his identity unknown to John" (my italics). A superficial point, perhaps, but one which can lead to trouble for philosophically unsophisticated writers such as those theologians who claim theology to be scientific because it is "the *objective* study of the idea of God, *His* being and *His* relation to the world . . ." (my italics). For most readers the phrase 'the idea of his being' (let alone 'the idea of His being') presupposes an actual as opposed to a possible referent for the possessive pronoun. Unlike Hintikka's phrase 'Homer's epistemically possible non-existence', which is intended to be translatable into non-committal terms, the phrase 'the objective study of (the idea of?) . . . His being', while conceivably so translatable, is clearly (in view of the capitalized 'His') not intended to be non-committal. In philosophical literature this kind of ontologically-loaded short-hand, while harmless enough when understood for what it is, could well confuse the reader struggling to follow. ("If he can't follow it he shouldn't be reading it" could reasonably be adopted as a precept only by writers of the doctrine of a secret sect.)

The article by Åqvist on action and causality is a model of precise thinking in a philosophical context which attracts more than its fair share of obscure and verbose attention. The concepts of action and causality are sufficiently finely tuned in ordinary speech to admit of precise formal manipulation in set-theoretic terms. For those without the necessary expertise, however, a concluding section surveying the ground covered and summarizing the philosophical pay-off would have been a welcome finishing touch.

Hermerén's article on the concept of 'model' is a valiant attempt to sort out all the different senses in which that term has been used. It has become so fashionable in so many disciplines to give an appearance of rigour by talking in terms of models, that this term has become stretched out of all recognition. Too often it is yet another case of wool-pulling by the imprecise use of precise language. Hermerén's attempt to classify seven fairly distinct senses is a step in the right direction. The trouble is that 'model' cannot be clearly explicated in isolation from 'theory'. Now 'theory' is ambiguous at least as between 'set of hypotheses' (theory$_1$) and 'interpreted nth-order system' (theory$_2$). But if 'theory$_2$' is, as it is intended to be, an essential part of the explication of 'theory$_1$', the difference between Hermerén's *theoretical model* and his *interpretative model* is unclear. A related point arises when he uses the expression 'isomorphic models' to name one of several *kinds* of model. Isomorphism is a *relation between* models (in one discipline at least), not, as he would have it, a relation between the model and the thing modelled (p. 186). Perhaps the biggest difficulty in talking about the various *senses of the word* 'model' is in avoiding talking as if there were different kinds of models—i.e., as if there were a generic concept of *model* under which all specific senses fall, subject to different restrictions.

A useful book which should start many more hares running.

G. B. KEENE

Deviant Logic. By SUSAN HAACK. (Cambridge University Press. 1974. Pp. xiv + 191. Price £3.80.)

The purpose of this book is to examine the philosophical problems posed by the existence of logics which appear to rival, in some sense, ordinary (two-valued) logic. It is tightly packed, closely argued, and gives a unified treatment to such apparently diverse subjects as intuitionist mathematics, Aristotle's argument for fatalism and quantum logic. The book is in two parts which I shall discuss separately.

The two key questions that are posed by the existence of rival logics are firstly "What is logic?" (What is its epistemological status? Is it something that could be rejected?) and secondly "Exactly what does it mean to say that one logic rivals (is an alternative to) another?".

The first part of the book is largely concerned with answering these questions.

Logic, it is argued, is a very general, but none the less ordinary (scientific) theory. It may therefore be rejected under suitable conditions (e.g., in the cause of simplicity, coherence, etc., of the overall set of beliefs). The position is essentially Quine's of the "Two Dogmas" period and rests heavily on Quine's attack on the analytic/synthetic distinction and his indeterminacy of translation argument. The author dubs it "pragmatic".

The arguments for the pragmatic view are clearly and cogently presented. However, in view of the crucial importance of this view, I felt that more time could have been taken to examine it. Questions such as "Why should one modify one's belief-set in the face of "recalcitrant" experience?", "Exactly what is an acceptable modification (p. 36), and what is coherence (p. 26)?", "If any belief may be rejected, may we not reject the belief that pragmatic considerations should determine the way we modify our beliefs?" ask to be considered.

In answer to the second key question, the author says that a rival system is one "the use of which is incompatible . . . with the use of the standard system" in the sense that it "should be employed *instead* of" it (p. 2). A formal system of logic is said to be *deviant* if (roughly) it is a non-conservative extension of classical (two-valued) logic, and *vice versa*. The author then demonstrates that deviance is neither a necessary nor a sufficient condition for rivalry. (This is not perhaps surprising since rivalry is defined in terms of the pragmatic notion of *use*, and deviance is a purely formal consideration about the theoremhood of certain strings of symbols, etc.) Although rivalry may not be formally characterizable, however, there are logics (e.g., intuitionist) which are intended by their proponents to be used instead of classical logic.

The author then disarms an argument of Quine (from *Philosophy of Logic*) and others, to the effect that there is no genuine rivalry between such systems, since the meanings of the connectives in the rival systems must differ. The author establishes that this is not necessarily true. She could further have pointed out that even if it were it would not show there was not genuine rivalry between the systems. After all, relativistic and classical mechanics are certainly rivals and as many people have pointed out (e.g., Kuhn, Feyerabend) a term such as 'mass' seems to have different meanings in these theories.

