a careful numbering of definitions, hypotheses, and theories etc. throughout and that these develop in a beautifully orderly fashion.)

The limitations of the development here are at least twofold (quite independently of the question of achieving greater scope through pursuing the alternative, axiomatic approach): (*i*) the treatment of the structure of science is traditional, with the theoretical/nontheoretical (θ-dependent/θ-independent) dichotomy appearing and each theory treated in isolation from its neighbours; we are a long way from interesting semantics and epistemology for theories here and (*ii*) the treatment of physical theory is at a simple level, e.g. classical particle mechanics is treated as Newton would have done in terms of masses, forces etc. (or, later in Part II, in terms of generalized positions, forces etc.). Many of the deeper philosophical and mathematical questions center around e.g. its treatment as a Hamiltonian flow on a symplectic manifold (e.g. volume cited above). But neither of these limitations seem to be intrinsic to the type of development; they simply demand a vastly more sophisticated development than Sneed (understandably) has offered. What is less clear is whether this kind of development within Sneed's framework is the best possible way to tackle these issues. My intuition says it is.

Part II deals with the concepts of the identity, equivalence and reduction of MTP's (chapter VI) and with the origins and evolution of an MTP (chapter VII). Roughly speaking, an MTP is, according to Sneed, a core mathematical structure (cf. above) together with a set of intended applications. Thus theories are identical (roughly speaking) just when their cores and sets of intended applications are identical; they are equivalent (very roughly speaking) when the core of one can be expanded to cover all of the intended applications of the other and vice versa (actually Sneed distinguishes a variety of equivalence relations of various strengths) and T_1 reduces to T_2 when (very roughly speaking) there is a suitable map from the core of T_1 to that of T_2 such that the intended applications of T_1 are mapped into those of T_2.

This development is badly needed. For the first time, I believe, we have been put in possession of an apparatus sufficiently rich and precise to discuss the *mathematical* detail of identity, equivalence and reduction among MTP's. This most crucial aspect of MTP's is usually brushed over in the informal discussion of these concepts in the literature and yet it contains many subtleties and illuminating insights (such as the crucial and complex role of constraints). On the other hand Sneed does not supplement his account with an explicit semantics rich enough to discuss, e.g. the fate of the predicates of a reduced theory. (Sneed clearly seems to assume substantial identity between intended applications, but what of the properties of these entities, are they, e.g. contingently identified too and if so what is the semantics of this difficult notion? And how is it grounded in the mathematical structure?) Thus we are still short of a philosophically satisfying account here. The same might be said of the last chapter, where a great deal of it restates informal commonsense in Sneed's terms. (A theory may develop by developing the range and kind of its intended applications.)

But with all the conservative remarks said, I want to conclude by saying that the book is a worthwhile one, making a solid contribution to the technical foundations needed for seriously discussing some of these intricate issues. It is clearly (if dryly) written on the whole (there are some ambiguities) and closely argued. It is an ideal launching pad for further, more detailed research. *C. A. Hooker, University of Western Ontario.*

HILARY PUTNAM. ***Philosophy of logic.*** New York, Evanston, San Francisco, London: Harper & Row, Publishers, 1972. vi + 76 pp. $6.50 (hardcover). $1.60 (paperback). np.

Philosophers of science, not simply logicians, will find much of interest in this book (despite its title). Within its extremely short length it provides a substantive overview of the role of abstract entities in science and it employs as many examples and arguments from physics as from mathematics and logic. This is one of those rare books which is both elementary without being trivial and sophisticated without either being overly intricate or presupposing a vast background of knowledge. Students of logic and/or philosophy of science will gain as much from this book as their professors—but the students may not be able to appreciate the depth of the thought that has gone into it.

From one point of view the book can be seen as an essay-length argument designed to show that modern versions of nominalism are intellectually dishonest and antiscientific and that a rational and undogmatic acceptance of modern science presupposes a realistic (platonistic) stance in regard to abstract entities. But it is more as well. The first half (Chapters I through V) includes a critique of the Leblanc–Quine substitution conception of logical truth and an argument against limiting the scope of logic to the first order. The Second half (Chapters VI

through IX, which can be read for the most part without the first half) concerns the role of abstract entities in physics and higher mathematics and it presents arguments against various attempts to "fictionalize" them and/or to "eliminate" them from the ontology of modern science.

