Book Reviews

Philosophy of Science in Russia

Problems of the Logic of Scientific Knowledge. P. V. TAVANEC, Ed. Translated from the Russian edition (Moscow, 1964) by T. J. Blakeley. Reidel, Dordrecht, Holland, and Humanities Press, New York, 1970. xii, 432 pp. $11.75. Synthese Library.

One thing emerges clearly from a reading of this collection of essays by Soviet philosophers of science and logicians: communication between the English-speaking and Russian-speaking communities of scholarship in this field is primitive, and almost incredibly so. The fault is heavily on the side of the Anglo-American philosophers of science, since their Soviet counterparts are apparently reading English and American works with relish, though with a time lag of at least a decade. If a similar situation existed in, say, mathematics or physics, it would be an international scandal. But the reasons are clear. Philosophy of science has been, largely, an Anglo-American preserve. The great historical traditions of French and German philosophy and history of science, and of contemporary logic—such figures as Mach, Duhem, Meyerson, Poincaré, Hilbert—were taken up into the contemporary Anglo-American tradition, to one or another extent. A whole generation of refugees from fascism informed and dominated the Anglo-American school for more than a generation—Carnap, Hempel, Feigl, Gödel, Tarski, Reichenbach, Popper, and others. There seemed to be no reason for English-speaking philosophers of science, surfeited with this *embarras de richesses*, and put off by the polemical Marxism-Leninism of Soviet philosophy of science of the '30's, '40's, and '50's (for example, in the critique of Einstein and of Heisenberg for "philosophical idealism" in relativity theory and in quantum mechanics, respectively), to invest the intellectual energy and the linguistic study required for a lively knowledge of contemporary Soviet work in these fields. Moreover, the cold war severely limited any serious scholarly contact, and, despite the fact that occasional visitors appeared on either side of the Curtain, the stance was formal and not that of practical intellectual working relationships.

The present collection makes one other fact clear: Within Soviet philosophy of science, or logic of scientific method, there is a large and apparently lively concern with those issues that are current among Anglo-American schools —logical analysis of the methodology of the sciences, of the role and character of deductive, inductive, and analogical inference, and of the role of statistical methods and of theory of probability in scientific thought—as well as a classical concern with epistemological questions concerning scientific knowledge. The hallmark of the present volume is its insistence on the use of contemporary methods of logical analysis, after the fashion of the logical empiricist movement in Anglo-American philosophy of science. Because there have developed alternative philosophical and methodological perspectives in the Anglo-American schools, and because, in a sense, logical empiricists have undercut many of their own earlier premises by their savage and sustained self-criticism, the present Soviet work seems dated in many of its concerns. Some of it is introductory in level of sophistication, though the character of almost all the essays is technical and workmanlike. The relation of dialectical materialism to the logical and epistemological issues at hand is at best a muted topic, dealt with only in some of the essays. In this sense, the volume is both refreshing and disappointing: refreshing, in that it is free of the "citational Marxism" which simply rehashes classical formulations or articles of belief *ad nauseam*; disappointing, in that there is not enough critical or interpretative discussion of the professed relation of dialectical materialist theory and analysis to the issues at hand. Perhaps this is asking too much. Enough that there is a grappling with a common core of problems, in their own, relatively autonomous terms. But this latter question remains on the agenda if one is to take seriously the professions of the relevance of dialectical materialism, and if one is to be intellectually and critically responsible to the Marxist tradition, as one of the major traditions in the history and philosophy of science.

