to anticipate that H' will be of a certain type is _analogical_ in character. (Mars does X; Mars is a typical planet; so perhaps all planets do the same kind of thing as X). Analogies cannot establish hypotheses, not even _kinds_ of hypotheses. Only observations can do that.

Hanson continues:

> Nor is it right to characterize this differnece between 'H-as-illustrative-of-a-type-of hypothesis' and 'H-as-empirically established' as a difference of psychology only. Logically, Kepler's analogical reasons for proposing that H' would be of a certain type were good reasons. But logically, they would not then have been good reasons for asserting the truth of a specific value for H' -- something which could be done only years later.[55]

In short, Hanson attempts to drive a wedge between the two types of reasoning, and _a fortiori_ into Feigl's spectrum-type view, by arguing that the one form of reasoning employs analogies and the other does not. This is sufficient, Hanson argues, to distinguish the two types of reasoning as _different types_, and not merely differences in _degree_ depending on the amounts of evidence available (as Feigl argues).

Hanson poses two plausible objections to his own view, and answers each in kind. First:

> An objection: "Analogical arguments, and those based on the recognition of formal symmetries, are used because of inductively established beliefs in the reliability of arguments of that type. So, the cash value of such appeal ultimately collapses into just those accounts given by H-D theorists".
>
> Agreed. But we are not discussing the genesis of our faith in these types of arguments, only the logic of the arguments themselves. Given an analogical premise, or one based on symmetry considerations -- or even on enumeration of particulars -- one argues from these in logically different ways. Consider what further moves are necessary to convince one who doubted such arguments. A challenge to "All A's are B's" when this is based on induction by enumeration could only be a challenge to justify induction, or at least to show that the particulars are being correctly described. This is inappropriate when the arguments rest on analogies or on the recognition of formal symmetries.[56]

The second objection Hanson poses to his view is:

> "Analogical reasons, and those based on symmetry, are still reasons for H even after it is (inductively) established. They are reasons both for proposing that H will be of a certain type and for accepting H".
>
> Agreed again. But, analogical and symmetry-arguments could never by themselves establish particular H's. They can only make it plausible to suggest that H (when discovered) will be of a certain type. However, inductive arguments can, by themselves, establish particular hypotheses. So they differ from arguments of the analogical or symmetrical sort.[57]

Actually, the argument could go still further even though it is not pursued by Hanson. Perhaps, what Hanson has in mind here could be expressed as follows. In induction by enumeration, analogies (or analogy statements) are used evidently, much like a single premise in a long chain of argument; however, in analogical arguments, analogies (or analogy statements) function like a rule of inference (not a premise, as such) which justify making transformations (or inferences) between two states of affairs. It is important to understand that the nature of this justification (i.e., that analogies are used as rules) resides in the fact that analogical arguments are used to engender some initial plausibility (for a hypothesis) only, and in no way does it attempt to establish the truth, or posterior probability, of a hypothesis as does induction by enumeration.

A stronger reply to Feigl's position, that the differences in reasoning are only in degree and not kind, would have been to suggest that what follows from this is that either: both contexts do employ a logic (in which case there is a logic of discovery as well), or that both do not employ a logic, in which case confirmation is without a logic also. This latter consequence would certainly be rejected by most H-D theorists, Feigl among them. And this disjunction is implied by Feigl's view. It is surprising that Hanson did not choose to attack Feigl in this way, since in an earlier work he (Hanson) once said:

> They (H-D theorists) are wrong. If establishing an hypothesis through its predictions has a logic, so has the conceiving of an hypothesis.[58]

My point against Feigl is that the converse of this would also appear to be true, or that neither is true.

Perhaps a still sounder approach to this whole question, however, would be to follow a suggestion made by Peirce. Peirce had suggested (cf. Chapter III) that logics should be distinguished and evaluated on the basis of what they are designed to do, or accomplish. In 'discovery' the desideratum is to suggest reasonable hypotheses prior to testing, whereas in 'justification' the desideratum is to confirm the truth of hypotheses once put forward. And since the respective end products are so different, there is every reason to believe that their respective logics will have some elements that are not common among them.

In the section to follow, this question about the sameness or difference in logics comes up again in a slightly different setting. And one of the first things to be noted there is that Hanson changes his position on this question from what he was arguing for (this is one of several contradictions to be found in Hanson's work on the problem). However, I shall offer an additional argument (against Hanson's later view) as to why the logics in the two contexts should be considered distinct - just as Hanson had been arguing here.

4. 'Logic of Discovery' à la Retroductive Inference.

The third and last line of argument, which Hanson left to future proponents of a logic of discovery, has to do with his attempt at a formal reconstruction of a schematism which might capture retroduction (as such) as a *bona fide* form of inference.

Before looking at Hanson's specific argument in this section, one preliminary observation is in order. In general, Hanson's argument in this section, one preliminary observation is in order. In general, Hanson's argument here takes the view that the logic in both the H-D account and the *retroductive* (R-D) account is *identical*, and that it is only the conceptual setting (or beginning points) which are different. However, compare this present point of view with his position taken against Feigl in the previous section: there he was at pains to show that the two forms of reasoning are logically *distinct*. Recall, for example, Hanson saying:

> Logically, Kepler's analogical reasons for proposing that H' would be of a certain type were good reasons. But, logically they would not then have been good reasons for asserting the truth of a specific value for H'.[59]

And even more revealingly:

> Given an analogical premise, or one based on symmetry considerations -- or even on enumeration of particulars -- one argues from these in logically different ways.[60]

And again:

> But, analogical and symmetry arguments could never by themselves establish particular H's. . . . However, inductive arguments can, by themselves, establish particular hypotheses.[61]

These statements, which were used against Feigl's position, contradict the point of view Hanson advances in this section, i.e., that the 'logic' of confirmation and discovery are identical -- though their "conceptual development is different in each context."[62]

I should like to make it clear that I am in general disagreement with one of the main views Hanson advances in this section (i.e., about the logics being identical); however, I include this discussion here not only for the purpose of historical thoroughness, but also because some of the other points are illuminating.

On the basis of his previous arguments (e.g., his earlier work) Hanson proceeds with the task of reconstructing what he takes to be the 'logic' (as such) of discovery; and to these theorists who continue to hold the "received opinion" that such a logic is impossible, Hanson summarizes his general position thus far:

> Now surely the scientist uses his head in these problem-solving, anomaly-explaining contexts! He reasons! And reasoning has some structure -- it moves from stage to stage; this, even before a conclusion is reached and tested.

> Who would argue that reasoning only occurs when one chooses to review the finished argument, that one which terminates finally in a fully tested conclusion? No one should want thus to argue, for it is obviously untrue. But those who exclude logical analysis from all stages of research-inquiry save that where the subject matter is the finished research report -- they are perilously close to being in just such a blame-worthy position.[63]

The general tact which Hanson takes in schematizing retroduction, reminds one of the two sorts of problems which can be solved by employing Boolean Normal Forms. For example, if a student is given a statement-formula in first-order predicate calculus, he may be asked to reduce this formula to a Boolean Normal Form, or, conversely, given a Boolean Normal Form, the student may be asked to find a statement-formula which would have the given Normal Form as its Boolean expression. In the former case the student reduces a complex statement-formula to a simpler expression (i.e., a Boolean one) while in the latter case the task is to find a complex statement-formula reasoning from the simple Normal Form. This is comparable to 'data-rich' versus 'data-poor' situations. It is noteworthy, however, that in the case of working with Boolean Normal Forms there exists a decision method (or algorithm) for performing both of these tasks; whereas in Hanson's account of the deductive vs. retroductive distinction, a corresponding method (or algorithm) is anything but clear. Nonetheless, Hanson attempts to make the case that this sort of parallel is more appropriate to the logic of discovery problem than has been recognized heretofore. Indeed, Hanson uses a similar, though looser, example to illustrate his general tactic in this direction.

> Consider a logic teacher presenting a problem to his class. One orthodox assignment might be this: "Here are three premises: A, B, and C. From these alone generate the theorem D". The teacher is here charging his students to find what follows from premises written 'at the top of the page'. This is related to the traveler's puzzlement when he asks, "Here I am, river to the left, mountains to the right, canyon ahead, blizzard to the rear; where do I go from here?"
>
> Contrast this with the quite different assignment a logic teacher might give: "Here is a theorem D. Find any three premises, A, B, and C, from which D is generable." Here he gives D to his students 'written at the bottom of the page', as it were. He requires them to work back from this to three premises which, if they were written at the top of the page, would be a 'that from which' D follows. Analogously, the traveler's question would be "Would I be able to return to here from over there? or over there?"[64]

It is clear that in the former case (i.e., arguing from A, B, and C to D) there is both a logical and geographical route connecting the premises with the conclusion. The crucial question which Hanson needs to answer is whether or not there are similar routes connecting the designata in the latter case. As one might rightly expect by now, Hanson (bravely) argues that there are such routes.

To argue this case, Hanson suggest that the logical apparatus used to assess retroductive (RD) arguments is the same as that used to assess hypothetico-deductive (HD) arguments. Thus, his argument goes, there are no logical reasons for affirming a logic in the one case while denying it for the other. The differences, Hanson argues, are "conceptual", not logical differences. (This is where Hanson is explicitly contradicting his arguments against Feigl). He compares the two cases thus:

> HD and RD proponents both recognize that their formal criteria for success in argument are precisely the same. In this Peirce was no less acute than Popper. Thus, imagine that one scientist argues from premises A, B, C and an hypothesis H, to a conclusion D (which, although perhaps originally unexpected, is confirmed in fact). Another scientist experimentally encounters the anomalous fact that D, and conjoins this statement with A, B, and C, so as to 'corner' an hypothesis H which, when bracketed with A, B, and C, will possibly 'explain' D. Both scientists have been arguing; both have been using their brains. Differently!
>
> But the criterion for their having *succeeded* with their different tasks will be just thus: *that D follows from A, B, C and H*. If either the first, or the second, scientist was mistaken in thinking D to be entailed by A, B, C and H, then his reasoning fails.[65]

The only differences in the two cases are, according to Hanson, the conceptual starting-places and the finished *desiderata*: but the logical connections are allegedly the same. He asks us to consider these two schemata:

HD characterization
A,B,C + H
initial conditions plus
hypothesis

D_1 D_2 D_3 . . .
observation statements --
as yet untested in some cases

RD characterization
D_1 D_2 D_3 . . .
anomalies, incompatible
with HD 'unpacking' of
orthodox H's

A,B,C + H_1' or H_2 or H_3 or
established conditions of inquiry
plus possible explanatory hypotheses

Hanson says of these that:

> *The solid arrows represent the actual order* of the scientist's argument -- the way in which he does, in fact, wend his way from the beginning to the end of his problem . . . *The dotted arrows*, however, *represent* the *logical order of the progressions*. In both characterizations the dotted arrows have the same sense -- 'they point the same way in the logical space', towards the particulars D_1, D_2, D_3. Hence the logical criteria for appraising the validity of arguments in either form are identical.[66]

This, in brief, is the approach Hanson takes in defending his view that a logic exists in both contexts, even though the conceptual problems in struggling toward their respective solutions are different.

Question: Are we to infer then that the logic of discovery is the same as the logic of confirmation, according to Hanson? As I shall attempt to show presently, Hanson's views on this question are confused at best. What appeared to be clear to him in his debate with Feigl, now becomes confused in his discussion about retroduction. Before explicating Hanson's views on the status of retroduction as a form of logic, I would like to state, at the outset, what I think Hanson's basic confusions are in this section.

First, it does not follow, as Hanson suggests it does, that from "the same criteria for appraising arguments" the logics are therefore "identical", this is a simple non sequitur; at best, such an argument might show that both contexts employ logic (of some kind) but not that they employ the same logic. And secondly, I believe him to be mistaken in holding the view that the difference between "discovery from facts" (i.e., the RD account) versus "confirmation by facts" marks only an epistemological difference, or as he says, only a "difference in conceptual starting places".

The two schemata (previous page) of the HD and RD accounts, is Hanson's way of attempting to show that although the "conceptual starting places" are different (in a problem-solving situation) the logic employed is fundamentally the same. However, it should be noticed that from the point of view of hypothesis-generation, which is what the PLD attempts to elucidate, the "actual order of the scientist's argument" (the solid line) is the crucial point at issue i.e., it is the method of reasoning to a hypothesis which requires reconstruction. Moreover, even if one stays within Hanson's retroductive framework, it can be shown that the reasonings employed in both contexts are logically distinct. Consider, for example, when an HD proponent is arguing from the hypothesis H, together with initial conditions A, B, and C. In this case all the derived 'D' statements (i.e., observation statements) will be, nay must be, mutually consistent with one another because they are, in effect, theorems. However, when an RD proponent is arguing from his anomalous 'D' statements (e.g., recalcitrant observations) to possible H's e.g., H_1 or H_2 or H_3 . . . H_n, these H's need not be mutually consistent. For example, H_1 could be "neutrinos exist", while H_3 could entail, or actually be, "neutrinos do not exist": both of which could be reasonable suggestions based on the same anomalies. Hanson does not seem to recognize that this consideration is sufficient to mark the two forms of reasoning as logically distinct. What Hanson has taken to be a mere "conceptual difference" or difference in starting-points vis-a-vis the problem-solving situation, is actually (in fact) a bona fide logical difference. It can easily be shown that the features which distinguish these two systems (i.e., HD and RD) are sufficient to distinguish them as radically different logics. Where the HD account never sanctions the derivability of mutually incompatible theorems, the RD account does: in short, the HD account recognizes (and subscribes to) the principle of non-contradiction and the RD account does not. Therefore they are logically different.[67]

It follows from this that Hanson really had a stronger reply available when he was defending the "logical distinction" (between the two contexts) against Feigl's 'spectrum-view'.