It is important to note that the "realism" endorsed by the author is only remotely related to the crude views espoused by Plato in the dialogues, e.g. there is no suggestion that "the abstract triangle" resembles concrete triangles in any way. The author's realism is much closer to the view held by Gödel in his well known essay on Russell (in *The Philosophy of Bertrand Russell*, edited by Paul Schilpp, New York, 1963, pp. 125–153). Indeed, the arguments employed by the author are largely more detailed versions of the "indispensability arguments" used by Gödel (e.g., *ibid.*, p. 137). Such arguments relieve some of the discomfort of wounds inflicted by rash use of Occam's razor and they make clear that "science" is not necessarily on the side of the nominalist. And this is all to the good in the reviewer's opinion. But after reading the book the reader is left in a quandary: he forms a strong suspicion that realism cannot be easily dismissed, but he still does not have a satisfactory statement of what realism *is*. The author has deftly encapsulated variants of nominalism, but realism is only sprinkled through the book.

For the most part the writing is unusually clear. The author even apologizes for his single neologism and he spares the reader those perverse displays of "learning" which send even the middle aged doctor of philosophy scurrying off for his *Webster's Unabridged*. Indeed, the book is highly quotable. Some samples are provided to give the reader an impression both of the literary style and of the philosophic style.

> "There is no reason in stating local principles to be more puristic, or more compulsive about avoiding reference to nonphysical entities, than in scientific discourse generally." (p. 14)
>
> ". . . 'true' makes no sense when applied to a physical object, . . . it is not the physical sentence that is true or false, but *what the sentence says*." (p. 18)
>
> ". . . [the logician] may just be constructing an uninterpreted formal system, but then he is certainly not doing logic." (p. 31)
>
> "Nominalists must at heart be materialists, or so it seems to me: otherwise their scruples are unintelligible." (p. 36)
>
> ". . . for now I simply dismiss conventionalism on the ground that no one has been able to *state* the alleged view in a form which is at all precise and which does not immediately collapse." (p. 62)
>
> "If the very things that make the fictionalist regard material objects, etc. as 'useful fictions' do not make it rational to believe the material object 'conceptual system', what could make it rational to believe anything?" (p. 70)
>
> ". . . [scientists] want successful predictions in order to confirm their theories; they do not want theories in order to obtain the predictions, which are in some cases of not the slightest interest in themselves, but of interest only because they tend to establish the truth or falsity of some theory." (p. 72)
>
> ". . . verificationism has made the perfectly sound and elementary distinction between truth of a scientific theory and truth of its observational consequences unpopular. . ." (p. 73)

The book is technically sound but there are two difficulties which might be worth noticing. (1) An otherwise accurate account of the inadequacies of a "nominalistic" account of logical truth is marred by the author's apparent presupposition that each formalized language comes with an intended interpretation. The latter is implied by his usage of the phrase "truth of all substitution-instances of L_i" (without giving an interpretation, e.g. p. 13). This slip allows the author to overlook the role of "universe of discourse" in the definition of logical truth (especially pp. 12, 16, 17). It also allows him to overlook the fact that in this context the nominalist is not trying so much to avoid classes as to avoid "possible worlds." Instead of taking logical truth as truth in all possible worlds the nominalist wants to define logical truth as truth (in "this world") of all substitution-instances. The same difficulty might also be at the base of the author's misleading comparison of (a) metalinguistic statements that first-order sentences are logically true with (b) the corresponding universally quantified second-order object language statements (p. 31). (2) A misleading statement of a metatheorem occurs in Chapter V (p. 38). The author states, ". . . there are finitely many statements $S_1, S_2, \ldots, S_n$ such that for an arbitrary state-

ment S, either $S \equiv S_i$, or $S \equiv S_2$ or . . . or $S \equiv S_n$ and moreover (for the appropriate i) $S \equiv S_i$ follows logically from the statement 'the number of individuals is N'." The first part of this conjunction seems trivial because of the tautology '$S \equiv P$ or $S \equiv \sim P$' (so N can always be taken as two). But the second part of the conjunction and other contexts implies that the author intended something like the following. "For each N there are m statements $S_1, S_2, \ldots, S_m$ such that for an arbitrary S the statement 'the number of individuals is N' logically implies one of the statements $S \equiv S_i$."