Now to the substance of the work: There is an introductory essay, by Tavanec and Švyrev ("The logic of scientific method"), which does raise the question of the relation of a dialectical materialist view to the views inherited from the logical positivists of the *Wienerkreis*, and dominant in the Anglo-American school—namely, what is called here "neo-positivism." The authors survey the main issues in the development of this neo-positivism, in a rather quaint way: it amounts to a rationale for taking the logical-analysis route seriously, despite what are seen as the philosophic and epistemological errors of some of its protagonists among the positivists. The issue concerns the *object* of logic (and, thereby, the actual subject matter of logical analysis). Agreeing with Reichenbach (and thus with the whole antipsychologistic interpretation of logic) that logic "does not study thought as a natural psychological process," the authors go on to criticize the logistic view for having taken "as a point of departure . . . the simply false assertion that the only possible object that logic could have is thought taken as a psychological process" (p. 20). Rather, they say, logic has as its "object" those *norms* of thought which are objectively conditioned, socially necessary, historically evolved, and available to empirical analysis because they are physically embodied in language. Thus, according to the authors, logical reconstruction has as its object scientific knowledge itself, specifically in its expression in the language of scientific theory and practice. What is at issue is the characterization of this object. Tavanec and Švyrev opt for a historical-materialist view of this object as the concrete historical activity and behavior of man, through which "language is bound with reality." But they accept the relative autonomy of research into this question as a matter of the "pragmatics" of language, and hold that this is outside the purview of logic proper. Logic as such can be studied in abstraction from this historical-practical context, therefore, by way of a *syntactic* characterization of logic (in pure formal logic "as a development of a framework of concepts on the logical structure of thought, i.e. the maximum development of the conceptual apparatus of formal logic" [p. 12]); and by way of a *semantic analysis* (". . . of a configuration of signs . . . given a definite logical interpretation . . . [requiring] definite rules giving the truth conditions for propositions of the system" [p. 15]).

Now this is fairly classical semiotic theory, noteworthy not because of its novelty but rather because of its adaptation to a materialist philosophy of logic. The marking off of "pure" and "applied" logic as distinct research areas in the logic of science is qualified thus: though pure logic analyzes and develops a conceptual apparatus, it is, "in the final analysis, developed in order to be applied to an empirical subject matter and to solve related practical tasks . . . only the possibility of this application of pure formal logic and of its logical calculi makes it a logic and not a simple concatenation of formalisms" (p. 13). My question would be: How "mission-oriented" need pure logic be? Is this proposal simply one for a philosophical context for understanding what logic is, or is it a constraint on purely formal researches? The authors obviously intend it as the former (in what they say elsewhere); but that it can easily be turned into an argument against "formalism" or "pure research" is clear.

Past this introductory essay, the book gets seriously down to epistemological and logical questions. The current issue of the relation of theoretical to empirical or observational statements is approached by V. A. Smirnov (in "Levels of knowledge and stages in the process of knowledge") and by V. S. Švyrev (in "Problems of the logical-methodological analysis of relations between the theoretical and the empirical . . ."). Both get into all the well-known difficulties of denying "pure experience" as the source of "direct observation" (relativized to "methods of schematization") as the only source of "meaningful observation terms in an empirical language" (p. 32). Again, there is room for fruitful and critical discussion with Anglo-American colleagues embroiled in this issue. Similarly, Švyrev's discussion of the history of the logic of discovery, its confusion with inductive procedures, and its relation to hypothetical-deductive method touches on current debate in this country. Unfortunately, the references to the American discussion go only up to 1958 (including Quine's "Two Dogmas of Empiricism" [1951], Carnap's "Methodological Character of Theoretical Concepts" [1956], and Hempel's "Theoretician's Dilemma" [1958], which mark an early stage of present discussion).

Among the more technical logical articles, the logician Zinov'ev, already known here for his work on many-valued logics (*Philosophical Problems of Many-Valued Logic*, rev. ed., Reidel, Dordrecht, 1963), contributes a long essay on "Logical and philosophical implication," Sadovsky writes on "The deductive method as a problem," Ruzavin contributes an essay on "Probability logic and its role in scientific research," Gorski writes on "Definitions and their importance for science," Rakitov writes on "Statistical interpretation of fact and the role of statistical methods in the structure of empirical knowledge," and Uemov attempts a formalization of analogical inference, in a discussion of the modes of analogy and their relation to induction. All the essays are characterized by an appeal to the formal-logical apparatus in the service of an analysis of applied questions of scientific inference and discovery. In this sense, the essays take on the look of much of the Anglo-American publication in this area. What remains at issue, both for the Soviets and for us, is whether the logical formalisms do much more than to abbreviate the formulations and analyses which can (and do) take place in ordinary nonnotational form. A schema such as the one Uemov introduces,