Aside from any particular differences in the way one chooses to distinguish retroduction, the important point is to come to some assessment of retroduction as a form of reasoning in problem-solving situations. I would now like to point out what I think is the major shortcoming with retroduction as a candidate for a logic of discovery.

The difficulty, or oddness, of retroduction does not necessarily lie in the fact that it allows mutually incompatible suggestion (i.e., H's), since many inductive systems can give identical probability values to two mutually exclusive events (e.g., the value 0.5 to the flipping of a fair coin). And, as Peirce said, retroduction is more akin to a form of _induction_ than to _deduction_. Rather, the difficulty with retroduction resides in the fact that it still sanctions _too many different kinds of H's_. Again, as was pointed out in the discussion of Peirce, the class of all possible H's which could explain the data is not equivalent to the class of plausible explanations. That a given hypothesis H _could explain_ the data, in every sense of 'could explain', is simply too loose and non-specific to provide an adequate foundation for _logical_, (i.e., formal), clarification and rigour. If the condition that "a hypothesis must be able to explain the data" be construed as a kind of inference rule, or even a condition for an inference rule, for retroduction, then it simply will not do the job of distinguishing _plausible_ H's from the infinite class of _possible_ ones. Consider Hanson's class-room example again of the beleaguered traveler in the situation where he says: "Here I am, river to the left, mountains to the right, canyon ahead, blizzard to the rear; _where do I go from here_?"[68] If the traveler employed strict _retroductive_ reasoning to the letter, then the suggestion that he fly out on the wings of a giant chickadee would be as legitimate as any other suggestion which he might have arrived at through the same retroductive process.

Plausible suggestions, like plausible explanations, require some form of clearly articulated formal, pragmatic, and material criteria (or conditions) before they can be considered compelling. Retroduction, in particular, has a considerable way to go along these lines before it can be considered an adequate reconstruction of the process of discovery. This is not to suggest that the retroductive approach to the logic of discovery problem is off on the wrong foot, I think it is an excellent beginning, but it remains just that -- a beginning. All earlier accounts of retroduction (i.e., Peirce's and Hanson's) have been inadequate and incomplete for the same sorts of reasons, eventhough Hanson's discussions have done much to clarify and improve this notion. In particular, Hanson's earlier discussions about <u>patterns of reasoning</u> within disciplines, and the role existing theories play in suggesting plausible hypotheses, are particularly rich with suggestive insight.

Perhaps the most fruitful avenue to investigate next would be to pursue these "patterns of reasoning" with an eye toward a more formal reconstruction of them. This would not only bring out any latent <u>method</u> of discovery which might exist in a given discipline, but it would (if successful) provide the subject matter for raising specific questions about a logic (as such) of discovery. The discussion which follows will therefore be directed towards that end.

FOOTNOTES

1 The Logic of Scientific Discovery (New York, 1959), pp. 33-34.

2 See, for example, his early works: "The Logic of Discovery", Journal of Philosophy, 55 (1958), 1073-89; Patterns of Discovery (Cambridge, 1958); "Good Induction Reasons", The Phil. Quarterly, II (1961), 123-34; "Is There a Logic of Scientific Discovery", Current Issues in the Philosophy of Science, (eds.) H. Feigl and G. Maxwell (New York, 1961); etc.

3 It is interesting to note, in this connection, that when Hanson taught at Indiana University he started a new academic department, a "History and Philosophy of Science Department"; the conjunction of these two research areas has continued to gain wider acceptance in academic circles ever since. In his introduction to Patterns of Discovery Hanson says, "This comports with my conception of philosophy of science: namely, that profitable philosophical discussion of any science depends on a thorough familiarity with its history and its present state."

4 "Notes Toward a Logic of Discovery", Perspectives on Peirce, R. J. Bernstein, ed., (Yale, 1965).

5 "Is There a Logic of Scientific Discovery", op. cit., p. 20.

6 "An Anatomy of Discovery", Journal of Philosophy, II, June 8, 1967.

7 For example, in his paper "Notes Toward a Logic of Discovery", op. cit., Hanson attempts to develop a formal notation which he thought might capture discovery; but in "An Anatomy of Discovery", op. cit., he analyzes the various types of meanings of the concept of 'discovery' in science; and in Patterns of Discovery, op. cit., still another approach is to show the "rationality" in various patterns of reasoning which led to scientific discoveries.

8 "Notes Toward a Logic of Discovery", op. cit., p. 47. Each of the three themes which Hanson mentions here continued to appear, with various modifications, in different papers. In fact, Hanson frequently confused these points to the extent that it is not always clear which one he is actually arguing for.

9 Throughout the book, for example, Hanson stresses the dependence of seeing upon theory; seeing, in science, is always "seeing-as". He says ". . . seeing is a 'theory-laden' undertaking. Observation of X is shaped by prior knowledge of X". The visitor to the physicist's laboratory "must learn some physics before he can see what the physicist sees . . . ", pp. 17-19.

10 *Ibid.*, p. 2.

11 *Ibid.*, p. 2.

12 *Ibid.*, p. 3.

13 *Ibid.*, pp. 70-71.

14 With the obvious exception, of course, as to the issue of whether there is a *logic* to the discovery of Hypotheses.

15 *Perception and Discovery*, E. C. Humphreys, ed., (San Francisco, 1969), p. 15.

16 *Op. cit.*, pp. 72-73.

17 *Op. cit.*, p. 358.

18 "Notes Toward a Logic of Discovery", *op. cit.*, p. 51.

19 *The Structure of Scientific Revolutions* (Chicago, 1962), pp. 3-19.

20 *Patterns of Discovery*, *op. cit.*, p. 119.

21 "Notes Toward a Logic of Discovery", *op. cit.*, p. 51.

22 *Ibid.*, p. 50.

23 *Patterns of Discovery*, op. cit., p. 90.

24 *Ibid.*, p. 90.
This "conceptual Gestalt" thesis is also the foundation of Hanson's epistemological position that "facts" and scientific observation are "theory-laden".

25 Kohler, *Gestalt Psychology* (London, 1929); also *Dynamics in Psychology* (London, 1939).

26 *Patterns of Discovery*, op. cit., p. 12.

27 *Ibid.*, p. 9.

28 *Ibid.*, p. 13.

29 *Ibid.*, p. 179.

30 *Ibid.*, pp. 6-7.

31 At Indiana University there was a popular "Hansonism" which graduate students were fond of quoting: "Never turn up your nose at a contradiction, because its negation is always true!" The meaning of which was that one might then be on the verge of discovering something important.

32 "Notes Toward a Logic of Discovery", op. cit., p. 48.

33 It might be objected that the distinction between a "perceptual point" (vis-a-vis Gestalt psychology) and a "conceptual point" is very tenuous; no matter, Hanson might be the first to agree. The strongest view, and the one most philosophically interesting, is the conceptual view of discovery; thus it will be the only one of concern here.

34 It should be noticed, in fairness to Hanson, that the phenomenon of being confronted by anomalous empirical data, is an epistemological, not a logical problem. Thus, there are no logical reasons prohibiting the construction of a logic of discovery which facilitate the shift from one Gestalt to another in order to resolve the anomaly. That is, it is logically possible to have a logical system which systematizes (at least some) transformations from one anomalous conceptual-Gestalt to another which is anomaly free. However, it must be recognized that de facto Hanson has not provided us with such a logical system, nor has he hinted at what such a system would look like.

There is some textual evidence in Patterns of Discovery to support an alternative interpretation of Hanson's position with respect to the resolution of anomalies. For example, one might interpret Hanson as holding the view that each "conceptual-Gestalt" carries with it a logic of its own, and peculiar to it (i.e., the logic is embedded in the Gestalt system); thus, on this interpretation, anomalies would be resolved by appealing to yet another (meta) system. However, on this interpretation Hanson would have even worse difficulties, since genuine anomalies would never arise unless there was something amiss in the meta-system itself, and this possibility opens the door to an infinite regress, whereby he would need a super-system to systematize changes from the meta-system, etc. etc.

35 "A Philosopher's Philosopher of Science", (Review of C. G. Hempel's Aspects of Scientific Explanation) Science, 152 (1966), p. 192-3.

36 Some other examples, however, are Kepler's elliptical hypothesis, and Einstein's General theory which do involve significant conceptual changes of the sort Hanson had envisaged. A non-circular planetary motion was alien to every paradigm prior to Kepler, and the constancy of the speed of light as a new primitive term was even more revolutionary in physics.

37 Cf. N. R. Hanson, "Leverrier: The Zenith and Nadir of Newtonian Mechanics", Isis, 53 (1962), p. 359-78.

38 Patterns of Discovery, p. 203-204.

39 *Ibid.*, p. 103.

40 Incidentally, a similar pattern of reasoning had been used to predict the ninth planet, Pluto, to explain the perturbations of Neptune, even though Pluto was not actually observed until 1930 by Clyde Tombaugh in California. See C. J. Schneer, *The Evolution of Physical Science* (New York, 1960), pp. 122-23.

41 *Patterns of Discovery*, pp. 123-24.

42 A more lengthy discussion of *ad hoc* hypothese is offered in chapter V of this essay.

43 *Patterns of Discovery*, p. 125.

44 *Ibid.*, p. 124.

45 *Ibid.*, p. 125.

46 *Ibid.*, p. 126.

47 See for example: "Is There a Logic of Discovery", *op cit*.; "The Idea of a Logic of Discovery", *op. cit*.,; and "Notes Toward a Logic of Discovery", *op. cit*.

48 Hanson does not cite a specific published reference where this view is put forward by Feigl; but in three separate papers (see previous footnote) Hanson continues to refer to "Feigl's view". I have not been able to trace down this view in Feigl's published work, but for present purposes it matters little since the view is an interesting one in its own right; thus I shall follow Hanson in referring to it as "Feigl's view", eventhough Feigl may have stated it differently.

49 "Is There a Logic of Discovery", *op. cit*., p. 22.

50 *Ibid.*, p. 22.

51 *Ibid.*, p. 22.

52 *Ibid.*, p. 23.

53 *Ibid.*, p. 23. In his later papers Hanson develops many other examples which illustrate this general point e.g., Newton's gravitation, Dirac's positron, etc.

54 *Ibid.*, p. 24.

55 *Ibid.*, p. 25.

56 *Ibid.*, pp. 26-27.

57 *Ibid*., p. 27

58 "Patterns of Discovery", *op. cit*., p. 71.

59 *Op. cit*., p. 22.

60 *Ibid*., p. 25.

61 *Ibid*., p. 26.

62 *Ibid*., p. 27. Incidentally, it should be noticed that the present essay (itself) has never taken a stand on the question of whether or not the *retroductive approach* is the best one for solving the problems in the logic of discovery. My own views on this will become clear later. The extensive treatment which 'retroduction' has received here is due primarily to the fact that the history and development of the problem, through the work of Peirce and Hanson, has followed this avenue.

63 "Notes Toward a Logic of Discovery", *op. cit*., p. 54.

64 *Ibid*., p. 55.

65 *Ibid*., p. 54.

66 *Ibid*., p. 60.

67 Hanson nowhere expresses what he means by the term 'logic'; however, I would suggest that this particular confusion on Hanson's part issues from a vague, if not ambiguous, conception of 'logic'. Long ago Peirce has said: "it (retroduction) is very little hampered by logical rules nevertheless it is logical inference ... having a perfectly definite logical form". *Collected Papers*, *op. cit*., (5.188). Clearly such a statement is in need of clarification and development, but neither Peirce nor Hanson bothered to do so. This, I would suggest, is where all the *logical* difficulties with *retroduction* begin; and Hanson's confusion here is just one instance of it.

68 *Op. cit*., p. 55.

CHAPTER V

SCIENTIFIC PARADIGMS AS PRESUPPOSITIONS FOR A LOGIC OF DISCOVERY

> To ask questions which you see no prospect of answering is the fundamental sin in science, like giving orders which you do not think will be obeyed in politics, or praying for what you do not think God will give you in Religion. Question and evidence . . . are correlative.
>
> R. G. Collingwood[1]

. Problems and Solutions

Earlier chapters of this essay have attempted to do two things: (1) to isolate what is really at issue in the problem of the logic of discovery, as distinct from the logic of confirmation; and (2) to examine and evaluate the views of some major proponents of such a logic. In the present chapter, I propose to introduce a single conception which helps to clarify much that has been lacking in the earlier attempts at reconstructing a logic, and which is also presupposed by most contemporary approaches to reconstructing a logic of scientific discovery. The conception which will be introduced to this problem represents a new and promising approach to the problem of the logic of discovery. In brief, the conception is an amended (or altered) version of a "scientific paradigm" which was introduced by Kuhn in The Structure of Scientific Revolutions.[2] Although the conception of a "paradigm" is now quite well known in the philosophy of science, the heuristic value in analyzing the problem through this notion has not been adequately recognized. Kuhn himself has said that he was not aware, nor had he seriously considered the question, of whether his work implied anything about the logic, as such, of scientific discovery.[3] However, when the logic of discovery problem is considered from the perspective of scientific paradigms, much becomes clear about the problem that had hitherto been either overlooked or obscured. This chapter will offer a fresh approach to the problem of the logic of discovery by bringing together the notions of *discovery* and *paradigm*.