The reviewer noticed only three mechanical errors each of which would cause trouble only for a beginner. The first prepositional phrase in the sentence quoted above from p. 14 should be moved. On p. 56 the author says that his case for realism is qualified but he must mean that his realism is what is qualified. On p. 69 'predicative' should be 'predictive'.

The chief objection to the book is that it is too short. An index and more references to the literature would also increase its utility. Taking account of various aspects of this book which may be objectionable, it is still an important contribution to contemporary philosophy. In the reviewer's opinion the author has succeeded in his stated goal of "shaking up preconceptions and stimulating further discussion." *John Corcoran, State University of New York at Buffalo.*

PAUL DIESING. ***Patterns of discovery in the social sciences.*** Chicago: Aldine Atherton, 1971. x + 350 pp. np.

This valuable book locates itself in the gap between the philosophy of science as usually executed by philosophers and "methodology" books written by social scientists. It is of importance to philosophers of the social sciences, anyone interested in the methodology of the philosophy of science, and to practitioners of social science. This book distinguishes itself from most philosophical writings about the social sciences by operating, in fact, at a level close to the actual context of social scientific inquiry. It may be of significant value to social scientists in increasing their self-awareness and effectiveness in their search for knowledge, which is no small virtue. Philosophers can learn a great deal from Diesing about the actual logic of social inquiry. Further, this book is a good example of what can be done by philosophers to reorient the philosophy of science so that it can promote the growth of scientific knowledge. Diesing focuses on two methods of inquiry in the social sciences—the formal method and clinical and participant-observer methods. His primary interest is in the process of the growth of knowledge in the social sciences and he starts with problems which arise within the context of inquiry, rather than problems arising from a philosophical model of science. He is not a Procrustes, stretching and cutting science to make it fit some philosophical bed, nor does he focus upon problems which have no value in furthering inquiry, e.g. grue and bleen. The relation to Hanson's work suggested by the title is intentional for, like Hanson, Kuhn, Kaplan and a few others, Diesing is interested in how science actually works. He shows that if one does look at the actual process of inquiry in the social sciences, it is a systematic process and there are characteristic patterns of the growth of knowledge.

The explicit subject matter of the book is the methodologies of mathematical modelling, and clinical and participant-observer inquiry. The latter two methods are taken as basically the same by Diesing and below I shall adopt his term, 'case study methods', to name them both. By method, Diesing means "the whole series of steps that a scientist or research team follows in the process of making a contribution to a field of knowledge" (p. 1). He distinguishes four basic methods currently in use in the social sciences: experimentation, statistical survey research, formal methods (with computer simulation as a subtype), and case study methods. Diesing looks at each of these methods as evolving through time, sometimes cross fertilizing each other, with a discernible and evolving "logic." Thus, mathematical modelling has borrowed elements from the experimental tradition, although the formalist experiment is quite a different sort of thing from the experimentalist's experiment. The texture of this book is dense, with a huge number of studies discussed, and it defies more than skeletal summary. Some of the questions Diesing asks of the two methods he studies are what are the basic steps in using this method, how is theory developed, what types of theories result from this method, what is the role of evidence, how is the objectivity characteristic of this method obtained and what is the implicit ontology of this method? In answering these questions Diesing provides a clear and valuable discussion of a broad range of issues arising from the process of scientific inquiry in the social sciences. He shows that each of the methods he discusses are distinct, coherent and systematic ways of obtaining scientific knowledge. Henceforth, anyone who wants to make sweeping claims about "the scientific method" will have to deal with Diesing's evidence against the unity of method in the social sciences.