Throughout this section (and in fact the whole book) no distinction is drawn between formal languages and natural languages. I feel this is a pity, since some of the above arguments would have come out clearer had the distinction been made. Clearly arguments that are relevant to the former (e.g., about sets of theorems or number of matrix values) are not directly applicable to the latter and *vice versa* (e.g., the argument from indeterminacy of translation). The same distinction would also have been useful on pp. 61-4 where there is a discussion of whether the use of a many-valued logic commits us to the rejection of the principle of bivalence. The answer to this question is clearly "It depends how you use it". That is, the answer depends on the intended (informal) interpretation of the logical symbols or matrix values (as the author, in effect, points out).

An additional benefit of drawing this distinction would have been to raise the important issue of the relationship between formal and informal languages. Is the purpose of formalization to construct a formal system that behaves like ordinary language, or may formalization be instrumental in bringing about a change in logic? (There is a brief discussion of this point on p. 119.) This is very closely connected with a deviant logic which, rather surprisingly, does not get a mention in the book—the Anderson and Belnap system of entailment. A discussion of whether the paradoxes of material implication force us to look beyond classical logic for a satisfactory formalization of "If . . . then . . ." is certainly within the compass of the book.

The second part of the book looks at a number of particular situations which some people have argued require a change in logic. (Future contingents, Intuitionism, vague-

ness, free logics, quantum logics.) Using the results of part one, the author examines (a) whether the situation genuinely requires a change in logic, and (b) whether the modifications that have been proposed are satisfactory. Each chapter provides an excellent analysis of the particular situation and a close scrutiny of the standard positions.

There is only one major point in this part where I wish to take issue with the author —her solution to the problem of the existential commitment of classical logic. Both of (1) $(\exists x)(Fx \vee {\sim}Fx)$ and (2) $Fa \supset (\exists x)Fx$ are theorems of classical logic. The second, however, is false if 'a' fails to denote, and the first seems (at best) only contingently true. The author argues that if we interpret the quantifiers substitutionally (instead of referentially), then the problem disappears. This is not altogether clear, however.

Firstly, if we read (1) as: (3) Some substitution instance of 'Fx $\vee{\sim}$Fx' is true, then it would seem to entail in some way the existence of a substitutional instance of 'Fx $\vee$ ${\sim}$Fx'. (It would be most odd to assert (3) and deny the existence of such an instance.) If therefore one objects to (1) as a theorem of logic on the grounds that it is only contingently true, then presumably one could object to (3) on the same grounds.

Secondly, the author accepts an argument of Wallace (Noûs 1971) that substitutional quantification does not entail all instances of the Tarski T-scheme, i.e., one no longer has reason to assert in general that (4) 'Fa' is true iff Fa. But now it seems that (2) is in danger. For suppose I accept that Fa. I can still deny that $(\exists x)Fx$, i.e., that some substitution instance of 'Fx' is true. It can no longer be asserted that 'Fa' is such an instance (i.e., a true one) since we have given up (4).

Although I have criticized the book, my criticisms are, in the main, of omission, and do not detract from the fact that the book achieves its aim with admirable clarity. I can therefore recommend it to anyone interested in non-classical logics in particular, and the philosophy of logic in general.

There is a useful appendix containing the matrixes of all the standard many-valued logics. The following are misprints:—
p. 62: two occurrences of "P_{m-1}" should be "p_{m-1}"; p. 97: "m(7_IA)" should be "m($\neg_I$A)"; p. 101: "$A \supset 1 : 1 = 0$" should be "$A \supset 1 = 0$"; p. 144: "(x)" should be "$(\exists x)$". Finally, the statement on p. 97 that the Gödel translation between intuitionism and S4 does not preserve deducibility is incorrect. (See Fitting, Intuitionistic Logic, Model Theory and Forcing, Lemma 7.2 p. 43 and the relevant completeness proofs.)

GRAHAM PRIEST

Ontological Reduction. By REINHARDT GROSSMANN. (Bloomington and London: Indiana U.P. 1973. Pp. vi + 215. Price £4.50.)

Despite its title, this book does not discuss the procedure of ontological reduction explicitly and systematically. Rather, it discusses some specific and some general ontological questions, in a generally anti-reductionist vein. Though an overall onto-logical view is implicitly put over by the end, there is no very strong unifying thread of discussion. I will not try to summarize the contents of the book as a whole, or any of the particular discussions, but will merely touch on three striking elements in Gross-mann's approach.

(1) Grossmann attacks ontological reductions which are based on material equi-valences, or supposed definitions of various kinds. He argues, for example, that the material equivalence of "The class determined by the (relational) property of being [numerically] similar to f is the same as the class determined by the (relational) property of being similar to g" and "The number of f's is the same as the number of g's" does not imply that the number in question is identical with the class in question (pp. 37 ff.). And most of his lengthy discussion of the ontological status of numbers depends on moves of this (not implausible) sort.

However, the alternative criterion of identity he eventually adopts (pp. 40, 48-51)