Book Reviews

Patterns of Discovery. An inquiry into the conceptual foundations of science. Norwood Russell Hanson. Cambridge University Press, New York, 1958. x + 241 pp. Illus. $5.50.

In my opinion, this is the most exciting book on the philosophy of science to appear in the last 10 years. It is exciting for various reasons, but the most important single reason is that at last we have a philosopher of science who is in fact writing about science and not about the papier-mâché constructions that frequently replace science in the writings of philosophers and logicians of science. Moreover, the realism (and thereby the novelty) of the approach strikes the reader from the early chapters of the book on. By the time one has reached the chapters on theories and on classical particle mechanics one has been introduced to what is almost literally a new way of *seeing* science—a way of seeing that enables one to remove the usual philosophical puzzles about "the reality of theoretical entities" and about "induction" from the all-too-central position that they normally occupy in the philosophy of science and to replace them with an undistorted view of a modern research science in full life.

Not only does Hanson know science but he has the requisite skills in logic and conceptual analysis for an undertaking of this scope. Thus, the need that his book fills is a complex one: the need for someone to write about science who has the technical equipment of a first-rate philosopher, the ability to see science as it is, and the good sense not to force it into one or another tidy schematism.

In order to explain Hanson's achievement it is necessary to describe briefly the conventional account of science. According to this account, observation reports in science are couched in one vocabulary ("the observation vocabulary") and theories are couched in another ("the theoretical vocabulary"). The "observation vocabulary" is thought of as stable and unchanging (in contrast to the highly changeable "theoretical vocabulary"). Observational reports are admitted to be corrigible, but the emphasis, by and large, is on the procedures by means of which theories are checked or tested against observation reports (which, it is assumed, any careful observer can verify with only a very small probability of error). Theories are entertained by scientists for various reasons, but the question of why a scientist entertains a theory (as opposed to how he tests it, once it is formulated) is dismissed as a question for "psychology" rather than logic. (Hanson points out in passing that this approach dismisses precisely the job that requires the genius—the Einstein, the Newton, the Kepler—from study by philosophers of science and focuses on the job that any well-trained graduate student can do.) The testing of theories is, in turn, treated as a basically simple matter; predictions (couched in the "observation vocabulary") are derived, and, if they turn out to be true, the theory is accepted. Insofar as considerations other than predictive success enter into the acceptance and rejection of theories, these considerations are usually lumped together under the name "simplicity" (some speak even more vaguely of the scientist's search for the "simplicity" of his "total conceptual system").

Now, this conventional philosophy of science has been running into increasing difficulty in late years. Hempel, for example, has pointed out a number of very serious difficulties in the attempt to make the notions of "testability" and "simplicity" precise, while Quine (following the lead of the 19th-century Duhem) has urged that the whole idea that scientific laws must be testable in isolation is a serious mistake. But these contributions are in technical articles that are unlikely to be encountered by the scientist or the scientifically trained layman interested in the conceptual foundations of science. Here, however, is a book-length treatment which, in addition, does more than criticize the "hypothetico-deductive" account at isolated points; it replaces it boldly and from the outset, not just with a different account but with a different (and more suggestive) set of questions.

Hanson begins by challenging the separation between "observation" and "inference" which contemporary philosophy of science has inherited from the positivism of the 1930's. Amplifying some remarks of Wittgenstein's on *seeing,* he stresses the extent to which even ordinary "garden variety" cases of "seeing" an event involve integration and organization, and he establishes the essential falsity of any account which separates this into two temporally or even logically distinct stages: first seeing the bare "sense datum" and then drawing "inferences." Scientists with different conceptual schemes, he argues, do not simply draw different inferences from a common stock of observation reports; they *observe* different things.

After this preliminary reexamination of the concept of *seeing* (and, by implication, of observation), Hanson moves to his central topic: the processes by which theories are arrived at, as opposed to those by means of which they are checked. Replying to the contention that this must be left for "psychology," he rejoins: "In the thinking which leads to general hypotheses, there are characteristics constant through the history of physics, from Democritus and Heraclitus to Dirac and Heisenberg" (page 72).

Hanson's discussion of the "patterns of discovery" is complex, and I shall only give hints of what it contains. Among other points, it stresses the role of unwieldy notations in hindering the discovery of a successful way of organizing data, and the somewhat surprising fact (illustrated by several novel examples from the history of science) that even after the "right" notation has been arrived at, there may still be trouble because the right notation "comes" initially with the wrong physical interpretation. (Kepler was already working with elliptical orbits as a mathematical device while he still subscribed to the theory that the orbit of Mars is an ovoid.) Hanson's account also includes a fresh discussion of the concept of causal explanation and of the role played in causal explanation by "theory-loaded" words. One point of which I heartily approve is this: he stresses the role of theories throughout, not just in prediction but in making nature intelligible. Once one has seen how theories are built into the concepts we use, and how a successful theory comes to be presupposed in a host of predictive, explanatory, descriptive, and computational contexts, one can also see why it is that a scientist will instantly reject one theory that "flashes into his mind" while deciding to pay serious attention to another—although both agree with the data from which he is working and neither has otherwise been tested.