In particular, the remaining discussions will be primarily concerned with making three points: (1) that paradigms, when conceived in the proper way, help to clarify more precisely the failures of earlier (historical) attempts at reconstructing a logic of discovery; (2) that scientific paradigms are, in fact, a significant *presupposition* of contemporary efforts at reconstructing a logic of discovery (this includes Bayesian approaches, analogical-argument approaches, and heuristic programming approaches); and (3) that the paradigm approach to the problem of discovery has the distinct advantage of being able to account for such things as "scientific creativity" and "flashes of genius" without abandoning prospects of finding a method (and *a fortiori* perhaps a logic) of discovery. This later advantage is particularly important since it is capable of explaining the apparent conflict which has long been thought to exist between holding the view that "scientific discovery requires irrational creative intuition" (cf. Popper), *versus* "scientific discovery has a *logic* to it" (cf. Peirce and Hanson). Earlier attempts at reconstruction could not do justice to the phenomenon of scientific *creativity* without sacrificing the logic, and *vice versa*.

Before discussing these points directly, however, consider for a moment a few thoughts which tend to suggest the feasibility of this new approach.

In *The Idea of History*, R. G. Collingwood makes some interesting points about the relationship between historical *questions* and historical _evidence_; these points can be particularly illuminating when raised in the context of scientific investigation generally. Collingwood says:

> . . . (One) will realize that every time the historian asks a question, he asks it because he thinks he can answer it: that is to say, he has already in his mind a preliminary and tentative idea of the evidence he will be able to use . . . Question and evidence in history are correlative. Anything is evidence which enables you to answer your question . . .
>
> A sensible question is a question which you think you have or are going to have evidence for answering . . . nothing is evidence except in relation to some definite question.[4]

Similar remarks are equally appropriate for scientific investigation. Indeed, Collingwood says: "To ask questions which you see no prospect of answering is the fundamental sin in science, . . ."[5]

If science be conceived of as an enterprise which is primarily involved in the _raising_ and _answering_ of questions about nature, it can be seen that the problem of the logic of discovery is more closely aligned with the _questions_ of science than it is with the _answers_ of science. Questions tend to reflect the puzzles of science, whereas answers reflect the accumulated knowledge, or finished research, of science. Suggested hypotheses, which are the _desideratum_ of a logic of discovery, are, in effect, restatements of the _questions_ which science "puts to nature". When Collingwood speaks of "a sensible question" as one "which you think you have or are going to have evidence for answering",[6] he is suggesting that the reasonableness of a question is directly related to the type of evidence which might be used to answer it. Thus, from this point of view, there are _plausible questions_ in science as well as _plausible answers_. And if, as I have suggested, conjectural hypotheses can be looked upon as the questions which science puts to nature (for answers), then it can easily be seen that the logic of discovery is concerned with the reasonable questions of science, far more than the answers. Moreover, since the problem of the logic of discovery is concerned with the process of suggesting reasonable hypotheses, the problem is also (or similarly) one that is concerned with the process of generating reasonable questions. One might expect, therefore, that the source, or mechanism, which helps generate scientific questions, is the same source or mechanism which helps generate scientific hypotheses -- the goal of a logic of discovery.

When one adds to the above line of reasoning the simple fact that scientific questions do not arise in a conceptual vacuum, but typically arise in a significant scientific context, and are expressed in the technical language of the given science, then one can see the appropriateness, and feasibility, of looking at the discovery problem through "scientific paradigms". Compare, for a moment, what Collingwood had to say about the relationship between question and evidence with what Kuhn says at one point about scientific paradigms:

> We have already seen that one of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assured to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. Other problems, including many that had previously been standard, are rejected as meta-physical, as the concern of another discipline, or sometimes as just too problematic to be worth the time. A paradigm can, for that matter, even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies.[7] (underlining is mine)

What Collingwood said about the relationship between question and evidence, Kuhn is here saying about problem (or puzzle) and solution. A scientific paradigm functions as a rich source of problems and puzzles to be solved. Thus, a scientific question, or problem, is intimately related to the existing theory and amassed body of evidence already accumulated by the respective disciplines. This background information, which is accumulated by the discipline, is typically the source of further scientific questions. The problems themselves are initiated by (i.e., originate in) the paradigm. As Kuhn puts it, "the paradigm furnishes interesting (and important) scientific puzzles to be solved by the discipline".[8] Thus, insofar as paradigms function as one source of scientific problems and questions, one might reasonably expect them to play an important role in the suggestion of hypotheses as well.

All of these (aforementioned) considerations about the relationships between question and evidence, puzzle and solution, are not intended to stand as definitive arguments for a logic of discovery.[9] These thoughts have been presented here primarily as a heuristic device which helps suggest the general feasibility, or, if you will, reasonableness of approaching the discovery problem through scientific paradigms. The remainder of this essay will therefore attempt to develop some of the ways in which paradigms illuminate the proposed attempts, both past and present, of reconstructing a logic of scientific discovery.

2. The Meaning of 'Paradigm'

In The Structure of Scientific Revolutions Kuhn introduced the conception of a "scientific paradigm" to help characterize the nature of the historical and conceptual stages in the development of science. Because the notion of a paradigm is so central to the book, nothing short of a complete reading of the book could capture the total richness of the concept. Indeed, Kuhn uses the term in several different ways to do several different jobs: Margaret Masterman has counted and elucidated at least twenty-one different senses of 'paradigm' used in Kuhn's book.[10] For the purposes of the present essay, however, only one of the more fundamental meanings of 'paradigm' need be elucidated here, and even this meaning will have qualifications which are not to be found in Kuhn's work. Moreover, a cautionary note is in order. Although I use the notion of a paradigm, it should not be inferred that I endorse Kuhn's epistemological views, nor even his view on scientific change.

Basically, a scientific paradigm is a conceptual and methodological framework which is shared by a 'scientific community', and this framework serves as a guide, or model for directing research within that community. Paradigms originate when the members of a scientific community recognize and accept some particular scientific achievement as supplying the foundations for the further practice of that discipline. As Kuhn says:

> Aristotle's Physica, Ptolemy's Almagest, Newton's Principia and Opticks, Franklin's Electricity, Lavoisier's Chemistry and Lyell's Geology -- these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. They were able to do so

> because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve. Achievements that share these two characteristics I shall henceforth refer to as 'paradigms', a term that relates closely to 'normal science'. By choosing it, I mean to suggest that some accepted examples of actual scientific practice -- examples which include law, theory, application, and instrumentation together -- provide models from which spring particular coherent traditions of scientific research.[11]

Thus, not only do paradigm-setting achievements bring new understanding to the scientific community, but they also have the effect of redirecting the research interests and subsequent practice of the scientific community toward the further articulation and development of those achievements. Indeed, Kuhn points out that the bulk of scientific research consists in working-out the further problems and puzzles which are generated by the accepted paradigm. For, in addition to the new understanding which the paradigm supplies, there is the promise of still more knowledge to be gained by applying the initial achievement to other areas, and to other sets of facts. This paradigm-oriented research Kuhn calls 'normal science'; by working-out these new puzzles scientists extend the frontiers of that field of investigation. It is this promise of new knowledge which attracts scientists to a paradigm.

> The success of a paradigm . . . is at the start largely a promise of success discoverable in selected and still incomplete examples. Normal science consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself.

> Few people who are not actually practitioners of a mature science realize how much mop-up work of this sort a paradigm leaves to be done or quite how fascinating such work can prove in the execution. And these points need to be understood. Mopping-up operations are what engage most scientists throughout their careers. They constitute what I am here calling normal science. Closely examined, whether historically or in the contemporary laboratory, that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others. Instead, normal scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.[12]

Thus, the notions of 'paradigm' and 'normal science' are very closely linked; and the phenomenon which they refer to are dependent on one another: where a paradigm exists, normal science then ensues; and the practice of normal science presupposes the existence of a paradigm.

It can also be seen that, for Kuhn, a paradigm serves first as an exemplar, or model, of successful research which establishes the beginnings of a new conceptual framework, and then this framework serves to guide, and tie together, discrete bits of knowledge in a cumulative way. Only normal or paradigm-based science is cumulative.

Some of the more concrete ways in which a given paradigm manages to entrench and perpetuate itself can be seen by examining the literature and educational practices of the respective disciplines. For example, the community shares a technical language which is expressive of the paradigm's problems and phenomena, the journals begin to publish and discuss paradigm-based research, and in some cases new journals are formed to cover the growing wealth of paradigm-based knowledge. Perhaps even more importantly, textbooks, which are designed to educate and train new practitioners of the science, recount the achievements of the paradigm, and introduce the student to a core of solved problems and techniques.[13] Kuhn points out that:

> The study of paradigms, including many that are far more specialized than those named above, is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition.[14]

Elsewhere, Kuhn also points out that the training of scientists includes the use of procedures and instruments which are peculiar to the paradigm; and he argues that these "Paradigm procedures and applications are as necessary to science as paradigm laws and theories."[15] It might also be pointed out that when a science becomes increasingly more professionalized, the norms and standards of the paradigm become more pronounced and, if you will, more dogmatic. The acceptance and proliferation of a scientific paradigm includes much more than the intellectual acceptance of a scientific theory.

> In the development of any science, the first received paradigm is usually felt to account quite successfully for most of the observations and experiments easily accessible to that science's practitioners. Further development, therefore, ordinarily calls for the construction of elaborate equipment, the development of an esoteric vocabulary and skills, and a refinement of concepts that increasingly lessens their resemblance to their usual common-sense prototypes. That

> professionalization leads, on the one hand, to an immense restriction of the scientists' vision and to a considerable resistance to paradigm change. The science has become increasingly rigid. On the other hand, within those areas to which the paradigm directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way.[16]

The preceding elucidation of Kuhn's conception of 'scientific paradigms' and 'normal science', while incomplete, captures at least the core meaning of those concepts required for the purposes of the present essay. It is important to bear in mind, however, that Kuhn's primary concern, indeed, the central thesis of his book, is about scientific change and "revolution" (hence the title: The Structure of Scientific Revolutions), far more than it is concerned with what might be considered the more mundane practices of 'normal science'. The notions of 'paradigm' and 'normal science' are used to describe the *status quo* which eventually results in bringing about change, or revolution -- the main interest of the book.

Very generally, Kuhn's thesis about change is that the history of science up to the present has been a history of successive research traditions, or paradigms, each one of which led to a state of "crisis" by the persistent appearance, or encroachment, of "anomalies" which the existing paradigm could not resolve. When the scientific community begins to regard its more persistent problems as not simply "puzzles to be solved" but rather as serious "anomalies" which threaten to refute the existing theory, then that science is said to be in "crisis" and thereby becomes a candidate for paradigm revision, or revolution. Confronted by crisis, scientists "do not renounce the paradigm that has led them into crisis"[17]; however, they may "begin to lose faith and then to consider alternatives".[18] The activity of normal science begins to give way; and in this situation:

> scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all of these are symptoms of a transition from normal to extraordinary research.[19]

If, and when, an alternative paradigm begins to appear on the horizon, debates between these competing schools set the stage for the ensuing revolution; and as Israel Scheffler points out:

> The choice between rival paradigms lies, in fact, beyond the capacities of normal science to resolve. Since it hinges upon considerations external to normal science, the issue in a paradigm debate is, indeed, revolutionary, involving a fundamental re-consideration and potential redefinition of normal science itself.[20]

Thus, while Kuhn is primarily concerned with elucidating these scientific revolutions (i.e., how paradigms change), the present essay, on the other hand, is primarily interested in 'normal science' which takes place in relatively secure 'paradigms'. The concern of this essay with "revolutions" and "science in crisis" is tangential.

With respect to the basic wisdom of looking at the history of science through paradigms, Imre Lakatos, one of Kuhn's more perceptive critics, finds agreement with Kuhn on this basic point. Lakatos says:

> The most important such series in the growth of science are characterized by a certain <u>continuity</u> which connects their members. This continuity evolves from a genuine research programme adumbrated at the start. The programme consists of methodological rules: some tell us what paths of research to avoid (<u>negative heuristic</u>), and others what paths to pursue (<u>positive heuristic</u>).

And he continues:

> One may point out that the negative and positive heuristic gives a rough (implicit) definition of the 'conceptual framework' (and consequently of the language). The recognition that the history of science is the history of research programmes rather than of theories may therefore be seen as a partial vindication of the view that the history of science is the history of conceptual frameworks or of scientific languages.[21]

Lakatos' notions of "positive and negative heuristics" are just another, perhaps more succinct, way of analyzing precisely how a paradigm influences the methodology of practising scientists. And it is this influence (i.e., how paradigms affect scientific methodology) which is of fundamental importance in the present analysis of the PLD.

As was mentioned above, Kuhn, at times, uses the term 'paradigm' to mean much more than is sanctioned, or needed, here. There are some minimal features of a paradigm which shall be of particular interest here because of the role they play in the suggesting of scientific hypotheses. For the purposes of the present essay, by 'paradigm', I mean no more than the conjunction of the following four selective features of a paradigm:

(1) That a given scientific community regards some particular achievement (or discovery) as fundamental to their enterprise, such that it serves as a source of further problems (or puzzles) to be investigated.

(2) That there is, typically, a backlog of data and information which the community shares and accepts, and has access to through its literature.

(3) That the scientific community shares a technical language, which is expressive of the problems and accomplishments in that field of investigation.

(4) That the training and textbooks for new members to the community include certain uniform concrete models, and a familiarization with certain rules and standards for scientific practice.

