1 Full Circle: The Return to Discovery

Novelty and method. These are the features we most commonly and instinctively associate with scientific inquiry.

Novelty. In the wake of the unprecedented expansion of our knowledge of the physical world brought about by early scientific investigations, it was natural that science should have been taken as a particularly effective means for uncovering new truths. Even among those early scientists for whom science was scarcely to be distinguished from mathematics, there was the conviction that it was an instrument of projection and amplification, not merely one of logical proof; and that these results were novelties of experience and not just of thought. While Galileo did not value empirical confirmation as an essential component of his method, for example, he did insist that new theories emerged from the facts—that empirical experience was at least the material source of the novelty of our knowledge.

Method. But experience without a pattern of inference, while it might supply many new facts to contemplate, is not yet a method. From the very beginning, scientists have believed that what they do relies in some fundamental way on the particular form of reasoning they employ. Few have challenged this judgment. This is why the term "scientific method," as it is used by many educated persons ignorant of the finer points of controversy, seems to denote something quite as real and familiar as the term "justice." We may have difficulty defining it, but we don't doubt its existence.

This wedding of a mathematical method and the empirical discovery of novelties gave birth to some puzzling verbal formulations, of which Newton's "deduction from the phenomena" is perhaps the most famous. None of these early thinkers would have suspected that this marriage would one day end in divorce. But if the philosophy of science of the last one hundred years has taught us anything, it is that novelty and method are not easily integrated into a seamless "science."

Strains in the marriage were hardly evident even through the nineteenth century, where thoughtful theories of induction and confirmation were profitably advanced; even if there was not universal agreement about the method, there was at least nothing to indicate that the whole enterprise was in jeopardy. Such a method would be both a means for "generating new theories and, because infallible, . . . would automatically guarantee that any theories produced by its use were epistemically well grounded" (Laudan 1980, p. 176). Laudan claims that those interested in novelty (the generation of new theories from the facts) were really primarily concerned with method (justification) all along. He also suggests that it was because seventeenth- and early eighteenth-century writers were suspicious of deep-structural explanatory theories, concentrating instead on detecting empirical regularities as a basis for forming empirical laws, that most generative logics of discovery were essentially inductive.

While such a theory of science was simplistic, its interest in the generation of theories did not, as Laudan suggests, arise merely by default from its passion for induction, which itself arose from its suspicion of deep theory. This links an interest in discovery directly to simple empiricism. You don't have to be an empiricist committed to classical induction in order to hold that the facts-to-be-explained play a decisive role in the generation of the theories which come to explain them (neither Galileo nor Newton embraced this species of induction); nor does the rejection of the inductivist model entail the rejection of a methodology of discovery.

Method without Novelty

Nevertheless, Laudan is right that discovery was in fact rejected along with induction. The effect of the "theoretical turn" toward deep explanation was a rejection of any account of the generation of theories from the facts (and of ampliative inference generally) in favor of exclusively consequentialist methodologies. The discovery question was linked at least de facto with the abandoned inductivist programs, while consequentialist justification came to be associated with this new emphasis on theory—on the explanatory function of theories, on the theory-ladenness of observations, and on the priority of theoretical paradigms in the evolution of scientific systems. Method prevailed over novelty.

The grounds for divorcing method from novelty was the division between justification and discovery. It radically reoriented our understanding of science, for the natural association between the growth of science and the generation of scientific theories was broken. Progress ("discovery" in a broader sense[1]) came to be tied to justification alone. There is no need to document the extent to which this view still represents the orthodox position on discovery. The general sentiment famously expressed by Popper in 1934 still probably speaks for the majority:

> The initial stage, 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 sci-

> entific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. . . . [T]here 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 Bergson's sense. (1959, pp. 31–32)

That is, novelty and method are incompatible *in principle*.

In addition to the new appreciation of deep explanatory theory, Laudan explains the abandonment of generative logics of discovery and the adoption of consequentialist (post hoc) evaluative methods in their place also as the result of questioning the infallibilist assumptions which precluded post hoc evaluation on the grounds of the latter's running afoul of the fallacy of affirming the consequent (pp. 177–81). But surely another reason was the problem of the validity of inductive inference itself. The problem is not just that inductive generalizations fail to constitute deep explanations (however the latter might be defined), but also that such generalizations are taken to be simultaneously demonstrative and ampliative (valid inferences capable of generating novelties), a double requirement that non-deep theories may fail to satisfy independently of the question of their shallowness.

Consequentialism freed theories of method from what had become the dead weight of the purely inductive programs. Eventually, however, a number of writers, beginning with Hanson (1958), began to suggest that too much had been given up in this radical reorientation. He perceived that the discovery question needn't be tied to traditional empiricism at all. He insisted on the theory-ladenness of observation precisely because he wanted to prepare the way for a new account of the inference of explanatory hypotheses from the facts. The closest Hanson himself came to providing an account of discovery was to utilize gestalt metaphors for the inference from part to organized whole (an inference not based on inductive generalization)—a conception he never characterized in any satisfactory way.

Instead of offering a truly generative account, Hanson was content to argue that there was more to scientific method than justification. *Plausibility* arguments on behalf of an already generated but not yet justified hypothesis formed a distinctive and legitimate component of method. Much of the interest in this intermediate context of "prior assessment," "preliminary evaluation," and "pursuit" dates from Hanson's pioneering effort. In the end, however, Hanson's *Patterns of Discovery* was almost as disappointing as Popper's *The Logic of Scientific Discovery*, for neither book would admit the possibility of a logical analysis of the actual generation of scientific hypotheses from the facts, that is, of what many would intuitively take to be "discovery." I will provide an analysis of this double failure in chapters 8 and 9.

