A Philosopher's Philosopher of Science

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A towering figure in contemporary studies within philosophy of science, Princeton's Carl Hempel has shaped the interests of most scholars today, even those who oppose his views. Professor Hempel is known for his fair-minded objectivity, his precision in analysis, and his industrious scholarship. Twenty-five years ago he published a paper entitled "The Function of General Laws in History." Research in philosophy of history, our understanding of laws of nature, and our conceptions of *explanation* and *prediction* have not been the same since. Indeed, the best of recent works concerned with historical method and with the analysis of scientific explanation all derive from the sinewy position articulated in Hempel's early paper. Other articles, such as "Geometry and Empirical Science" and "On the Nature of Mathematical Truth," gave further evidence of his sure hand. But Hempel always, sooner or later, returns to his first love—the Logic of Explanation. In a masterful paper of that title (written jointly with Paul Oppenheim), Hempel drew the guidelines for future discussions. The very strength of some of his opponents' positions derives from the clarity and power with which Hempel's original analyses were written. An even more mature article, "The Theoretician's Dilemma," draws these strands together so as to rope off the attention of philosophers of science from less central issues. Hempel's arguments there hold his reader's eye on the complex undertaking known as "theory-construction." And now, at last, we have Hempel's **Aspects of Scientific Explanation: And Other Essays in the Philosophy of Science** (Free Press, New York, 1965. 515 pp., $12.50), which serves as the impressive capstone to everything excellent that has gone before.

What are these Hempelian contributions that have done so much for the discipline of philosophy of science? The life work of a serious philosopher cannot be summarized in a few words—but Hempel's analytical energies have centered on the idea of explanation; what is it to have "explained" an anomaly? What is it to have resolved an observational perplexity? Hempel's reaction to such queries consists in an account of what it is to locate the perplexing anomaly within an inference chain, one that is anchored in a law of nature. This is known as "the covering-law model" of explanation. And it is from this distinctive commitment that most of Hempel's further conclusions derive, and against which is directed the opposition of his critics. For it at once becomes clear that, in those disciplines which may lack respectable covering laws, "proper" explanation may not be possible at all. Of course, historians and social scientists were quick to riposte that "proper" explanations in their subjects were simply not Hempelian. Either that, or they agreed with Hempel and undertook to overhaul the objectives and methodologies of their own disciplines, the better to instantiate Hempel's dicta.

Given this covering-law conception of explanation, an interesting conceptual symmetry was soon disclosed vis-à-vis the related concept of prediction. For if x is explained by tracing it back to unproblematic conditions a, b, and c within the conceptual framework provided by laws L_1, L_2, L_3, . . . then it at once appears that, from nothing more than a prior knowledge of a, b, and c (now initial conditions), one could have *predicted* x to begin with, through inferential conduitry provided by L_1, L_2, L_3. On this account, then, explaining x is the same as predicting x after x has happened. And predicting x is explaining it before it has happened. With new gusto critics took aim at this Hempelian target. I joined ranks with many others who felt that there were respectable examples of explanation within the history of science, explanations which were wholly unrelated to any corresponding prediction. The same was urged for predictions which seemed to lack any corresponding explanation—statistical predictions, for example. In all these discussions Hempel's fairness and objectivity were as apparent as was his dialectical prowess. (At least one of his former critics now feels that earlier onslaughts were the result of misunderstanding and too-hasty argument. Others do not feel this conciliatory). Hence the issues raised by Hempel remain wonderfully stimulating across the entire length and breadth of analytical philosophy and philosophy of science. But his special contributions to our understanding of the nature and roles of hypotheses, of the interconnections between theoretical structures and the offering of explanations, between observational support and the generation of viable predictions—these contributions, manifesting as they do Hempel's precision and care, have placed his stamp on all future discussions of such issues.

Some critics will, and already have, chided Hempel for not having written a book quite different from the one he chose to write. *Aspects of Scientific Explanation* could probably not have been composed by a philosopher concerned predominantly with the process of discovery, or with the philosophical aspects of scientific problem solving. Nor is it likely that a scholar steeped in the lore of history of science would have given himself over to the intense analysis presented in Hempel's book. All the more reason then, for us to be grateful that Hempel has done what he did do. (Why assail Klee for not being Renoir?)

No philosopher worth his salt can do all things at once. Hempel could not have been more explicit about not wishing to engage in "the psychology of discovery." Nor is he exercised in this book with what might be styled "the conceptual analysis of scientific problem-solving"; this latter concern would center on the criteria and *de facto* inferential techniques involved in examples of scientific reasoning. (This second undertaking is not a species of empirical psychology, but wrestles with issues of methodology and "right thinking" at the frontiers of scientific inquiry.) There is no dearth of scholars who work with these latter types of subject matter. It strikes me as churlish, therefore, that some have upbraided Hempel for not also concerning himself with these matters. Hempel's stated business is with the logical anatomy of explanation and prediction.