No doubt, even this basic meaning of 'paradigm' has far-reaching epistemological implications. However, despite the intrinsic interest of these issues, a discussion of these points would be far beyond the scope of an essay on the problem of the logic of discovery as such. The stated purpose here is to reexamine the logic of discovery issue from the perspective of this basic meaning of 'paradigm', irrespective of other issues which may lie in balance.

Based on the present meaning of 'paradigm' then, the term 'normal science' means, as it did for Kuhn, paradigm-based science. One of the main characteristics of 'normal science' is the fact that the research problems which it undertakes are viewed as "puzzles" which are well within the means of the paradigm to resolve; that is, the problems that await solution are in no way regarded as threats to the foundation of the paradigm, but rather, they are regarded as prime candidates for resolution (or puzzling-out) at some future date. Thus, normal science is characterized by the presence of a relatively secure paradigm, confident of solving its puzzles.

'Science in crisis', on the other hand, is a science whose foundation (or hard core) is being threatened by serious <u>anomalies</u>, and this has the effect of undermining the viability of the existing paradigm itself, not to mention the practice of 'normal science'. A science in crisis cannot rely on the prescriptions of the paradigm to solve further problems because the veracity of the paradigm itself is under serious question and attack. In this situation, 'normal science' is impoverished and cannot move ahead because of the paradigm's internal conflicts; thus, the attention of the community becomes directed inward toward paradigm revision, or indeed, toward replacement of the paradigm with one that appears anomaly-free.

A more succinct way of describing these situations would be to say simply: in 'normal science' the paradigm is firmly grounded, thus, there is a viable research model to follow; whereas in 'crisis situations' the security of the paradigm has been undermined and fragmented (by anomalies) thus, there is no viable research model to follow.

Before leaving this discussion for the main business of analyzing the PLD, there is one last point which should be clarified. In his book, Kuhn makes a distinction between "puzzles" (or normal research problems) and "anomalies". However, the way in which he attempts to make this distinction leaves something to be desired from a more formal, or reconstructionist, point of view. Basically, Kuhn attempts to make the distinction by holding that a "puzzle" is the normal type of research problem which occupies the scientists' investigative activities, and this activity typically results in furthering the development of that science; in any case, however, the research leaves the stability of the paradigm intact. "Anomalies", on the other hand, are problems which continue to produce recalcitrant observations, or results, (i.e., results which are unpredicted and unexpected), which tend to refute the existing theory, thus, undermine the paradigm. However, when asked what distinguishes an unsuccessful "puzzle" solution (or attempted solution) from an "anomaly", Kuhn replied: "There is no logical difference between a puzzle and an anomaly. It is simply a difference of what types of difficulties, and which people, you take seriously".[22] Thus, Kuhn really bases the distinction between a puzzle and an anomaly on sociological and psychological considerations; hence the change from periods of 'normal science' to 'crisis' is really a bandwagon effect, or a matter for mob psychology. In any case, the distinction between "puzzle" and "anomaly", and the corresponding changes which they bring about, are not distinctly demarcated by Kuhn.

For purposes of clarity, the present discussion will attempt to make the distinction between "puzzle" and "anomaly" somewhat more precise. In keeping with the spirit of Kuhn's distinction, the difference between "puzzle" and "anomaly" has a parallel in logical problem-solving. A puzzle may be conceived of as similar to the normal problem solving situation where a person has a set of premises (P), and is attempting to derive a desired conclusion (C): in using his rules of inference, if 'C' is not derived on the first attempt, then ingenuity and perseverence is called for.[23] In any case, the problem-solver's first attempts did not produce anything of the form 'non-C', suggesting that the desired conclusion is impossible; thus, the problem-solver's first reaction is that more ingenuity is required, and that he simply has not been clever enough to solve it. This interpretation of "puzzle" corresponds to one of Kuhn's oral statements made at a symposium:

> In normal science the effect of a mis-spent experiment is: "that I (the scientist) just couldn't solve the puzzle"-- not that "the theory is wrong".[24]

Here the parallel is clear: in the one case (i.e., logic) the investigator fails to derive the desired conclusion, in the other case (i.e., in science) the results of the experiment are inconclusive, and the hypothesis is neither confirmed nor disconfirmed. A puzzle remains in both cases.

Further, in logic, an "anomaly" would be the situation where the problem-solver actually derives something of the form 'non-C' in his attempt to deduce 'C'. That is, he deduces a conclusion which is clearly incompatible with the predicted, or expected, result. Thus, by *modus tollens*, he infers that there is something wrong with the premise set itself. And, in logic, because the number of axioms and premises for any problem is always finite, and well defined, one can see where adjustments need to be made. However, in science, because there are so many suppressed premises in the form of 'auxiliary hypotheses', and relevant 'initial conditions', it is extremely difficult to determine what sorts of changes need to be made in order to remedy an anomaly. At this point (i.e., for these reasons) it must be recognized that the parallel between science and logical problem-solving breaks down. However, a closer look at the precise reasons why the parallel breaks down shows that the distinction between puzzles and anomalies can be usefully demarcated along logical lines despite the difficulties in properly directing the *modus tollens* in scientific anomalies.[25]

In particular, when a scientific investigator obtains experimental results which are recalcitrant (i.e., not predicted by the theory) it is not clear what revision or adjustment should be made. For example, the recalcitrant or unexpected result could be due to a number of things, such as: (1) an incomplete account of the relevant initial conditions, (2) the employment of an auxiliary hypothesis which is false, (3) the theory could be incomplete in the sense that it requires enrichment through the introduction of new concepts, or more seriously, (4) the existing theory could be fundamentally wrong. In any case, when unexpected results are obtained it may not always be clear to which of these specific areas the modus tollens should be directed. However, despite these difficulties with anomalies in science, there are no compelling reasons which would force one to give up the distinction between "puzzle" and "anomaly", nor the logical model for construing the distinction. Indeed, from the point of view of rational reconstruction, it would be premature, and imprudent, to do so. I think this for several reasons, reasons which are both theoretical and practical.

First, from a theoretical point of view, a clear distinction between puzzle and anomaly can be made by defending the notion of crucial experiments in science (i.e., that it is possible to obtain experimental results which refute hypotheses). This point of view can be maintained along the lines pointed out by Adolf Grünbaum in his criticism of the Duhem-Quine thesis which had denied such experiments.[26]

From a practical point of view, however, and more pertinent to actual scientific practice, is the fact that paradigms help the practitioner to distinguish between puzzles and anomalies, and how to cope with them once they have been recognized. Insofar as the paradigm furnishes the community with certain standards and expectations, it thus has the capacity to recognize when those expectations are being seriously violated. That is, the paradigm helps the working scientist to distinguish between results which are merely inconclusive (and thus a puzzle), and results which are clearly recalcitrant or unexpected (and thus anomalous). Inconclusive results are those which the evidence is not strong enough to warrant either confirmation or disconfirmation; whereas recalcitrant (or anomalous) results are those which are clearly incompatible with what the standards and expectations of the paradigm would lead one to anticipate. Thus, part of the function of the paradigm is the determination of what sorts of results are to be regarded as anomalous, and what sorts are merely inconclusive. And, as is clear in Kuhn's work, paradigms continue to function in the presence of some anomalies all the time; it is only when there is a serious accumulation of anomalies that the science begins to face "crisis".

Moreover, when anomalous results are obtained there are various ways in which they are typically handled. Because of the relative security and predominance of the paradigm at large, in the face of recalcitrant data there is an understandable reluctance to over-throwing the paradigm-base itself. Instead, what emerges is a kind of hierarchical check-off list for the less serious things that could be changed in order to remedy (or account for) the anomaly. For example, auxiliary hypotheses might be revised or even invented before the status of a law would be seriously challenged. This is not to suggest that laws cannot be challenged or disconfirmed, but only to point out that this consideration (or move) would be one of the last to be sanctioned by the paradigm, and what is taken to be good scientific practice. As Imre Lakatos has pointed out in his discussion of what he calls a "negative heuristic":

> All scientific research programmes may be characterized by their 'hard core'! The negative heuristic of the programme forbids us to direct the modus tollens at this 'hard core'. Instead, we must use our ingenuity to articulate or even invent 'auxiliary hypotheses',

> which form a _protective belt_ around this core, and we must redirect the _modus tollens_ to these. It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core. A research programme is successful if all this leads to a progressive problemshift; unsuccessful if it leads to a degenerating problemshift.[27]

A "progressive problemshift" in normal science is where the research activity can move on, or progress, to further puzzles; and a "degenerating problemshift" occurs when anomalies are encountered and readjustments, indeed, revisions, of the paradigm are required. Of course, the last and most serious adjustment would be the overthrow of the paradigm itself.

In sum, the present distinction between "puzzle" and "anomaly", and how each of these are handled in science, comports quite well with Lakatos' discussion of "progressive" and "degenerating" problemshifts. I think either of these approaches are more precise, and thus preferable, to the socio-psychological account of these distinctions offered by Kuhn.

From the point of view of these Kuhnian (or Kuhn-type) conceptions then, it will be instructive to take a brief look, in retrospect, at the earlier (historical) attempts at reconstructing a logic of discovery.

3. A Retrospective

The examination of Whewell's views on the problem of the logic of discovery (Chapter II) revealed three distinguishable theses about scientific discovery. Whewell argued:

(1) that the introduction of a conception (or hypothesis) is a required element in discovery, and that this conception is supplied by the mind of the discoverer.

(2) that there is no "Art of Discovery" per se, but it is possible to formulate, or point out, certain "aids" to this process.

(3) the process of hitting upon the right conception is one of skillful trial and error by a trained mind.

However, despite their promise, Whewell's views on these topics were left either underdeveloped, or simply confused; but against the back-drop of scientific 'paradigms', and 'normal science', these confusions become much more transparent and understandable.

One of the distinguishing characteristics of Whewell's philosophy of science was his early recognition of the fact that discovery, hence the progress of science, required the introduction of a conception which is supplied by the mind to "collegate" observed phenomena into laws. In the debate with Mill, Whewell was at pains to point out that hitting upon the right "colligating concept" was the most difficult part of scientific discovery, and that this "conception is supplied by the mind and superinduced upon the Facts".[28] Whewell argued:

> To hit upon the right conception is a difficult step; and when this step is once made the facts assume a different aspect from what they had before: that done, they are seen in a new point of view; and the catching this point of view, is a special mental operation, requiring special endowments and habits of thought. Before this, the facts are seen as detached, separate, lawless; afterwards, they are seen as connected, simple, regular; as parts of one general fact, and thereby possessing innumerable new relations before unseen.[29]

It was found (in Chapter II) that this part of Whewell's view was sound, at least as far as it went. However, with his emphasis on how difficult it is to hit upon the right "colligating concept", together with his view that there is no "Art of Discovery", Whewell leaves the impression that finding the right type of hypothesis ("colligating concept") requires a flash of genius on the part of the investigator. That is, Whewell gives the impression that the correct hypothesis, which is a "conception supplied by the mind", comes to the investigator more or less _ex nihilo_, and is then "superinduced upon the Facts".[30] Whewell failed, I think, to recognize the influence which paradigms and normal scientific practice have on this process. Normally, (e.g., in normal science) the correct hypothesis is one of a certain type, and this type of hypothesis is suggested by the models, training, data, and language of the existing _paradigm_. The correct type of hypothesis does not typically come to the mind of the investigator _ex nihilo_, nor, as Whewell had argued against Mill, from simply _looking_ at the data; but rather, it comes couched in the terms of an available paradigm, and with a background of normal scientific practice. In "normal science", where the research problems can be characterized as "puzzles", qualities of "creative genius" are required far less than ingenuity -- the pieces of the puzzles are already there, albeit not always in observational form.

It would appear that Whewell was so impressed with the achievement of Newton which established a new paradigm, that he took Newton's discovery (e.g., universal gravitation) to be the perfect case of all scientific discovery. Certainly, Whewell's view that science continues to move toward more and more general laws (i.e., up the hierarchical pyramid of his "Inductive Table") suggests that Whewell took Newton's discovery as his model. For Whewell, the "_order of discovery_" always consists in introducing a new, more general, colligating concept, which "possesses innumerable new relations before unseen".[31] This contrasts sharply with the Kuhnian view that "normal" scientific discovery consists in "mopping-up", or working-out the present puzzles of an existing paradigm. Kuhn would agree with Whewell on the importance and impressiveness of Newton's discovery but he would disagree (as would I) with Whewell on just how typical this is of scientific discovery generally.

When one considers Herschel's (Whewell's colleague) discovery of Uranus, and Whewell's own work in physics, and on the tides, Whewell's position on "the order of discovery" appears even more curious. All of these latter discoveries resulted from Newtonian "paradigm-based" research: none of them introduced the new, more general, colligating concept which Whewell finds characteristic of the "order of discovery".

This is not to suggest that Whewell was wrong in his general emphasis on the need to employ hypotheses (or colligating concepts) which are not part of the observation data itself: on this point he was right, and far more perceptive than Mill. This is to suggest, however, that his "order of discovery" was ill-conceived; and that he was confused about the genesis, and logical order, of these hypotheses in the progress of science. The Kuhnian view, which is being considered here, would point out that the existing paradigm provides a rich supply of models and concepts from which the investigator constructs hypotheses, and seldom (in normal science) does his research require the invention of totally new concepts of the type Whewell envisaged. In general, Whewell failed to recognize the role played by paradigms in suggesting the "colligating concepts" which, for him, were essential to scientific discovery. If one considers only those "revolutionary" discoveries which establish whole new paradigms (e.g., universal gravitation) then Whewell's views are not far off the mark; however, when one considers the more common paradigm-based discoveries of science, Whewell's views are misleading.