A number of important points in the logic of science follow from this account. One of the most important is this:

since a theory may be built into the concepts we use in the description (in fact, in the very observation) of phenomena, to give up an important scientific law would be to do more than to give up some predictions we had become fond of making; it would be to "let our concepts crumble." Thus it can be that the abandonment of a scientific law may be a *conceptual* impossibility notwithstanding the fact that the law is empirical in the sense of aiding in the derivation of testable predictions. To put it differently, certain scientific laws (for details, see Hanson's chapter on classical particle mechanics) are *not* "empirical" in the sense that no experiment now conceivable (and this is not a "psychological" use of *conceivable*!) could overthrow them, although they are not "definitions," and they are not "a priori" either (since their abandonment *would* be conceivable if an Einstein or a Newton were suddenly to provide us with a whole new way of conceptualizing the phenomena in question). Since I feel strongly that overworking of the "empirical statement-or-else-a-definition" dichotomy is one of the worst faults of conventional philosophy of science, I was extremely happy to see Hanson take this up so thoroughly and so convincingly. Indeed, Hanson shows in detail how the same law may function in one context as a testable generalization, in another as a definition, in another as a conceptually a priori statement, and in yet another as a computing device. (I would only add: one should stress the point that the law does not have different *meanings* because it is employed in so many ways; sentences in a natural language—and not just laws—can quite frequently be used in so many different ways *because* they have a single meaning.)

Among other problems touched on in these chapters are the familiar worries about the "reality of theoretical entities" (what better reason could there be for accepting a system of concepts than that it makes the world intelligible?) and the difficulties that some have felt about the use of exact numbers in theoretical science. The book culminates in a chapter on elementary particle mechanics which shows the power and fertility of Hanson's ideas through their ability to render some of the dark mysteries of quantum mechanics understandable, not in the sense of providing final clarification (that is the goal of the physicist rather than of the philosopher of science) but understandable in the context of the past history of scientific theory-construction, and in the context of a growing research science.

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Plain Talk from a Campus. John A. Perkins. University of Delaware Press, Newark, 1959 (order from University Publishers, New York). x + 195 pp. $4.

Since their average tenure is less than 5 years, many state university presidents are not in office long enough to reflect very much upon their experiences, much less reduce them to book form. John A. Perkins, president of the University of Delaware since 1950, is one of the exceptions. He speaks not only as an experienced educational administrator but also as one who has achieved recognition in the field of public administration. His *Plain Talk from a Campus* is a sharp analysis and a searching commentary on some of the critical problems in contemporary American education.

Part 1 deals with the purposes of education, both higher and secondary. According to the author, colleges and universities confront four main sources of problems: overwhelming increases in enrollment; the extremely divergent preparation of high-school graduates; the tendency of most institutions to "emphasize tradition far more than change"; and the peripheral functions which barnacle the pilings of American education. In view of the fact that higher education enrollments quintupled during the first quarter of the present century and doubled in each subsequent 15-year period, one may wonder how "overwhelming" our problem of sheer numbers is, but there can be no question about the fact that Perkins has come to grips with some of the major educational issues of our time.

In Part 2, his analysis of the problems of financing higher education, particularly on the state level, is very incisive. What he has to say about the shortcomings found almost everywhere in the patterns of state expenditure and taxation makes very understandable the fiscal fumblings of many state legislatures, and one must agree with him that more federal support is inevitable if these mounting difficulties are not overcome. In his opinion, moreover, bringing the Federal Government more largely into the picture implies no new peril.

Perkins' special interest in public administrations is reflected in the third part of the volume. He stresses the role that the colleges and universities ought to play in training students for public service careers, and he urges a wider realization of what Walter Lippmann has called "the public philosophy." A telling contrast of a wryly amusing sort is drawn between American political leadership of the past and present, in a chapter on "Benjamin Franklin and the organization man."

The final section of *Plain Talk from a Campus* is a potpourri, having to do with such miscellaneous topics as the ingredients of effective university administration, research and publishing, the neglected importance of books as media for learning, what a president does and does not include in his annual report, and the need among students for more self-discipline.

All in all, John A. Perkins has given us some plain talk which needs to be heard and heeded within and around all of our campuses.

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Trend and Tradition in the Prehistory of the Eastern United States. Illinois State Museum Scientific Papers, vol. 10. American Anthropological Association Memoir No. 88. Joseph R. Caldwell. Illinois State Museum, Springfield, 1958. xiv + 88 pp. Illus.

This synthesis of the archeology of the eastern United States, originally written as a doctoral dissertation at the University of Chicago, should prove most valuable as a general introduction to the subject. It has the advantage over previous syntheses, such as *Archeology of Eastern United States,* edited by James B. Griffin (University of Chicago Press, 1952), of being a true synthesis and not just a compendium of local sequences. On the other hand, it avoids the disadvantage of *Method and Theory in American Archaeology,* by Gordon R. Willey and Philip Phillips (University of Chicago Press, 1958), in that the synthesis is expressed in narrative fashion and is not compressed into a rigid scheme of developmental stages based primarily upon what happened in nuclear America. The present volume is truer to events in the eastern United States.

The acknowledged weakness of this synthesis is that, for lack of time to cover the literature thoroughly, the author concentrated on the southeastern United States, where he has done most of his own research. On the other hand, the volume does present fresh material on Southeastern archeology, and, if any area is to be emphasized, this is the best, since the most important developments took place here, at least during the later periods. The volume also suffers from a certain vagueness of conceptualization—for example, *trend* and *tradition* are not precisely defined, and neither are most of the actual trends and traditions covered in the monograph.

The author sees three major trends in the prehistory of the eastern United