Structure of Science

Philosophy of science, a separate discipline, meets philosophy proper on the question, "What exists?"

May Brodbeck

Ernest Nagel's treatise on the philosophy of science has, for some time now, been anticipated eagerly by all who are familiar with the high quality of his many essays in this area. The anticipation is thoroughly justified by this volume. The several fundamental issues in the logic of scientific explanation are here discussed extensively and in depth. The nature of scientific laws, of causality, of theories, and of explanation are considered not only in general, but also as these problems arise within specific contexts in physics, both classical and modern, in the biological and the social sciences, and in the study of history. The connections among these areas, that is, the various aspects of the problem of reduction, are treated in detail, as they should be, in a volume devoted to scientific explanation. Nagel is as scholarly and accurate about the relevant scientific and historical details as his readers have good reason to expect him to be, and that is scholarly and accurate indeed. As one would also expect, Nagel attends not only to the more technical issues in the philosophy of science, for instance, the nature of geometry and its relation to physics, but also to matters of more general interest. He discusses the various claims that have been made by laymen and by some scientists-turned-Sunday-philosophers about, say, the relation between the quantum theory and the thesis of scientific determinism, about the possibility of a science of man and society, and the implications these issues have for human freedom. In all such matters he is, to my mind, on the side of the angels. Patiently and carefully he exposes the fallacies and confusions of those who willfully or ignorantly misinterpret and twist the findings of science in the interests of obscurantist special pleading.

If, gratifying as all this is, for many of us the book contains few surprises, that is hardly a relevant criticism. It is an excellent thing to have it all here, systematically worked out, in one volume. (A second is also promised.) Nor are we, after all, the audience that matters. Some lessons, some insights are peculiarly slippery and, it seems, must always be learned anew by each generation. Many of those lessons and insights will be found in this volume. Not the least is the general nature of the work. His book really is a study in the logic of science, not merely speculative commentary on how scientists contrive to make discoveries or to communicate with one another. This is wholly admirable. If I must dissent from Nagel on certain fundamental issues, this does not in the least affect my judgment that the work as a whole is a most valuable and considerable achievement.

"Real" and "Exist"

The philosophy of science, unlike philosophy proper, takes our common-sense world of tables and chairs, the sun, the stars, and other people for granted. In other words, it assumes the common-sense realism of our everyday belief in the independent existence of an external world. Insofar as the philosophy of science concerns itself with questions about "reality," it is not with the status of ordinary perceptible material objects and their properties, but with those special entities which the scientist talks about but which we do not perceive. As Nagel points out, we do not know that there are electrons in the same way or in the same sense that we know we have hands and feet. Since we don't, there is a problem. Are these entities real, like hands and feet, or are they speculative, as the existence of men on Mars is speculative, or are they merely fictions, not perhaps as unicorns are fictions but as being merely verbal or symbolic instruments useful to the scientist for explaining and predicting phenomena? The problems and the range of answers are familiar. The dialectic is subtle and complicated. Before a stand can be taken, the various connotations of "real" and "exist" must be teased out. To exist is to be observable: for the materialist, observable by more than one; for the dualist, a felt pain is as real as a chair. Again, to exist is to occur in one or more scientific laws. Or, to exist is to be a simple element of which everything else in some sense consists. These are some of the connotations that are relevant to clarifying and resolving the issue. Once such distinctions have been made, there may be little more that needs to be done in order to show how one may agree that there are indeed electrons, that they are real, yet agree too that there is an "ontological" difference between electrons and chairs or colors. This done with the care that it requires, one may well tend to echo Nagel's statement that the difference between the instrumentalist and realist views is simply a "preferred mode of speech." Yet given the grounds on which he makes this assertion and the scope he allows it, radical dissent is, I believe, required of anyone who wants to maintain our commonsense realism.

Inevitably, at some point philosophy proper which examines our commonsense assumptions will, implicitly or explicitly, be injected into the discussion. However, an adequate philosophical analysis must preserve our ordinary belief in the independent reality of tables and chairs and, once these are firmly anchored, *then* discuss the connection of electrons and neutrinos to this reality. An analysis that fails in the basic philosophical task has not eluded the verbal snares leading to the Pickwickian denial of an external world, whether that denial takes the form of straightforward idealism or of any of its instrumentalist variants. Unfortunately, just at this crucial juncture where the philosophy of science and philosophy proper meet, Nagel's analysis is weak and vacillating. We see our hands and

The author, professor of philosophy at the University of Minnesota, Minneapolis, reviews here *The Structure of Science* by Ernest Nagel (Harcourt, Brace and World, New York, 1961. 618 pp. $7.50).