It was also pointed out in Chapter II that Whewell's views on the logic of discovery contained a certain <u>tension</u> which he never managed to reconcile. The tension issues from his holding the following two views: (1) that there is no art, or logic, of scientific discovery, indeed, "an Art of Discovery is not possible"; and (2) scientific discovery requires "training", "discipline", and "skill", and does not come about by "accident". It was pointed out that if it is true that scientific discoveries are "non-accidental", requiring the "skill and discipline of a trained researcher", then it would seem (to me, at least) that an "Art of Discovery" (as he called it) would not only be <u>possible</u>, but indeed <u>plausible</u>. Whewell even used the phrases "informed trial and error" and "skillful guesswork" in explicating his view of the discovery process. Keeping this in mind, it can be seen how illuminating, indeed useful, the concepts of 'paradigm' and 'normal science' could be in helping to explain this tension in Whewell's views. The skill, training, and discipline which Whewell spoke about is precisely that which Kuhn used in explicating a "paradigm"; and Whewell's "skillful guess-work" and "informed trial and error" are exactly what characterizes the "puzzle-solving" of "normal science" for Kuhn. If, as I have suggested, "puzzle-solving" has much in common with normal problem solving in logic, then it would have been reasonable for Whewell to at least suggest that a logic of discovery is <u>possible</u>, even though none had ever been constructed. Despite his disclaimer, everything in Whewell's position suggests that a logic of scientific discovery is not only <u>possible</u>, but <u>plausible</u>. Whewell did not develop his position far enough to arrive at this conclusion; interestingly, whenever Whewell said that an "Art of Discovery is not possible" he nowhere offered an argument to that effect. If Whewell had offered such an argument, the tension in his position would be more strained, and the incompatibility with his general view about discovery would be clear, and more obvious than it is now.

In general, it would be fair to say that most of the difficulties in Whewell's position on discovery stem from his view on the nature, particularly the newness, of the required "colligating concept". For him, discovery involved the introduction of a new, or novel, concept; and insofar as this concept was a novel one, he could not see how logic could help in its introduction: this is probably what prompted him to say "an Art of Discovery is not possible". However, had Whewell <u>not</u> taken "revolutionary" discoveries, which do introduce novel concepts as his model, he may have been better able to see that "normal science" does not require introducing such novel concepts, since most of the concepts are provided by the paradigm. Insofar as Whewell continued to hold that discovery is "non-accidental" and requires "informed trial and error", it is surprising, and unfortunate, that he did not pursue that line of thinking and conclude that the process (in normal science) may therefore be rationally reconstructable.

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In the discussion of Peirce's contributions to the problem of the logic of discovery (Chapter III), two central difficulties were found to lie in his general views. Again, from the perspective of the Kuhnian concepts introduced here, these difficulties are not only more understandable, but indeed more manageable. Thus, it will be instructive to take a brief look at these difficulties from this point of view.

The first difficulty was found to lie in the inadequacies of his schema which was intended to capture the form of retroductive (or abductive) inference. Retroduction, it will be recalled, is Peirce's view of the form of reasoning used to introduce hypotheses into scientific reasoning. Of this form of reasoning, Peirce says:

> Long before I first classed abduction as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis -- which is just what abduction is -- was subject to certain conditions. Namely, the hypothesis cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts of some of them. The form of inference, therefore, is this:
>
> The surprising fact, C, is observed;
> But if A were true, C would be a matter of course,
> Hence, there is reason to suspect that A is true.
>
> Thus, A cannot be abductively inferred, or if you prefer the expression, cannot be abductively conjectured until its entire content is already present in the premise, "If A were true, C would be a matter of course".[32]

And the earlier discussion of this view adopted the Hanson-Buchdahl reformulation of this schema, which is:

(1) P (some surprising phenomenon is observed)
(2) $H \supset P$ (If H were true, it would explain P as a matter of course)
(3) H (Hence, there is reason to suspect that H is true)

As was argued earlier however, not all possible hypotheses that could explain the phenomenon are reasonable H's, thus, the schema fails to distinguish between suggesting reasonable H's from unreasonable ones, i.e., the class of plausible hypotheses is a sub-class of the possible ones that could explain the data. The problem of course, is to explain what suggested H in the first place (i.e., in the premises), and Peirce's schema does not throw any light on that. It would not do to say simply that "H is suggested because H is reasonable", because reasonableness is not an intrinsic property of hypotheses, but rather it (reasonableness) is a relation between a proposition (or hypothesis) and relevant background information.[33] It is background information which helps to demarcate the class of plausible H's from the possible ones.

Thus, in order for Peirce to be able to have abduction be a form of inference which sanctions only plausible hypotheses, as opposed to any (and all) that could explain the data, he would need to have his schema take into account the "background information" which renders some hypotheses more reasonable than others. An adequate account of the relevant background information would explain why some hypotheses are regarded as "reasonable", and this in turn would show where the H (i.e., in the premises of the schema) came from in the first place.

Insofar as Peirce's schema omits any reference to the relevant background information we remain at a loss to know why the hypothesis put forward by a scientist is anymore reasonable than that put forward by a gypsy; moreover, we have no idea where the scientist's hypothesis came from in the first place. Scientists, unlike gypsies, do not use crystal balls; nor do scientists put forward just any (possible) hypothesis which could explain the data. Abduction, as a putative logic of suggestion, leaves these difficulties unanswered.

Again, the concept of a paradigm is extremely suggestive and useful for coping with the difficulties in Peirce's schema. A paradigm, after all, can be regarded as the conjunction of all the relevant "background information" with which a scientist approaches a problem. Thus, when a scientist confronts a problem, or "some surprising phenomenon is observed" (as Peirce put it), he is already laden with a plethora of concepts, accumulated information, training, models, and technical language which will play an important role in suggesting his hypotheses. In short, the hypotheses will be paradigm-based: the scientist would attempt to formulate his hypothesis in terms of the concepts and information already provided by the paradigm. Thus, among other things, the paradigm serves to demarcate a certain class of hypotheses which would be regarded as more plausible than the infinitely large class of non-paradigmatic hypotheses which are simply possible. Peirce's schema failed to demarcate this class of plausible hypotheses from the much larger class of possible ones, and this is precisely what paradigms would help to do. Peirce's original schema for abduction could be reformulated, and thus strengthened, with the addition of more steps (e.g., steps (2) and (3) below) which are provided by the existing paradigm. The schema might look as follows:

(1)	P	(Problematic, or surprising, phenomenon)
(2)	B	(Background information consisting of paradigm models, concepts, and information)
(3)	H'	(A set of hypotheses wholly constructable from, and consistent with, 'B')
(4)	$H \supset P$	(If H, which is a member of the set H', were true, P would follow as a matter of course)
$\therefore$ (5)	H	(Hence, there is reason to suspect that H is true)

Thus, the relevant background information could be worked into Peirce's schema without much difficulty. While it remains possible that the set H' is fairly large, this is still a vast improvement over Peirce's original schema which did not recognize any such class. Paradigms provide science with steps (2) and (3) of this schema, and thus enable on to distinguish the plausible, from the merely possible, hypotheses.

The second major difficulty which was found to lie in Peirce's position on the logic of discovery, is a difficulty which is very similar to one found in Whewell. The difficulty issues from Peirce's view that abduction is a form of inference used to introduce new ideas into a problem-solving situation. Recall his saying "It (abduction) is the only logical operation which introduces any new ideas, ... ".[34] As was pointed out earlier, Peirce's use of the word "new" in the phrase "new idea" is potentially problematic. Peirce does not seem to notice that there are certain formal difficulties with a conception of "logic" which would allow one to introduce totally new terms or new concepts into the existing logical framework (i.e., as part of the framework). Logic cannot provide a mechanism for introducing totally new types of entities into its system. Formation rules, whether only tacitly or explicitly formulated, serve the function of specifying the types of entities for which the inference rules apply. Thus, it was pointed out, there is an important sense of "new term" which a logical system cannot accommodate without violating its own formation rules.

In the discussion of Whewell it was noted that Whewell appeared to recognize the difficulty of introducing new ideas through logic, and thus concluded that "an Art of Discovery is not possible". However, as was pointed out in the discussion of Whewell, the error in this line of reasoning is the assumption that scientific discovery always involves the introduction of "new terms", or new "colligating concepts". In normal science, it has been argued, the existing paradigm provides the investigator with a rich supply of concepts and models from which he constructs hypotheses; and seldom (in normal science) does his research require the invention of totally new concepts of the type envisaged here by Whewell and Peirce. Thus, Peirce also failed to recognize the role played by paradigms in suggesting scientific hypotheses.

This latter point regarding the introduction of new terms in scientific discovery is worth reiterating since it is a fairly common point of misunderstanding in this context. It should be remembered that the problem of the logic of discovery is to rationally reconstruct the methodological process of suggesting plausible hypotheses in investigative situations, i.e., we are trying to find out how hypotheses are suggested, and whether this process is amenable to formalization. There is nothing in the nature of the problem itself which implies that the hypotheses must necessarily contain new terms, or new concepts. Indeed, the vast majority of hypotheses in normal scientific practice do not involve new terms or concepts, but rather, they typically contain concepts and ideas that are already provided by the paradigm. To concentrate on those dramatic cases where totally new concepts are introduced (as Whewell and Peirce seemed to do), is to overlook the normal state of affairs in suggesting hypotheses. While totally new concepts are sometimes suggested as well as revolutionary achievements which result in paradigm revision, it is particularly misleading to focus on these cases as exemplars of the discovery process generally.

In short, the difficulties involved in introducing new concepts can be averted by restricting (at least temporarily) the scope of the discovery problem to normal paradigm-based discoveries, where the need for new concepts is relatively insignificant. This class of normal paradigm-based discoveries represents the vast majority of discoveries in cumulative scientific research. Thus, by restricting the scope of the problem to normal science, one not only avoids the formal problem of violating a given set of logical rules (e.g., introducing new concepts into an established framework), but one properly concentrates on the typical (or normal) instances of discovery, and recognizes the exceptional instances as just that -- exceptional. After all, if a logic can be found for either class of cases, then this would be a step in a positive direction; and should the class of cases be large, then so much the better.[35]

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When one begins to look back at Hanson's views on discovery from the perspective of Kuhnian paradigms, one sees so many points of comparison and contrast that the temptation is to simply list these points rather than discuss them. Indeed, some of these points of contrast are so sharp and illuminating, that it would require a separate essay to do them justice. However, in the present context, an examination of one or two of the major points of contrast will suffice for this brief retrospective.

On most of the basic epistemological issues in science, Hanson and Kuhn are in fundamental agreement with one another. Both defend the general thesis that scientific facts are "theory-laden", and observation in science is shot-through with theoretical commitments. Moreover, they both hold the view that there is no theoretically neutral way to describe so-called scientific facts: for Kuhn, scientific facts are determined (or overdetermined) by the existing paradigm; and for Hanson facts are always ordered into (and constitutive of) specific conceptual-Gestalts.

However, despite the agreement on fundamental epistemological issues, Hanson and Kuhn would appear to differ markedly on their analyses of scientific discovery. Insofar as Kuhn did not discuss the logic of discovery issue as such, all one can do is interpolate along such lines as have been recommended in this essay.

The disagreement between Hanson and Kuhn on the discovery problem would appear to begin at the very basic question (or perhaps, observation) of what type of investigative problems scientists normally engage themselves in. Where Kuhn argues that in normal science the scientists is typically working-out the puzzles of the paradigm, which includes considerable "mop-up" investigation, Hanson on the other hand, pictures the scientist as typically engaged in the more dramatic activity of seeking new theories, and new conceptual-Gestalts. For Kuhn, Hanson's "conceptual Gestalt" is already a part of the existing paradigm, and normal research consists in developing and refining the paradigm into broader areas. In contrast, Hanson says:

> I should argue strongly that the history of scientific progress is not a history of increasingly refined laboratory techniques but a history of changing <u>conceptions</u>. Something was looked at in a new way, the priority of some principle of nature was challenged, ...[36]

To further develop this dynamic view of scientific progress, Hanson consistently stresses what he takes to be the general instability and perplexity of scientific research. He says, for example:

> One must compare the conceptual perplexities of contemporary physicists with those of Galileo, Kepler, Descartes and Newton when they were creating physics. A Galileo grappling with acceleration, or a Kepler considering a noncircular planetary orbit, or a Newton reflecting on the particulate nature of matter and light -- these do not differ essentially from cases of

> a Rutherford entertaining 'Saturnian' atoms, or a Compton proposing a granular structure for light, or a Dirac suggesting a positive electron, or a Yukawa wrestling with the idea of a 'messon'. This is frontier physics, natural philosophy.[37]

This picture of scientific research activity is considerably more profound and dynamic than the "puzzle solving" of Kuhn's "normal science". This does not suggest, however, that one picture is correct and the other wrong: both descriptions of scientific research manage to capture the particular aspects of it with which they are concerned. The contrast in descriptions here is much like two descriptions of an army, say: one description might focus on the logistical planning (and perplexities) of the generals, while the other focusses on the individual activities and problems of the soldiers. In their respective views of scientific research activity, Hanson and Kuhn have selected different basic data from which they begin. It is to be expected therefore that they would have different views on the relative importance of such things as "creative genius" versus routine "paradigm procedures" in the process of scientific discovery. Where Hanson sees the researcher as typically grasping for new theories (and conceptual Gestalts) to explain anomalies, Kuhn views him as being guided by a paradigm to apply an existing theory to new and different classes of phenomena.

