

new body rather than Y as a person with a new brain, seems to have escaped Penelhum entirely.

But as I said earlier, the basic error is rectifiable and, once this is done, does not really affect the sense of Penelhum's arguments against the coherence of personal survival. One need only substitute for 'bodily identity' the words 'brain identity'. The same difficulties he cites arise when we talk of 'disembodied persons' and 'persons with a resurrected brain'. The advantage, however, of making this amendment is to lend scientific respectability to what is after all an admirable exercise in philosophical acuity.

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ROLAND PUCCETTI

*The Problem of Inductive Logic*. Edited by IMRE LAKATOS. (Amsterdam: North-Holland Publishing Co. 1968. Pp. viii + 417. Price 105s.)

This book is one of three containing the revised proceedings of an international colloquium on the philosophy of science held at Bedford College, London, in 1965. It contains the papers given, as later amended, and suitably polished versions of the subsequent discussions. The topics dealt with are: models of probability (Freudenthal), justification of inductive rules of inference (Salmon), rules of detachment in inductive logic (Kyburg), probable knowledge (Jeffrey), enumeration *vs.* elimination (Hintikka), consilience of inductions (Hesse), inductive logic and intuition (Carnap), and a subsequently written history of the problem of inductive logic (Lakatos).

What one finds of interest in this large and expensive volume depends largely on one's attitude to the pervasive Wagnerian epic strife of Popperians *vs.* the rest, especially well catered for in Lakatos' long culminating *Götterdämmerung*. The prevailing histrionic tone, and metaphors of boldness, strife, and the slaying of theories (e.g. p. 395) I guess owe more to the barricaded atmosphere of L.S.E. than to their inherent clarifying power or any convincingly evidenced feature of scientific practice. (In which connection, as editor of the *Br. J. Phil. Sci.*, I must note that the paper referred to at the bottom of page 324 never appeared because it was never received, not because of any editorial bias!)

For those whose prime interest in a good idea is not when Popper (or Carnap, or Wittgenstein, . . .) first had it, there are some less personalized and more dispassionate discussions. Some are more technical than philosophical, and within each class the best seem to me those by Hintikka and Carnap. The one develops a system to cater for both eliminative and enumerative aspects of inductive practice, put clearly enough to assist a profitably specific discussion of its strengths and weaknesses, and hence of specific features of inductive practice. The other neatly shows the parallel dependence of both inductive and deductive systems in use on a preformal ability to see the validity of the simplest instances of such reasonings.

Hesse's paper explores a problem of conjoining probabilistic confirmation with hypothetico-deductive explanation—that merely adding a theory to two laws deducible from it cannot consistently raise their probabilities. The proffered solution, in terms of theories reinterpreting laws to show analogies between them, is more suggestive than conclusive, given the vagueness of the notions appealed to.

Salmon's paper rehearses some well known objections to Carnap's analytic probabilities as guides in life and argues that, instead, the vindication of induction requires synthetic statements. For this he puts forward Reichenbachian frequency posits, being careful, however, to avoid both the lottery paradox and any serious relevance to the problem of induction by refusing to apply them to any single case. There follows some relevant discussion of the distinction between estimated and inferred probabilities and then, very aptly, Kyburg's paper on rules of detachment. This concerns the central issue of whether induction is or is not primarily concerned with inference, i.e., a process leading to accepting statements as true without further reference to their inconclusive evidence. If so, as one would naturally wish, the problem of the relations of evidential support and other features of a statement to its acceptability must of course be faced. But neither Kyburg's paper nor the subsequent discussion seemed to me to face it at any depth.

The equally deep issue then raised, of the relation of knowledge to belief, again seemed to me inadequately discussed. The extreme approaches here never meet in the book: "Popper's thesis that 'belief-philosophy' has nothing to do with the philosophy of science" (p. 383) is not pressed against Jeffrey's attempt "to make the concept of belief do the work that philosophers have generally assigned the grander concept [of knowledge]" (p. 166). The discussion of Jeffrey's paper seemed to me almost the most disappointing of all because of its failure to face obvious objections to his very fashionable subjective approach, and on this I should like to take brief space to comment.