feet, but not molecules or psi-functions. Accordingly, Nagel distinguishes theoretical laws or "theories," groups of statements about these unobservable entities, from "experimental" laws which, not necessarily based on laboratory experiment, state connections among observable things and their properties. The distinction is familiar and important. Nagel agrees that it is important, but holds also that it is "vague." Here begins, for me, the elusiveness of Nagel's discussion. *On the one hand*, we are told that with each experimental concept a definite overt procedure is associated, endowing its laws with "determinate empirical content," while this is not true of theoretical terms or of the statements in which they occur. Repeatedly, reference is made to the "facts of observation." We also have extended comment, with good illustrations, on the significant circumstance that our explanatory theories may change while the experimental laws remain the same, nor are the latter "contingent upon the fate" of the former. These comments support the thesis that hands and feet are "ontologically" different from electrons and molecules. *On the other hand*, Nagel also insists that the distinction between what is and what is not observable, though important, is only one of degree. So too, therefore, is the distinction between changing "theoretical" and stable "experimental" laws. This amounts to saying that there really is only a difference in degree between hands and feet and electrons. It follows, for Nagel, that the distinction between the instrumentalist and the realist views with respect to hands and feet as well as with respect to electrons is only verbal. The point at which we have merely a "preferred mode of speech" is thus pegged at the level, not of the problematic entities of theory, but down to ordinary material objects and their properties, like boiling point and temperature.

Dewey's Influence

Though he conscientiously catalogs the weaknesses of instrumentalism and the strengths of realism, Nagel ultimately is unable to free himself of instrumentalism. (John Dewey was an early, strong, and, as must now be seen, ineradicable influence on his thought.) The explicit snare in his case, trapping him, one can't help feeling, into instrumentalism, is the notion of "implicit definition." Doing no harm in mathematical, purely formal contexts, it leads to catastrophe when applied to descriptive systems by generating the illusion that uninterpreted marks on paper, symbols that have not been tied to observable referents, nevertheless have descriptive meaning. Nagel is aware of this illusion and its dangers when discussing the difference between pure and applied geometry and the problem of the relation of geometry to physics. Indeed, without this awareness, that issue cannot even be approached intelligibly, let alone discussed as excellently as Nagel does. Yet he carries over the notion of "implicit definition" from contexts where it is relatively innocuous to contexts where it is far from innocuous. Not only the theoretical concepts, but the experimental descriptive concepts are all said to be "implicitly defined" by the statements in which they occur. As is well known and lucidly discussed in detail by Nagel, the terms of a theory are only "partially coordinated" to experimental concepts. This means that not the individual terms, like mass- or velocity-of-a-molecule, but only certain arithmetical functions of combinations of them, like momentum, are logically tied to experimental concepts like pressure. If the term were not compromised beyond repair, it would perhaps do no harm to say that the uncoordinated individual terms of a theory were given "meaning" or "implicitly defined" by its axioms. When, however, this notion is carried over to the experimental concepts, which are the source and basis of such descriptive meaning as the theory has, instrumentalism is unavoidable.

Nagel justifies extending the notion of "partial" meaning to the experimental concepts by leaning in part on the argument from the possibility of alternative definitions when a term occurs in several well-confirmed laws. This circumstance, so the argument goes, means that each law partially defines the terms occurring in it. But if at any given time, one of the observable phenomena associated with the concept were not chosen as the defining property, then we could never make any empirical statements about that concept. Apparently aware of this danger of either unintelligibility or vacuity, Nagel concedes that when several procedures are available one of them is chosen as the defining property. His chief argument therefore apparently rests on the use of the real-number system and of such idealized notions as point-masses, while we measure only discontinuous magnitudes and actual bodies. For Nagel, this apparatus puts macroscopic Newtonian theory on a par logically with quantum mechanics, the experimental concepts in the same boat with theoretical ones, that is, velocity of a car in the same boat with velocity-of-a-molecule. But what is partially coordinated in such cases is part of the *logical* apparatus, a real number to a class of rationals; a point with zero dimension to the moon. The issue, however, concerns not the logical but the *descriptive* terms, like mass-of-a-molecule and temperature. It can therefore only be clarified by first assuming once and for all the partial interpretation of the logical, arithmetical concepts, which are shared by theoretical and experimental statements alike, and then by inquiring into the difference between electrons and billiard balls, for there is *still* a difference and that is still a problem.

At the root of Nagel's inability to embrace an unequivocal realism with respect to hands and feet is his intense dislike, inherited from Dewey, of any "given" element in experience, any self-contained noninferential knowledge that may serve as the basis for all knowledge. Though tables, like colors, are observable, unlike colors they are not at any one instant wholly observed. Thus about tables and thermometers it is possible to raise a question, though not, intelligibly, about colors and sounds. Fearful of that *bête-noire* of all instrumentalists and idealists, sense-data, he rehearses, albeit in an off-hand and half-hearted way, the conventional arguments against the translatability thesis. He concludes, conventionally enough, that because the "translation" proposed is not practically feasible, the thesis has no justificatory force. So the question of the physical reality of raindrops and temperature, like that of molecules and kinetic energy, becomes merely a matter of a "preferred mode of speech," for neither temperature nor, *ipso facto*, kinetic energy can have meaning apart from the system in which they are inextricably bound.