As was pointed out, Hanson's view of the process of discovery is a development of Peirce's theory of retroduction; and Hanson is quite explicit about how he conceives discovery to take place via this process:

> ... the retroductive account pictures him (the scientist) only as possessing the initial conditions and some profoundly upsetting anomaly, by reflection upon which he seeks an hypothesis, or rather a kind of hypothesis, to explain the anomaly and to found a new theory. That is,

> he seeks a novel H-D framework within which to reveal the anomaly as logically-to-be-expected.[38]

It is clear that, for Hanson, discovery consists in finding "novel H-D frameworks", or new conceptual Gestalts as he says elsewhere; thus he does not distinguish between those discoveries which take place as the result of normal scientific practice within a paradigm, and discoveries which result in new theories and new paradigms. As was suggested earlier however, the distinction is particularly important since the requirements (or ingredients) for discovery in the latter cases are far more sophisticated than those required for discovery in the former cases. While it is easy to share Hanson's interest in those great discoveries which establish new theories and new H-D frameworks, one would be amiss in thinking that most discoveries are like this, or have such results; indeed, one would be further amiss to think that most scientific research is directed to "finding new theories" and new "frameworks". From the point of view of paradigms and normal science, the type of discovery which captures Hanson's interest is seen as comparatively rare "resolutions of anomalies" which are more typical of a science in crisis. Hanson's account of retroductive discovery gives the impression that every discovery results in a new theory; it fails to account for the cumulative discoveries which result from the practice of normal science within an existing framework, or paradigm.

As a result of Hanson's view that discoveries typically result in new theories, there are two interesting consequences for some of the other views which Hanson holds in this connection. Hanson was deeply impressed with Leverrier's prediction of the unseen planet, Neptune, and also with Leverrier's subsequent reasoning in predicting the planet

Vulcan to explain the aberrations in the perihelion of Mercury. Of the former prediction Hanson says "it raised classical mechanics to its highest pinnacle";[39] and the two predictions taken together were "the Zenith and Nadir of Newtonian Mechanics".[40] What is interesting about Hanson's example, at least from the present point of view, is that it is what Kuhn would call a classic case of "paradigm-based" research. Even Hanson says, at one point, that: "aberrations in the perihelion of Mercury made Leverrier uncomfortable; but to have scrapped celestial mechanics then would have been to refuse to think about the planets at all".[41] Thus, Leverrier was not looking for a new H-D framework, but rather, both of his predictions were based on the available one; and to have not used the available framework "would have been to refuse to think about the planets at all". Leverrier's hypotheses did not, in any way, result in establishing a new H-D framework, which as Hanson says, characterizes scientific discovery. Indeed, many of Hanson's own examples do not support his general thesis about discovery; but on the contrary, many of them do support the distinction being made here via paradigm-based discoveries.

A second point of interest which results from Hanson's view of discovery can, again, be seen quite clearly when placed against the background of the above example. In the earlier discussion of Hanson's view of retroduction (Chapter IV), it was pointed out that, at one instance, Hanson appeared to be making a significant improvement on Peirce's schema for retroduction, by making explicit reference to "background information" in the schema. The Leverrier example is an excellent case in point, since, in this example Hanson recognizes that the presence of Newtonian mechanics was an integral component in

Leverrier's reasoning. Leverrier used Newtonian mechanics, as Hanson recognized, to help him suggest his hypotheses.[42] And in the earlier discussion it was pointed out that Hanson seemed to be reformulating Peirce's schema by taking such background information into consideration. At that point Hanson's schema might have been:

e	(background information, e.g., Newtonian mechanics)
P	(Puzzling phenomenon observed)
$\underline{H \supset P}$	(H could explain P as a matter of course)
$\therefore H$	(It is reasonable to suggest H)

Even this much would have been an improvement over Peirce's schema, since the ingredients for the H might be embedded in e. However, Hanson never pursues the business of introducing background information, and when he does, its function is extremely vague. Indeed, Hanson's repeated statements about the form of retroduction leave this factor out; moreover Hanson's most explicit statement about retroduction all but denies that this factor is relevant:

> the retroductive account pictures him (the scientist) only as possessing the initial conditions and some profoundly upsetting anomaly, by reflection upon which he seeks an hypothesis, or rather a <u>kind</u> of hypothesis, to explain the anomaly and to found a new theory. That is, he seeks a novel H-D framework within which to reveal the anomaly as logically-to-be-expected.[43]

This description of retroduction does not take into account the background information which Hanson found so important in his discussion of Leverrier's hypotheses; and this is particularly unfortunate since it would have been a decided improvement over Peirce's schema, and also a step toward recognizing the function of paradigms in the discovery process.

There are many more points in Hanson's views on discovery which paradigms help to illuminate, but space considerations will not permit their development here. However, before leaving this discussion of Hanson, there is one point which might be made here since it will be pursued further in the next section.

It will be recalled from Chapter IV that when Hanson defended his views against Feigl's attacks on a logic of discovery, Hanson argued for a form of analogical reasoning. Hanson's position was that retroduction (as a third kind of inference) was far more akin to analogical reasoning than it was to induction or deduction. Yet Feigl persisted by suggesting that the reasons one gives to inductively establish a particular hypothesis, are the same sorts of reasons one would have for suggesting H in the first place; as Feigl says: "They are reasons both for proposing that H will be of a certain type and for accepting H". But Hanson argues for the difference in the types of reasons by saying to Feigl:

> Agreed, again. But, analogical and symmetry arguments could never by themselves establish particular H's. They can only make it plausible to suggest that H (when discovered) will be of a certain type. However, inductive arguments can, by themselves, establish particular hypotheses. So they differ from arguments of the analogical or symmetrical sort.[44]

Without discussing this debate again (or taking sides) the point to be noted here is that analogies are relational i.e., analogies are a type of relation which exists (or is asserted to exist) between two relata. Even if one admitted, with Hanson, that retroduction involves analogical reasoning, and that this is different from induction, the interesting question is: "between what two things do these analogies hold?" Hanson does not discuss this question, hence he does not give adequate recognition to the fact that analogies require a base of accumulated experience from which analogies can be generated. The models of successful research, which are part of the paradigm, are extremely suggestive here. Kuhn would say (perhaps to Hanson) that the analogical relation exists (or is seen to exist) between the data and the existing paradigm. The problematic data is seen as an exemplar, or even as an instance, of the models and laws in the paradigm. Kuhn says, in this connection:

> Though the strength of group commitment varies with non-trivial consequences, along the spectrum from heuristic to ontological models, all models have similar functions. Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each . . . [45]

Thus, Hanson's position on analogies could have been strengthened, and our understanding broadened, had he recognized the role played by background information (*viz*. paradigms) in suggesting these analogies, and *a fortiori* scientific hypotheses.

4. Paradigms and Contemporary Approaches

Recently, there have been some important new developments in the problem of the logic of discovery, the origin of which is sometimes quite independent of the history we have been tracing here. These new approaches to the problem share the same practical concern (*viz*., suggesting plausible hypotheses) with Whewell, Peirce, and Hanson, though their methods for dealing with the problem differ considerably. These recent approaches to the problem fall roughly into three groups, and for purposes of classification I call these: (1) the Bayesian approach, (2) the analogical reasoning approach, and (3) the heuristic programming approach. Each of these approaches have characteristics of their own, and corresponding bodies of literature which discuss them. In the present section I will discuss each of these approaches in a rather general way, to point out that each of these approaches, either tacitly or explicitly, presupposes the existence of paradigms for their viability.

Perhaps, it should be made clear that though I believe there is brilliant, indeed fascinating, progress being made in each of these approaches to the problems, my intention here is primarily to show how paradigms play a key role in these approaches, and to suggest that a more systematic study of paradigms could aid in their progress.

Another point is worth noting here. The foregoing discussion has already suggested, and this should be quite clear by now, that the prospect of reconstructing a logic of discovery for "normal science" (*simpliciter*) is far more promising than that of finding a *general* logic of discovery which would include the introduction of new frameworks, such as those required in "crisis" situations. It was pointed out that this more limited task avoids many of the difficulties encountered by earlier philosophers, and can also serve as a first step toward a general theory of discovery. In the context of the contemporary approaches to the problem, it is important to note that many of these approaches already assume this more limited task as part of their basic *modus operandi*. Indeed, the distinction referred to here often appears in the psychological literature as a distinction between "concept formation" and "concept identification".[46] In concept formation, the conceptual scheme is changed or modified by the concepts introduced in hypotheses; in concept identification, hypotheses use only concepts already in the conceptual schema. Thus, by using the type of distinction which has been suggested in this essay, many contemporary approaches to the problem are proceeding unencumbered by the logical and psychological difficulties which beset their historical predecessors.

The first contemporary approach to be discussed here, which I simply call the Bayesian approach, has a rather peculiar history. It might be said that some Bayesian inductive logicians have recently developed an interest in the "context of discovery" as a result of certain technical problems encountered in confirmation theory. From the vantage point of the history of the problem of the logic of discovery, the Bayesians have entered the discussion through the side door, so to speak, *viz*., through the "context of justification". Thus, the origin of this approach is traceable to Mill's concern with inductive proof procedures, and the history of inductive logic in general.

The interest of some Bayesians in the context of discovery is motivated by a desire to understand the origin, or foundation, of "prior probabilities" -- a notion which is central to any application of Bayes' theorem, and is particularly important for assessing the inductive strength of putative confirmatory evidence. Wesley Salmon is one philosopher of science in particular, whose work shows an increasing interest in the context of discovery to understand the source of "prior probabilities". To better understand how it comes to be that Bayesian logicians are taking an interest in the context of discovery (via prior probabilities), consider the following statement by Salmon which expresses this connection:

> If Bayes' theorem provides a correct formal schema for the logic of confirmation and disconfirmation of scientific hypotheses, it tells us that we need to take account of three factors in attempting to assess the degree to which an hypothesis is rendered probable by the evidence. Roughly, it says, we must consider how well our hypothesis explains the evidence we have (this is what the H-D schema requires), how well an alternative hypothesis might explain the same evidence, and the prior probability of the hypothesis. The philosophical obstacle that has always stood in the way of using Bayes' theorem to account for confirmation is the severe difficulty in understanding what a prior probability could be. I have argued elsewhere that it is essentially an assessment of what one might call the <u>plausibility</u> of the hypothesis, prior to, or apart from, the results of directly testing that hypothesis.[47]

And further on Salmon adds: "The only crucial issue is the existence of such prior probabilities for use in connection with Bayes' theorem".[48] Thus, the progression, or development, of Bayesian interest in the context of discovery runs as follows: in order to apply Bayesian statistics to assessing the confirmatory strength of a hypothesis, one must have an assessment of the prior probability of that hypothesis prior to testing, but a prior probability is the same thing as the initial plausibility of an hypothesis, hence, in order to find out how hypotheses get their initial plausibility one must look at the "context of discovery". This line of reasoning is what I have dubbed the "side-door entrance" to the discovery problem, and Salmon is one of its leading proponents.

There is no difficulty in applying Bayes' theorem for predicting outcomes for finite sets of alternatives i.e., when the number of mutually exclusive possibilities is known in advance. For example, Bayes' theorem can be used effectively in situations such as rolling a fair die, or picking balls from an urn with a known content; in short, its use is non-problematic when the prior probabilities are known in advance. In science however, the number of possible hypotheses that could (i.e., logically could) explain the data is virtually infinite. So the problem of applying Bayesian statistics in scientific situations becomes one of: first, demarcating a class of plausible hypotheses from the infinite class of possible ones, and second, assigning relative values of probability for each plausible alternative. In actual practice however, there is no non-problematic (nor non-controversial) way of achieving even the first objective i.e., that of demarcating the class of plausible hypotheses prior to testing. The literature in inductive logic contains many proposals for determining, and in some cases simply assigning, values for the initial plausibility (i.e., prior probabilities) of hypotheses. For example, Rudolf Carnap in Logical Foundations of Probability (1959) attempts to develop a satisfactory formalized language whereby one can assign a priori weights to all statements, including, of course, all hypotheses; Leonard J. Savage in his influential book The Foundations of Statistics (1954) bases these assessments of plausibility, or prior probabilities, on a subjective judgement of plausibility, or "personal probability" as he called it; and the "frequentist" school (e.g., Reichenbach and Salmon) look for prior probabilities to emerge from some sort of success frequency for certain types of hypotheses. All of these proposals have fallen short of their promise as a general method for assessing initial plausibility of hypotheses in concrete investigative situations.

However, Wesley Salmon is one philosopher of science who has recently begun to look more closely at the history of science, and actual scientific practice, to determine how these prior probabilities (or plausibility judgements) are arrived at de facto.[49] He says, for example:

> I am strongly inclined to believe that the Bayesian schema comes closer than the H-D schema to capturing actual scientific practice, for it seems to me that scientists do make substantial use of plausibility considerations, even though they may feel somewhat embarrassed to admit it. I believe also that practising scientists have excellent intuition regarding what constitutes sound scientific methodology, but that they may not always be especially adept at fully articulating them. If we want the soundest guidance on the nature of scientific inference, we should look carefully at scientific practice, rather than the methodological pronouncements of scientists.[50]

Salmon is suggesting that an examination of actual scientific practice is the most direct way to learn how prior probabilities are arrived at. And prior probabilities are necessary for Salmon's program since they afford the universe of discourse, so to speak, for which (or <u>within</u> which) Bayes' theorem can be applied as an inference rule. Bayes' theorem, as an inductive inference rule, is useless without prior probabilities. In this context, prior probabilities serve the same function as formation rules in deductive logic -- without them, the inference rules are ineffective.