$$(a,b)\ Cx \vdash \frac{(a)P}{(b)P}$$

(namely, that a and b are similar [in some property P] and both are causally related to x, and that it *follows* from the common causal relation that a and b are similar in P), is a useful formalism *only* if (i) it gives a clearer account of the structure or form of the abstract relation than can be given in its verbal analogue or if (ii) it functions notationally in a system of deductive inference for which the rules are stated. Otherwise, it becomes redundant, or redolent of bristling formalisms whereby "technical" respectability is assured. True, Uemov attempts such an inference-sketch, basing it on the *translation* of analogy-relations to those of some formal schema in which inference can be carried out. I do not doubt that such logical schemata can be formulated (or that some existing formalisms may be interpreted for this case). The question is, what power (either of inferential procedure or of understanding) does this yield, as an *applied* logic of science? (This is, after all, the condition put upon such "pure logics" in the introductory essay.) The question may be asked even more concretely thus: *Given* the "power" yielded by the formalism, as a "pure" logic, what is the relative measure of utility which this power affords, given the end in view (an applied logic of scientific methodology), to that afforded by nonnotational analysis? (One parameter here is the relative "noise" which needs to be overcome, in explicating the notational form.)

One parting shot, on the complex array of methodological questions raised in the essays. Rakitov takes a baldly empiricist view in his definition of "facts" in science: They are those "empirical propositions" which are "statistical résumés of direct experimental data" (p. 406). The statistical probability of such a proposition, according to Rakitov, involves the law of large numbers, such that the probability of a proposition $P_s = m/r$ (where r is a sufficiently large number of independent experimental observations and m the number of favorable instances) should approach the ideal limit of its logical probability P_L as arbitrarily closely as possible ($| P_s - P_L | < \mathcal{E}$, where $\mathcal{E}$ is any arbitrarily small number). In effect, then, nothing counts as a scientific fact without an exhaustive statistical summary of a large number of discrete and independent observations. Where then is the role of the single instance, in science (or in ordinary life, where commonplace "facts" are alleged)? Rakitov writes this off to complex statistical processing by the nervous system (in the commonsense case) and to analogous processing in experimental contexts. In effect, replication becomes a condition for factuality, and actual repetition of experiment, in large numbers, the condition for factual claims. Only computer simulation of experimental situations and computer processing of the information yielded give the analogue to nervous-system processing in the commonsense case. Thus single-observation judgments are themselves viewed by Rakitov as statistical summaries of "individuals" taken as statistical aggregates (for example, in the observation "This rose is red," or in a single measurement, say, of a man's height) and thus, such judgments always yield an indeterminacy. This is an extremely roundabout (albeit interesting) way of asserting the fallibility of empirical knowledge-claims. But it assigns fallibility (or the possibility of error) to the single observation on the grounds of the statistical indeterminacy of such an observation. The assumption is that repeated observation of independent instances yields greater confirmation approaching the limit of scientific "fact." The problem is that the fact "All swans are white" is no fact, and never was, albeit P_s approached P_L arbitrarily closely for a very long time. This essentially confirmationist (and hence subjectivist) theory of "fact" doesn't sit well with an objectivist theory of scientific knowledge, though it may be offered as a theory of rational belief.