Thus, despite the many illuminating things Nagel says about the structure of theories, his discussion of the status of the theoretical entities founders and fails at the juncture where philosophy proper meets the philosophy of science. With the particles left unanchored to an unequivocally external reality, the whole edifice crumbles. It crumbles because

there remains no truth or falsity which does not depend upon either a shifting usefulness or a viciously regressive coherence. Without this self-contained truth, the notions of testing a theory and of what is and what is not evidence become unintelligible. Fortunately, many issues in the philosophy of science are remote from this delicate philosophical underpinning. Despite, therefore, what seem to me the weaknesses of this volume on such fundamentals, it can remain an admirable and distinguished contribution.

Russian Anthropology

The Ancient Culture of the Bering Sea and the Eskimo Problem. S. I. Rudenko. Translated by Paul Tolstoy. Henry N. Michael, Ed. (Anthropology of the North: Translations from Russian Sources, No. 1.) Arctic Institute of North America and University of Toronto Press, Toronto, Canada, 1961. iii + 186 pp. Illus. $3.

In a field as inadequately served as anthropology has been in this respect, it is indeed a pleasure to hail the inauguration of a Russian translation series as admirably conceived and competently executed as this new project of the Arctic Institute of North America. The project, which is supported by the National Science Foundation, may well serve as a model for comparable projects in other disciplines. Congratulations are in order for the organizer, Henry B. Collins, and for the editor, Henry N. Michael, who combines linguistic proficiency and knowledge of the subject matter with editorial experience. Paul Tolstoy's translation of this first volume sets a high standard.

The work selected for the initial publication is a basic contribution to Eskimo archeology by one of the outstanding senior Soviet archeologists. It records the results of field research (in 1945) on the Siberian side of the Bering Strait. Rudenko's investigations were in the nature of a reconnaissance: locating and testing sites, gathering collections, and carrying out some limited excavation at the most promising points. This excellent report describes the work, the sites, and the sizable collection of specimens; it is copiously illustrated.

The most important result was the discovery at Uelen, near East Cape, of a major site of the earliest (Okvik) stage of the classic Bering Sea culture sequence. Remains of all succeeding stages were identified at one point or another on the coast of the Chukchi Peninsula, but no earlier traces were found—a matter of some interest to proponents of an Asiatic origin for Bering Sea Eskimo culture. The report concludes with a discussion of Eskimo origins, based on the author's interpretation of the evidence provided by the harpoon and skin boat complex, art styles, and composite implements. Rudenko justly demolishes the efforts of Western scholars to derive the Eskimo from interior Siberia but on the other hand demonstrates the presence of southern parallels to Eskimo culture. Somewhat carried away by his preoccupation with the latter, he sees the Eskimo as a group of migrants from insular southeast Asia, who arrived at a relatively late date and who intruded as an alien wedge into the Bering Sea region. However, this hypothesis lacks any foundation when other types of evidence are examined and has attracted no following, although it has served a useful purpose in drawing attention to the neglected problem of cultural relationships between the Eskimo area and the Pacific coast of Asia. I hasten to add that these speculations in no way detract from the solid value of Rudenko's report.

Additional volumes of this series, promised for the near future, will be eagerly awaited.

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Regulatory Profession

Science and Public Administration. James L. McCamy. University of Alabama Press, University, 1961. viii + 218 pp. $3.50.

Science and government are old partners who are now getting along very well together, due to a new profession, public administration, which was developed to serve as the channel of communication and to insure that scientists do not engage in government or government delve in science. Perhaps this is not exactly McCamy's thesis, but the scientist who reads his presentation may gain the above impression. McCamy presents the case for the specialized profession of the public administrator, the official who, by reason of special abilities and training, is capable of making the administrative decisions, once the scientists present him with the facts. To a degree the scientist who reads this book may be prejudiced by the subordinate role to which he is assigned and by the implication that the scientist is the technician to the administrator who takes the scientists' developments and decides how to use these in the world today.

Too much emphasis is given to establishing a gap between science and other cultural areas by developing the myth of science and the conflict with religion. These conflicting arguments are used to develop a need for a group other than scientists, rather than a group including scientists, to administer the problems science creates.

If McCamy is disturbed that the National Science Foundation gives, in his opinion, too little support to the social sciences, he may well be interested to know that some scientists feel otherwise. To the author of this book, science includes the social and behavioral sciences, and in fact there is a hint at times that he considers public administration a science involving knowledge and techniques as complicated as other recognized sciences.

While consideration is given to the organization and structure of science, there is a lack of a parallel discussion of the development of the public administrator, how one learns to make decisions on scientific facts, and on what basis and how one integrates scientific, political, and social facts. If it is true that "science creates the social problem for which the public administrator must recommend solutions," then the inquiring scientist-reader would like to know how these solutions are obtained by public administrators, so that he can weave these into the social monsters he is accused of creating.

There are some who write in a challenging manner to hold the reader's interest. Scientists may not agree with the role which McCamy has given them, but they will find his discussion of continued interest; interspersed between ideas with which they will take issue are many thoughts which will merit both further study and support. The book is not a handbook on how to administer a public science organization, but rather something for both scientists and administrators to "chew on."

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