First, Jeffrey connects betting and knowledge by "restrict[ing] attention to certain 'natural' gambles, in which the prize for winning is the truth of the proposition gambled upon" (p. 168). This notion is derived from the theory of Jeffrey's *Logic of Decision*, where belief and desire are simultaneously measured by preference rankings of propositions and their negations. But it is not clearly put here; in fact it is put the wrong way round. The *truth* of the proposition is what is gambled on, and what is won is the value the gambler attaches to the possible world that occurs, in which the proposition *is* true. This value may not at all coincide with what he previously put on the proposition, which is an average of the values of all such possible worlds (in which the proposition is true), weighted by the subjective probabilities (however bizarre) he assigns to them. All this seems to me to leave quite unsettled the two crucial questions: (i) what *is* a truth worth to the man who's won his bet on it? (ii) how does a subjectivist ever settle a bet? Without serious answers to these questions, the betting metaphor is empty.

But there is more. Jeffrey effectively characterizes observation as that which (causally) compels certain degrees of belief (e.g., p. 176). Since he has no other, objective, way of characterizing it, he cannot distinguish his instances, which we would all grant to be so, from (say) an emotional crisis, which could similarly compel a degree of belief (high, say, in a lover's faithlessness). How, without other criteria for distinguishing observation from dyspepsia as a source of belief, can Jeffrey make sense of saying that "coming to have *suitable* degrees of belief . . . is a matter of *training*" (my italics)? Suitable for what? By what means trained? By what constraints corrected—Alka-Seltzer, perhaps? And if there are other criteria, as it seems there must be, which can be appealed to in assessing the evidential relevance of experience, what relevance does this endless introspecting, of shifting betting quotients on unshakable bets for indeterminate prizes, have for the analysis of our knowledge, probable or certain, of an outside world? These matters are not here as adequately discussed as they need to be. One final point. Jeffrey claims as an advantage of his theory "that here we work with the subject's actual beliefs", but his theory ascribes to them certain coherence constraints, and he presents no evidence that these are usually (or ever) satisfied by actually expressed preference rankings. Who or what, one wonders, then, is the subject of the theory? Especially since Jeffrey denies, in answer to Suppes (p. 189), that his theory "takes any account of the specific powers and limitations of human beings".

D. H. MELLOR

*Logic and Philosophy: A modern introduction.* By HOWARD KAHANE. (Belmont, Cal.: Wadsworth. 1969. Pp. xv + 450.)

This is a textbook for a one-year introductory course in logic, aimed at the "average" (American) student. Propositional and predicate logic (including identity, definite descriptions and properties of relations) are presented first, with emphasis on their use rather than their formal properties. There is then an unusually long, self-contained section on syllogistic logic, followed by three chapters on induction and scientific method, three on selected philosophical problems to which formal logic is held to be relevant, a chapter on the nature and properties of formal systems, and an appendix on intuitive set theory. There is a companion "study guide" volume (not reviewed).

The material is well laid out, the pace is slow, with plenty of examples, and points are clearly made. Truth functions are presented first, and the notion of a valid argument form defined as one such that none of its substitution instances has true premises and a false conclusion. Then rules of inference are introduced and justified in terms of this truth-functional property.

The difficulty with this approach is that the whole system depends directly for its plausibility on the truth-functional analysis, including that of conditionals. The author convincingly argues that no *other* truth-functional analysis of conditionals will do, but asserts rather than argues that "in general, that part (of their meaning) captured by material implication is all that is needed to solve problems containing implications". There is an apparently fallacious argument on page 26: "Every conditional sentence . . . is such that if its antecedent is true and its consequent is false, then the conditional sentence is false. Therefore, a conditional sentence is true if it is not the case that its antecedent is true and its consequent false." Later, in the section which purports to show that logic is a useful tool in philosophy, the problem is raised that dispositional and counterfactuals contain conditional elements which cannot be expressed as material implications. There is a danger that all but the most credulous reader will conclude that "logic" is not relevant to philosophical analysis here, nor to anything much besides.