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By revealing *ex post facto* the credentials of scientific arguments which have succeeded, he is able to delineate for us all what sorts of considerations might very well guide the problem solver through his labyrinthine scientific jungles. Hempel seeks to clarify for us the logical features of anomalies—the conceptual background of "the unexpected." With the sure virtuosity of a master logician he delineates how it is that observation-series can confirm a hypothesis, or disconfirm it. He undertakes to reveal how ensembles of data can corroborate theories, or can serve ultimately to render them useless in further inquiry. Sundry logical problems connected with the philosophy of probability punctuate his book on many pages. And without his illuminating contributions to our understanding of the function of *hypotheses* we should be poorer in analysis than we are at the moment.

Granted, these concerns are all centered on what might be called "the logic of the finished research report." And why not? Finished research reports are not themselves all of a piece. They can be distinguished from each other by virtue of the degree to which one is well made, while the others are not. One may rest its conclusions on observation and upon data logically aligned and inferentially bound to the terminus of its argument. Another may derive its strength from models and analogy. It is notorious how many research reports fail by being weak in both these respects. Any practicing scientist who would refuse the reflections of a serious logician at this stage of his inquiry would not be behaving reasonably. There are other interests for philosophers of science; "the logic of the scientist perplexed" might identify one such area. But this is not Hempel's forte, and in the groves of Academe it is still possible for a man to write books about what he pleases—and we are fortunate that this is the case when a philosopher like Carl Hempel chooses to write.

Thus, activities on the scientific battleground are of many different kinds. Some scholar could be interested in studying the attitudes of researchers before and while winning scientific encounters. Such a scholar would be concerned with the psychology of discovery—as were Wertheimer, Hadamard, and Polya. The thought processes of discoverers would be the focus here. Or perhaps it is the rational strategies invoked during such encounters that capture the imagination of philosophers. Contributors to this area of inquiry would be Toulmin, Peirce, and Whewell, among others. "Good reasons for entertaining as-yet-unestablished hypotheses" would be the watchword here. Still others will address the necessity of coming to understand what conceptual moves must be made in order for the scientific terrain, once achieved, to be related and integrated with the larger logical geography of the scientific enterprise. Such scholars will set out the logical justification for such new territories being adjudged properly acquired, with claims on them fully and justifiably established. "The logical underpinning of scientific knowledge" is the motto here. In this department of inquiry names like those of Carnap, Tarski, Reichenbach, and Hempel are the ones to be reckoned with. And this comports well with *dramatis personae* within the scientific epic itself. There are those whose discoveries came in a flood of inspiration—the Rutherfords, the Poincarés, the Kékulés, and the Keplers. There are also those whose unprecedented work at the frontiers was supported by brow-breaking initial arguments of considerable cogency—the Clerk Maxwells, the Newtons, and the Galileos. And, finally, there are those whose great and necessary contributions consisted in fortifying, strengthening, and holding the scientific ground already won by scholars in the vanguard. These are the Eulers, the Laplaces, and the Lagranges. The philosophical concern of Carl Hempel lies parallel to this camp. For the distinguished productions of his life, and of this present book, are tied to questions of justifying the arguments, eliciting the criteria, and exposing the inferential structure of those areas of knowledge that have become the glorious legacy of the scientific endeavor.

A Theory of Instruction

The battle for the souls of American schoolchildren continues. The weapons have been a little research, a lot of rewritten curricula in mathematics and science, and an assortment of books on education by people who were not, before the crusade, professionally interested in the education of children in elementary and secondary schools. Jerome S. Bruner of Harvard's Department of Psychology and Center for Cognitive Study has brought together a little group of essays—**Toward a Theory of Instruction** (Harvard University Press, Cambridge, Mass., 1966. 176 pp., $3.95)—that are gracefully written, lucid, and, uniquely among contemporary commentaries on education, never shrill. Bruner sketches out a theory of human development and a theory of instruction, he writes of education as a product of cultural evolution and of cultural evolution as a course of study for children, he rehearses strong opinions about the relation between English style and thinking and about the nature of "the will to learn," he presents some beguiling observations on children with learning blocks, and, despite the range that he covers, he manages a certain thematic unity.

Two basic ideas hold the essays together. Bruner believes that the instruction of children requires a continuing conversation on the relation between intellectual development and pedagogy, a conversation between the psychologist of development and the teacher. By the way, the psychologist is seen as valuable in educational reform not only because he can describe patterns of human growth and carry out research on motivation but also because of his "lively sense of what is possible." The second theme that unifies Bruner's wide excursions is his enthusiastic commitment to Rational Man. In the days of Vietnam and New York subways it is heroic to suggest that "much of the intrusive nonrationality about us . . . derives from our [nonsymbolic] operations upon experience" as though symbolization in natural language, number, or logic had ever been a guard against nonrationality. So too with the child's desire to learn. Curiosity seems "among the most reliable of the motives" and "the will to learn is an intrinsic motive, one that finds both its source and its reward in its own exercise." Bruner's optimism dips only once: in writing about children who have blocks to learning, he reminds us of "the imperiousness of our drives and the demands of powerful, nonrational, and indocile unconscious mechanisms."

Toward a Theory of Instruction is not a technical book, and Bruner disarms the critic by putting down his own essays—". . . too little data, too little systematic observation, too sparce an arsenal of analytic tools." Nonetheless, Bruner raises issues of great consequence and, coming at a time when