The last comment concerns the unfortunate transliteration style, especially in the bibliographies following each article. The translator, after asserting in his prefatory note that "blatant errors [in the Russian bibliography] have been corrected," goes on to note such trivia as "H. Reichenbach for G. Reichenbach" (there is no "H" in Russian, and "G" is its standard substitute!), and *then* to list such entries as "Gusserl" (for Husserl), "Uorf" (for Whorf), "Gil'bert" (for Hilbert), but worst of all "Van Xao and Mak-Noton" (for Hao-Wang and McNaughton), Čerč (for Alonzo Church), and "N'juton" (for Newton). Some familiarity with the authors cited should have yielded normal spellings, instead of these barbarisms of transliteration. The bibliography, on the positive side, suggests a large number of technical-analytical works in Russian (many by the authors represented here) which ought to become known to American logicians and philosophers of science so that the discussion on these matters between Russian-speaking and English-speaking colleagues can be pursued intelligently and critically.

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Prerevolutionary Scientists

Science in the British Colonies of America. RAYMOND PHINEAS STEARNS. University of Illinois Press, Urbana, 1970. xx, 762 pp. $20.

When Raymond P. Stearns first turned from the study of European history to devote his scholarly efforts to early American science, he moved into a field that was very thinly populated. Aficionados can recall the names of Theodore Hornberger, Frederick Brasch, and a very few others. To my knowledge, the history of American science was not being taught at any college or university in the country. The surface of the subject had barely been scratched and most historians still did not realize that there was any significant scientific activity in America during the colonial period. Although a great many historians apparently still live in the same ignorant bliss, Stearns has played a major role in persuading the majority that scientific pursuits were important concerns for at least a part of the population. His pioneering studies of American fellows of the Royal Society, of that institution's role in promoting science in the colonies, and of various individuals involved in promotional effort on both sides of the Atlantic have been influential in restructuring our thought about that period.

The present volume, which I received only a few days after news of the death of the author, is a superb example of the type of work for which Stearns was known: careful, detailed, the result of meticulous scholarship. Its aim, in the author's words, is to provide "within a single cover, a comprehensive overview of the scientific interests and activities of American colonials . . . in the expectation that such a treatment would supply a basis for historical perspective, for comparison and contrast, and for the creation of a sense of growth and development of science in the colonial era."

The book delivers what the author promises. It is comprehensive—or at least as close to it as there is any need to be. Within the 686 pages of text, Stearns discusses virtually every scientist of any consequence who lived or worked in colonial America. He gives a clear account of their work and accurately assesses it, in most cases remaining true to his expressed belief that "the integrity of science at any moment of its history must be that of its own time," that one must not judge earlier work in terms of its "rightness" or "wrongness" according to modern science but must be aware that many different views of nature have been "scientific" in their own day. The book is truly a mine of information that can safely be neglected by no one working in the field, and it will be useful for years to come.

But once this is said, one must also point out that not all the work on colonial American science has yet been done. As much as one must admire the comprehensive nature of the book, one should also be aware that—especially because it is a good book of its kind—it reveals all the limitations of the essentially descriptive, encyclopedic approach to history that Stearns took. It is a record of missed opportunities to make significant statements about the nature of colonial science. It suffers mostly because it has no analytical framework; instead, the framework is simply geographical and chronological. By this I mean that the author moves from an account of science in New England, through science in the West Indies and in the Southern mainland colonies, back to the West Indies at a later period, then once again to the Northern mainland colonies. Each is considered separately, almost in isolation from the others; the only thread that ties them together, at least in the early period, is the tenuous one of the activity of the Royal Society in promoting science in each area. Even within the areas, each scientist is considered under a separate heading. This organization means, of course, that there is some repetition that could have been avoided by a different framework. But most important, it means that promising lines of research simply cannot be followed. For example, in one place Stearns mentions that a circle of colonial scientists was beginning to develop, to carry on correspondence and exchange among themselves; in another he suggests that Paul Dudley, whom he correctly assesses as one of the most skillful of the colonial scientists, had discussed many scientific problems with a wide range of New England scientists. These are certainly important details, and Stearns was aware of them, but the organization he adopted made it impossible for him to give them more than a passing mention. It comes almost as an admission of failure, therefore, when he notes on the next-to-last page of text that "the triumph of colo-