Because logics of pursuit and plausibility restrict themselves to a twilight zone of inquiry, there has been pressure to reassimilate them to the strategies of either generation or justification. It has been argued (Kordig 1978, for example) that evaluating the plausibility of a hypothesis (whether it should be pursued) is not qualitatively different from evaluating a hypothesis with a view toward its justification; on the other hand, some have argued that pursuing a hypothesis may involve the actual generation of that hypothesis—thereby reopening the question of a generative logic of discovery (Achinstein 1970, Simon 1973, Gutting 1980, Nickles 1980b, Wartofsky 1980, Brown 1983). These proposed new generative "logics" are not, needless to say, reversions to inductivism or attempts to defend the traditional model of abduction.

These logics concentrate on a variety of problem-solving constraints that succeed in narrowing the range of possible new hypotheses. Such constraints include models and analogies (which are among Gutting's "regulative principles of abduction," 1980) as well as, according to Nickles (1980a), "most any item of information, any more or less established or accepted 'law', principle, rule, or fact which helps to set the problem by imposing a condition on its solution" (p. 285). These constraints, Nickles says elsewhere (1980b), "rarely will constitute the solution to the problem in the desired form . . . but they do implicitly specify the range of acceptable answers" (p. 37).

Insofar as these constraints represent the contributions of already achieved theoretical knowledge, however, they provide no more than sophisticated processes of elimination, and advance the problem no more than Popper and Hanson have done. However, if the novel facts to be explained are included among these constraints, a new path to a generative logic of discovery may be discerned. Those who have included such facts among prior constraints (such as Nickles), however, have not singled them out as functioning in a epistemically different way from other constraints and, where deep-structural theories are concerned, have treated them as secondary. In doing so, they have squandered an opportunity. I will return to all of these questions after my own proposals are developed.

Logics of pursuit and plausibility have been productive and illuminating, but their ultimate benefit may be indirect: in shifting the attention of philosophers back to the process of generating hypotheses, they may have prepared the way for reopening what may be the most philosophically provocative and potentially constructive question of all: How may novelties be generated rationally? While Laudan doubts the very legitimacy of any logic of discovery, he has, almost tantalizingly, left the door open a crack:

> While it is true that logics of discovery that involve some form of enumerative induction are taken seriously only by philosophers who believe that "observational laws" typify scientific inquiry, there are other *non*inductive logics of discovery which have been invoked by those concerned with the analysis of full-blown theories. . . . I shall call these "self-corrective logics of discovery." Such "logics" involve the application of an algorithm to a complex conjunction which consists of a predecessor theory and a rele-

> vant observation (usually one that refutes the prior theory).... Unfortunately, a century and a half of exploration by successive major thinkers failed to bring the self-corrective program to fruition. (1980, pp. 179–80)

Laudan (1973) complains that no one bothered to show that any of the methods actually proposed by methodologists such as Hartley and LeSage were really self-corrective (p. 234). He then proceeds to show how this interesting idea was trivialized by Peirce, who relied on a more formal probabilistic approach. Following his criticism of Peirce, Laudan notes that he hadn't really taken up the question of "whether full-bodied self-correction in the traditional sense is beyond our powers of explication" (p. 245).

It is such an explication that I offer. My reformulation of the problem of discovery would return us to a version of these self-corrective logics and would bring the philosophy of science full circle—back home to its original intuitive appreciation of a rational method for generating novelties.

Novelty without Method

More recent developments in the philosophy of science are positioned on the other side of the discovery-justification distinction. Where hypothetico-deductive and Popperian methodologies have committed themselves to a justification-without-discovery program, post-Kuhnian approaches reverse this priority, accepting the challenge to account for the novelty in discovery, while abandoning the idea that such amplification occurs through some binding formal method. This new historicism dissolves, rather than solves, the problem of a logic of discovery.

At first, this approach appeared to be a continuation of the new possibilities explored by Hanson. With Hanson and Kuhn, the monolith of consequentialism seemed more vulnerable. I initially welcomed these developments, for history shows us generation, and a theory of generation, it seemed to me, pointed to a logic of discovery.

But things have taken an unexpected turn. Discovery is now taken so broadly by historicist philosophers of science that what was initially interesting about it as a logical problem has been lost. Because a formal (logical and epistemological) theory was wanting, discovery was swallowed by empirical studies. "Logics" of discovery yielded to soft "heuristics" which uncover regularities in empirical cases and use them to construct rules or maxims to guide research. The problem is that, while historicist methodologists may thus maximize the extent to which their theories are adequate to the facts of actual cases, their new empiricism may not provide fully explanatory theories of inquiry.

Such efforts are by no means misdirected or valueless—on the contrary, direct frontal attacks from the theoretical side had either failed to formulate a distinctive epistemology of discovery or had stalled; it was time for more empirical strategies, for immersion into case studies, for the philosophical equivalent of curve-fitting. A new theoretical approach, however, should also have its place and value in the scheme of things. While Kuhn may have made a good case for taking seriously the fact that our acceptance of a new scientific paradigm cannot be made logically compelling by the constraints of formal method, he did not suggest that such a formal approach to our understanding of science was unnecessary, but only that it was insufficient.[2]

The particular interpretation of discovery offered here has not been advanced elsewhere and the arguments to be developed in its defense have not had their day in court. Would things have developed differently had there been a viable methodology of the generation of new theories available? It's hard to say.