Salmon's proposal to examine scientific practice to discover how plausibility judgements are made, and how prior probabilities are assessed, is sound advice. This is, after all, what Whewell, Peirce and Hanson have been advocating for the past hundred years. However, Salmon's own research into this question has yielded much vaguer, and somewhat stark, results. For example, he notes that certain types of hypotheses such as "the simpler ones" or the more "aesthetic ones" have a higher frequency of success in science than other candidates.[51] And at times Salmon is somewhat more specific by pointing out, for example, that in the biological sciences mechanical models of explanation are to be preferred over teleological ones, just as they are in the physical sciences. In general, however, Salmon's work in this area is more to be commended for its admonitions than its specific results; he says, for example:

> If I am right in claiming not only that prior probabilities constitute an indispensible ingredient in the confirmation of hypotheses and the context of justification, but also that our estimates of them are based upon empirical experience with scientific hypothesizing, then it is evident that the history of science plays a crucial, but largely unheralded, role in the current scientific enterprise. The history of science is, after all, a chronicle of our past experience with scientific hypothesizing and theorizing -- with learning what sorts of hypotheses work and what sorts do not.[52]

My suggestion is: what better way to learn about what Salmon is calling for than the study of scientific paradigms, and how paradigms influence the suggesting of plausible hypotheses? Not only has Kuhn's work gone further than Salmon's toward showing how paradigms effect the plausibility judgements of science, but it has also gone a long way toward showing how prior probabilities are contingent upon the existing paradigm. This is what was meant earlier when I suggested that paradigms are a presupposition of the Bayesian approach to the logic of discovery problem. Paradigms describe how, and why, certain types of hypotheses (e.g., paradigm-based hypotheses) are regarded as more plausible than others.

Moreover, in the revised edition of his book, Kuhn describes a paradigm as a "disciplinary matrix": "'disciplinary' because it refers to the common possession of the practitioners of a particular discipline";[53] and 'matrix' because, in effect, it delimits the parameters of reasonable suggestions, both in terms of the problems to be solved and of the type of solutions to be accepted. What I am suggesting here is that in normal science, the scientific paradigm (or "disciplinary matrix") is the source of prior probabilities: the probabilities for which Bayesians have been trying to discover the origin. From this point of view, the Bayesian approach to the discovery problem might be well advised to end its speculation about general types of hypotheses in science (e.g., Salman's) and begin looking at the specific inter-workings of paradigms in normal science. Perhaps this would illuminate the precise source of those prior probabilities which have hitherto been so illusive.

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Another recent approach to the problem of the logic of discovery is to develop what might be called "analogical reasoning". Hanson had suggested this as being a fundamental characteristic of retroduction, though Hanson never developed it to any useful degree. And more recently Kuhn himself has turned his attention to the role which models and analogies play in scientific reasoning.[54]

There is little question that analogies and analogical reasoning play a role in suggesting plausible hypotheses; however, most of the work in this area has focussed on the epistemological and psychological aspects of <u>seeing something as being like</u> something else e.g., seeing this problem or data as like a problem or data which has been encountered before. While such studies are indispensable for a fundamental understanding of analogical reasoning in science, there have been very few attempts to actually formalize this type of reasoning as distinct from straight-forward inductive reasoning as such. Indeed, some logic books simply refer to analogical reasoning as a form (or type) of inductive reasoning.[55] For all the similarities between the two types of reasoning however, from the point of view of the "context of discovery" the differences should not be overlooked because the differences are significant. Not only can the differences be seen from the fact, as Hanson argued, that inductive arguments can be used to prove hypotheses whereas analogies cannot, but more importantly, in the context of discovery analogies serve only a heuristic role in suggesting which data, and properties of data, might be relevant for hypothesis formation; the hypothesis can then be checked and tested by normal inductive methods. In short, straight-forward inductive techniques do not (and cannot) by themselves pick out the relevant respects in which one problem (or set of data) may be like another, since the possibilities are virtually infinite. This is the point on which Whewell criticized Mill's "canons of induction". In practice, analogies assist in delimiting the infinity of possibilities to a more manageable set of plausible ones.

Assuming then, at least from a methodological viewpoint, that analogical and inductive arguments are different, let us look briefly at the problems involved with developing analogical reasoning into a logic of discovery.

In his textbook on logic Salmon has pointed out that:

> The strength of an analogy depends principally upon the similarities between the two types of objects being compared. Any two kinds of objects are alike in many respects and unlike in many others. The crucial question for analogical argument is this: are the objects that are being compared similar in ways which are relevant to the argument?[56]

Kuhn, for example, has tried to examine the ways in which scientists learn to select the relevant data in analogical reasoning, and he explicates his view via the notion of an "exemplar", i.e., certain models, and "symbolic generalizations" serve as exemplars for the solution of other problems and sets of data. Kuhn asks: "How have they learned, faced with a given experimental situation, to pick out the relevant forces, masses, and acceleration?" And he answers, in effect, that it is a skill developed as part of the training for paradigm membership. He points out that:

> In practice ... it is not quite the case that logical and mathematical manipulations are applied directly to f=ma. That expression proves on examination to be a law-sketch or a law-schema. As the student or the practising scientist moves from one problem situation to the next, the symbolic generalizations to which such manipulations apply change. For the case of free fall,
>
> f=ma becomes $mg = m\frac{d^2s}{dt^2}$; for the simple pendulum it is
>
> transformed to $mg\sin\theta = -ml\frac{d^2\theta}{dt^2}$;
>
> for a pair of interacting harmonic oscillators it becomes two equations ... and for more complex situations, such as the gyroscope, it takes still other forms, the family resemblance of which to f=ma is still harder to discover.[57]

Kuhn's general point is that when a student of a given science learns to understand the past achievements of the science, some of which may have been instrumental in establishing the paradigm from the start, he then uses these achievements (expressed in symbolic generalizations) as models for approaching future problems.

> The student discovers, with or without the assistance of his instructor, a way to see his problem as <u>like</u> a problem he has already encountered. Having seen the resemblance, grasped the analogy between two or more distinct problems, he can interrelate symbols and attach them to nature in the ways that have proved effective before. The law-sketch, say f=ma, has functioned as a tool, informing the student what similarities to look for, signalling the Gestalt in which the situation is to be seen.[58]

Thus, for Kuhn, the successful experience of one generation of science is passed on to the next by employing certain exemplars in analogical reasoning.[59]

In the earlier discussion of Hanson's views on analogical reasoning it was pointed out that analogies are relational in character (i.e., analogies exist between relata), and that Hanson had not taken into consideration the relevant background information which constitutes one part of the analogical relation. By contrast, it can be seen that Kuhn's work on analogies in sciences is far more informative, and that it marks a further step in a promising direction. In particular, Kuhn's analysis of precisely how analogies are generated and used in actual situations is far more precise than any previous efforts in this direction. As Salmon has pointed out, it has always been the case that:

> To evaluate the strength of an analogy it is necessary to determine the relevance of the respects in which the objects of different kinds are similar. Relevance cannot be determined by logic alone -- the kind of relevance which is at issue in analogical arguments involves factual information ... These arguments, like most inductive arguments, occur in the presence of a large background of general knowledge, and this general knowledge must be brought into consideration to evaluate the strength of analogies.[60]

Paradigms furnish this background information, moreover, they furnish models and methodological rules to assist in suggesting and evaluating analogical arguments. It is in this sense that paradigms are presuppositions of the "analogical reasoning approach" to the problem of the logic of discovery.

The point of this discussion on analogies has not been to discourage, nor criticize, further work in this area; on the contrary, some of the work in this area shows considerable promise. The point here has been to suggest that analogies, like beauty, are "in the eyes of the beholder"; and that the study of paradigms can augment our understanding of how specific types of analogies come to be employed in actual scientific practice.

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Another area of research which has far-reaching implications for the problem of the logic of discovery is that of general problem-solving in systems science and operations research. I call this the "heuristic programming approach" since the most important concept in this work is that of a heuristic. It should be noted at the outset however, that the people working in this area do not describe (nor consider) themselves as working on "the problem of the logic of discovery" as it has been known in philosophy.[61] The origin of this work is to be found in the history of cybernetics, and its driving force is the practical problem of harnessing computers to solve human problems: one class of which is that of suggesting plausible hypotheses in science. Thus, as a contingent matter of fact, the recent work in heuristic programming overlaps with the historical concern of developing a logic of scientific discovery.

A systematic survey of the progress in this area of research would be impossible in this context, not only because of space limitations, but primarily because the literature in this field is presently accumulating at an exponential rate. For purposes of the present essay it will be sufficient to note some of the more important points of contact between the relevant distinctions in this essay and those employed in heuristic programming. By simply reading four or five of the more basic (or land-mark) papers in the area of heuristic programming, one can immediately see that their efforts at problem-solving have confronted the same sorts of difficulties that the reconstruction of a logic of discovery has confronted.[62] But more importantly than this however, one also sees that the research in heuristic programming has already put-to-work many of the distinctions (albeit in different jargon) which this essay has argued for as being fundamental for a logic of discovery.

Basically, "heuristic programming" is an attempt to design general computer programs which are capable of solving problems for which there exists no effective, or no algorithmic, method of finding solutions. Sometimes this undertaking is described as an attempt to design methods for solving "ill-structured", as opposed to "well-structured" problems.[63] A "heuristic", as such, is simply a rule of thumb, or device, which aids in the discovery process; but they do not offer infallible guidance, nor guarantee success. Marvin Minsky simply states that:

> By "heuristic" we mean to refer to things related to problem solving. In particular we tend to use the term in describing rules or principles which have not been shown to be universally correct but which often seem to be of help, even if they may also often fail. The term "heuristic program" is thus used in reference to the distinction between programs which are guaranteed to work (and are called 'algorithms') and programs which are associated with what the programmer feels are good reasons to expect some success. The term 'heuristic' has come into use as a noun as well. We ask someone 'what heuristics does your program use?' and he answers with a listing of particular tricks, short-cuts, inductive bases, and the like, regarding each heuristic feature as an individual 'heuristic'.[64]

Thus, the basic idea of a 'heuristic', and the development of heuristic programs, can be seen to correspond perfectly with a point made in Chapter I of this essay, viz., that a logic of discovery need not be a deductive, nor algorithmic, system of discovering hypotheses such as that discussed by Descartes, M. Bunge, and Hemple; nor need it be a simple matter of serendipity, or psychology, as was suggested by Popper, Braithwaite, and Rudner. It was pointed out (Chapter I) that between these extreme positions there is a spectrum of possibilities for a logic of discovery, and heuristic programming is just one of the alternatives.

Among the many accomplishments in this area of problem-solving research (i.e., heuristics) are such thing as the successful design of a computer program which can discover proofs to theorems in the sentential calculus of Whitehead and Russell,[65] also, heuristic checkers and chess programs,[66] and heuristic programs to solve algebra word-problems.[67] Perhaps the most significant feature of programs which solve problems via heuristics is that they resemble the methods actually used by persons in solving problems; that is, they do not go through an exhaustive list of all possible solutions available. If an exhaustive search for solutions were required it would be of little use in most scientific problem situations; moreover, problems which are amenable to exhaustive search are, in themselves, uninteresting; for as Minsky writes:

> In one sense all such problems are trivial. For if there exists a solution to such a problem, that solution can be found eventually by any blind exhaustive process which searches through all possibilities. And it is usually not difficult to mechanize or program such a search.
>
> But for any problem worthy of the name, the search through all possibilities will be too inefficient for practical use ... a search of all the paths through the game of checkers involves some 10^{40} move choices, in chess, some 10^{120}.[68]

Heuristic programming does not employ exhaustive search methods, but rather, it employs selective search: and the better the heuristic, the better are its selections for trial solutions. Thus, the conception of a heuristic, and heuristic programs, is particularly fitting for the types of problem situations encountered in science.

In passing, it is interesting to note that if one had a completely successful heuristic device, which could guarantee success, one would then have an algorithm, which is equivalent to having a formal decision procedure. In such a case however, one would no longer have a "heuristic" because, by definition, heuristics are simply rules of thumb which do not guarantee success i.e., they are "non-effective" rules, or aids, to discovery.

The problem of a logic of discovery, which is to find a systematic method for suggesting plausible hypotheses, resembles the problem of finding heuristics in problem-solving. For example, the problem of finding heuristics for making good chess moves,[69] has the same features as the problem of selecting plausible hypotheses from the large class of possible ones: in both cases, the problem is to devise methods which will search through a set of possible solutions for a subset of the best ones e.g., chess moves or hypotheses. However, good heuristics, like good hypotheses, are not typically found by accident: their discovery requires painstaking research and development. Indeed, it might be argued that to rely on heuristics for a solution to the logic of discovery problem, is simply to move the problem back to another level where the same sorts of difficulties must be faced. This argument has much to recommend it, since the similarities between the two sets of problems are far more than tangential. In the present context however, this is not the point. What is important to note here is the fact that heuristics, like the other approaches to the discovery problem, relies on the models, techniques, and information of scientific paradigms for their development. This reliance of heuristic programs upon paradigms and normal science is not simply a contingent, nor casual, dependence either: heuristic programs employ the "current theory" (of the selected science) as an integral part of the program itself. For example, in "Heuristic DENDRAL",[70] which is to date the most successful program for suggesting hypotheses in actual scientific problem situations (i.e., organic chemistry), the program not only employs the existing paradigm theory, language, and data, but its use is restricted to a particular piece of mechanical apparatus (i.e., a mass spectrometer) in that discipline. This is not to discredit the success, indeed usefulness, of such programs in any way; my intention here is merely to point out that the feasibility, indeed usefulness, of such programs are shot-through with paradigm-determined contingencies. Perhaps an "acid test" for my view that heuristic programs are dependent on existing paradigms (in non-trivial ways) would be to note the extent to which the design of heuristic programs would have to change with a change or revision in paradigms. With a change in paradigms not only does the specific data change, but the <u>nature</u> of the data, and its corresponding relations are so different from the previous paradigm, that what was once regarded as plausible is now no longer in consideration. The design of heuristic programs would have to change considerably in order to reflect this change in plausibility assessments. A good heuristic in one paradigm, would not be a good heuristic in another paradigm. Moreover, if one were to design a heuristic program to be general enough so as to work across paradigms, or even in other areas, its directions (i.e., its suggestions) would be so <u>non-specific</u> that it would be virtually useless as a heuristic device. An example of one such prescription is

"Step 3: suggest classes which contain plausible hypotheses by looking at the most significant features of the data".[71] But such suggestions beg all the interesting and difficult questions which face a logic of discovery e.g., how does one know which "features" are the most "significant", or which class of hypotheses is the most "plausible"? When heuristic programs attempt to generalize their range of applicability they tend to lose the specificity which makes them useful as heuristic devices.

To date, however, when heuristic programs are designed to do specific tasks within a discipline, such as Heuristic DENDRAL in organic chemistry, they come quite close to being a logic of discovery for that area. One might want to withhold that laurel however, on the grounds that its range of applicability is too specific, or overdetermined. But again, as R. D. Carmichael pointed out as early as 1930:

> It is conceivable that the logic of discovery is not one in the sense of something indivisible, but that it is relative to the field of investigation, or the point of view so that one should not speak of a logic of discovery in any absolute sense, but only of such a logic as relative to a given discipline or a given goal of investigation.[72]

One might also want to withhold the title "logic of discovery" to certain heuristic programs on the grounds that they are not sufficiently formalized. Presumably such an argument might hold that the mere writing of a computer program (e.g., a heuristic program) is not equivalent to the formalization common to logical systems. My own view of this matter would be to suggest that such an argument would eventually result in quibbling over the meanings of words such as 'formalization', 'logical system' and 'program'. It is clear to me at least, that some heuristic programs capture the spirit, if not the letter, of a logic of discovery -- albeit with a limited range of applicability.

All such arguments aside however, the main lesson to be learned from heuristics, as from the other approaches to the discovery problem is that there is much to be learned about the generation of plausible hypotheses from the phenomenon of scientific paradigms, and the practice of normal science. There is a sense in which many of the approaches discussed above have already recognized this, at least tacitly, and all that remains is to have philosophers take heed of it.

5. A Note on Creativity and Discovery

As was pointed out earlier, some philosophers working on the problem of the logic of discovery have had difficulty reconciling what appear to be two conflicting aspects of the discovery process: on the one hand, the discovery process often seems to require "creative genius", while on the other hand they were attempting to reconstruct a logic to the process. As Herbert Simon once pointed out to a computer conference: "Combining the words 'Creativity' and 'The Computer' in the title of this conference is a little bit like combining Beauty and the Beast."[73]

It was pointed out, for example, that Whewell, contemplating the novelty of Newton's hypothesis of universal gravitation, seemed to throw up his hands and conclude that "an Art of Discovery is not possible". In many places throughout Hanson's work we find him lavishly praising the "genius" of many historical discoverers, while at the same time he (Hanson) was arguing that these discoveries employed "retroductive inference". And Popper simply states: "My view may be expressed by saying that every discovery contains 'an irrational element', or a 'creative intuition' in Bergson's sense".[74] It is clear, at least from the point of view advanced in this essay, that these latter views failed to take due recognition of the distinction between discoveries within normal science, and discoveries which bring about new paradigms, or resolve the anomalies of a science in crisis. Once this distinction is recognized, then the apparent conflict between "creative genius" and "logical process" disappears. Normal science does not require, and usually disfavours, the introduction of novel concepts into the available paradigm; thus, creativity is not a typical prerequisite for successful paradigm-based research. This is not to say that "creative intuition" <u>never</u> helps to advance the progress of normal science, but it is to suggest, as Pasteur argued long ago, that: "Revealing insight plays a minimal role in scientific discovery" and that "chance favours only the prepared mind".[75] However, in "crisis" situations, where a researcher cannot rely on the guidelines of a secure research tradition, he is left to his own wits, and "creativity", or something like it, is highly desirable.

In normal science, where the suggested hypotheses are typically within the parameters of a paradigm, the prospects for a logic of discovery appear to be quite good: perhaps even by way of the several approaches discussed above. However, for science in crisis, or anomaly resolution, the prospects for a logic appear to be much less, precisely <u>because there is no paradigm available to aid in the resolution</u>. In these latter situations, "insight" and "creative intuition" are all that one has to guide him. Indeed, it is these latter discoveries which histories tend to record, and we tend to remember. Histories of science, like histories of nations, point up moments of drama and sudden change; perhaps this is as it should be. However, from the point of view of the logic of discovery, this tendency does poor justice to the cumulative discoveries which take place in normal science; and it is here that a logic is most apt to be found.

One of the particular virtues of looking at the problem of discovery from the perspective of paradigms, is that it enables one to work on a method, and thence a logic, without the immediate encumbrance of the _problem of creativity_. And given the history of this problem, it would appear that this is not a small advantage.

FOOTNOTES

1 R. G. Collingwood, The Idea of History (Cambridge, 1953), p. 281.

2 T. S. Kuhn, The Structure of Scientific Revolutions, revised edition, (University of Chicago Press, 1970).

3 Personal communication, March 27, 1969.

4 The Idea of History, op. cit., p. 281.

5 Ibid., p. 281.

6 Ibid.

7 Kuhn, op. cit., p. 37.

8 Ibid., p. x, and p. 102.

9 It is worth noting however that none of the popular literature in the philosophy of science has seriously investigated the relationship between scientific questions and hypotheses. I think the relationship is, as my discussion suggests, much more obvious and important than has been recognized; if this is in fact the case, then a study of the logic of questionsmight afford an alternative route to the problem of the logic of discovery.

10 Criticism and the Growth of Knowledge, eds. Imre Lakatos, Alan Musgrave, (Cambridge, 1969).

11 The Structure of Scientific Revolutions, op. cit., p. 10.

12 Ibid., pp. 23-24.

13 Ibid., p. 43.

14 Ibid., pp. 10-11.

15 Ibid., p. 60.

16 Ibid., pp. 64-65.

17 Ibid., p. 77.

18 Ibid.

19 Ibid., p. 90.

20 Science and Subjectivity, (New York, 1967), p. 76.

21 Criticism and the Growth of Knowledge, op. cit., p. 132.

22 Personal communication, March 27, 1969.

23 It is assumed that no decision procedure holds for the logic under consideration here.

24 University of Illinois Symposium on Scientific Theories and Conceptual Change, Urbana, Illinois, March, 1969.

25 It should be understood that while the strict parallel between logical problem-solving and science breaks down, my position is that the parallel with logic is far more useful and precise than the socio-psychological account offered by Kuhn. My view on this concurs with those of Imre Lakatos (op. cit.) who argues that rational reconstructions, of this sort, are to be favoured over the irrationalism (e.g., sociology and psychology) of scientific change found in Kuhn.

26 See his "The Duhemian Argument" Philosophy of Science, 27, 75-87, (1960); "Law and Convention on Physical Theory", Current Issues in the Philosophy of Science, op. cit.; and "Geometry, Chronometry and Empiricism", Minnesota Studies in the Philosophy of Science, Vol. III, (Minneapolis, 1962).

27 Criticism and the Growth of Knowledge, op. cit., p. 144.

28 Whewell, op. cit., p. 144.

29 Ibid., p. 278.

30 Ibid., p. 144.

31 Ibid., p. 278.

32 Collected Papers, op. cit., Vol. V, 5.189, p. 117.

33 This point, indeed this criticism, was argued for earlier, Chapter III.

34 Collected Papers, 5.171.

35 It might be argued that by restricting the problem to "normal science", where new concepts are not required, I have, in effect, recast the problem from antecedent historical efforts. However, my response to this would be that I have deliberately recast the problem in this way to avoid the persistent errors of past attempts, while at the same time preserving the central issue. I have simply modified the problem to a more manageable form; in sum: the problem needs recasting.

36 Perception and Discovery, op. cit., p. 15.

37 Patterns of Discovery, op. cit., p. 119.

38 "Notes Toward a Logic of Discovery", op. cit., pp. 52-53.

39 Patterns of Discovery, op. cit., p. 203.

40 "Leverrier: The Zenith and Nadir of Newtonian Mechanics", Isis, 53 (1962), pp. 359-378.

41 Op. cit., p. 103.

42 Hanson did not recognize however, that this use of Newtonian mechanics violates his general statements about the components of discovery.

43 "Is There a Logic of Discovery", op. cit., p. 27.

44 Ibid.

45 Structure of Scientific Revolutions, revised ed., op. cit., p. 184.

46 See P. Suppes "Concept Formation and Bayesian Decisions", Memorandum TM-1262, System Development Corporation (Santa Monica, 1963), p. 2.

47 "Bayes' Theorem and the History of Science", Minnesota Studies in the Philosophy of Science, Vol. IV (University of Minnesota Press, 1970) p. 22. The present discussion also relies on Salmon's The Foundations of Scientific Inference, (Pittsburg, 1967).

48 Ibid., p. 23.

49 See his "Bayes' Theorem and the History of Science", (1970), ibid.

50 Ibid., p. 26.

51 Ibid., p. 32.

52 Ibid., p. 33.

53 Structure of Scientific Revolutions, op. cit., p. 182.

54 See in particular Kuhn's "Postscript" to Structure of Scientific Revolutions, ibid., pp. 187-190. Other work on analogies can be found in Mary B. Hesse's Models and Analogies in Science, (Notre Dame, 1966); E. H. Hutton "The Role of Models in Physics", British Journal of Philosophy of Science, IV, 1953, p. 284; T. G. Evans, "A Heuristic Program to Solve Geometric Analogy Problems", Proc. Spring Joint Computer Conference, Vol. 25, 1964, pp. 327-338.

55 See Wesley C. Salmon, Logic (Englewood Cliffs, 1963) p. 70.

56 Logic, ibid., pp. 70-71.

57 Structure of Scientific Revolutions, op. cit., p. 189.

58 Ibid., p. 189.

59 Kuhn develops this line of thinking in far greater detail than space will permit discussing here. I have, I think, captured the general thrust of his remarks.

60 W. Salmon, Logic, op. cit., p. 73.

61 One interesting exception to this is the work of C. West Churchman and Bruce G. Buchanon in "On the Design of Inductive Systems: Some Philosophical Problems", British Journal of Philosophy of Science, Vol. 20, (1969), pp. 311-323.

62 Many of these papers are included in the following sources: E. Feigenbaum and J. Feldman (eds.) Computers and Thought, (New York, 1963); A. Newell, J. Shaw and H. Simon, Elements of a Theory of Human Problem Solving, Paper P-971, Santa Monica, The Rand Corp., March, 1957; M.L. Minsky, "Some Methods of Artificial Intelligence and Heuristic Programming", Mechanization of Thought Processes, (London, 1959); C. West Churchman, On Inquiring Systems, Report #SP-877, Systems Development Corp. (Santa Monica, 1962); P. Suppes "Concept Formation and Bayesian Decisions", Systems Development Corp. (Santa Monica, 1963).

63 A. Newell, "Heuristic Programming: Ill-Structured Problems", to appear in Progress in Operations Research (in press).

64 "Some Methods of Artificial Intelligence and Heuristic Programming" op. cit., p. 36.

65 See A. Newell, J. Shaw and H. Simon, "Empirical Explorations of the Logic Theory Machine", Proceedings of 1957 Western Joint Computer Conference, Feb. 1957; also A. Newell and H. Simon, "The Logic Theory Machine", Transactions on Information Theory, IT-2, No. 3, September 1956.

66 N. J. Nilsson, "A New Method for Searching Problem-Solving and Game Playing Trees", Working Paper (Artificial Intelligence Group, Stanford) November 1967; A. Newell and H. Simon, "An Example of Human Chess Play in the Light of Chess Playing Programs", Working Paper (Computer Science Dept., Carnegie-Mellon U.), August 1964.

67 J. Paige and H. Simon, "Cognitive Processes in Solving Algebra Word Problems", in B. Kleinmuntz (ed.) Problem Solving: Research, Method and Theory, (New York, 1965).

68 "Steps Toward Artificial Intelligence", Computers and Thought, op. cit., p. 408.

69 Discussed by A. Newell, J. Shaw and H. Simon in "Chess-Playing Programs and the Problem of Complexity", Computers and Thought, op. cit.

70 See B. Buchanon and G. Sutherland, "Heuristic DENDRAL: A Program for Generating Explanatory Hypotheses in Organic Chemistry", Artificial Intelligence Working Paper No. 62, Stanford University Computer Science Dept., to be published in Machine Intelligence, eds. D. Michie et. al.

71 See C. West Churchman and Bruce G. Buchanon, "On the Design of Inductive Systems: Some Philosophical Problems", op. cit., p. 316.

72 Logic of Discovery, op. cit., p. 9.

73 Reprinted in "Understanding Creativity", Creativity: Its Educational Implications, J. C. Gowan et. al. (eds.), (New York, 1967), p. 43.

74 Logic of Scientific Discovery, op. cit., p. 32.

75 Quoted from The Language of Psychology, G. Mandler and W. Kessen (eds.), (New York, 1963), p. 246.

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