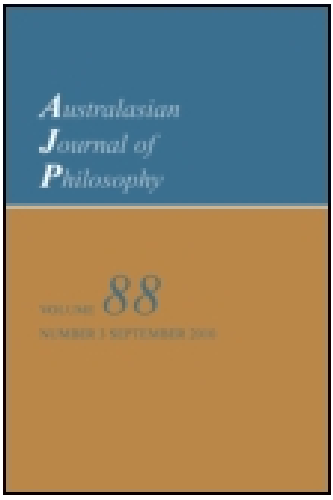

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Reviews

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REVIEWS

Aune, Bruce, *Reason and Action,* Philosophical Studies Series in Philosophy, Vol. 9, (eds.) Wilfrid Sellars and Keith Lehrer, Dordrecht, Holland, D. Reidel Publishing Company, 1977, xi, 206 pp.

This discussion of the nature of actions and of practical reasoning is so well written and contains so many sensible, intelligent ideas, that it should not remain unnoticed. Aune's theses on these topics are, as he admits, almost all derived from those of Wilfrid Sellars. Indeed at times the work appears to be just an elaboration and slight modification of Sellars's views. But the exposition and defence of those views, and the qualifications made to them by Aune, are clear and interesting.

In the first of the four chapters which comprise the book, Aune asserts that people's actions involve volitions and movements caused by volitions, but after considering different versions of such a volitional theory concludes that there is little to choose between Prichard's view that actions are only volitions, Davidson's opinion that actions are the movements people bring about intentionally, and the 'composite' theory of R. G. Collingwood that actions are composed of both volitions and the movements they cause. Of more interest in this chapter is Aune's argument that the logic of action sentences does not oblige us to include events and actions as part of our ontology, as Davidson has maintained. Referring to Romane Clark's analysis of adverbial modification, Aune shows that action sentences may be interpreted as just stating predicates, often quite complex predicates, of agents. Instead of a theory of actions, Aune suggests we adopt a theory of agents.

Chapter two is appropriately entitled 'The Springs of Action'. Central in it is an analysis of volition as a form of intention, more specifically as a thought of the form 'I will now do such and such' together with a propensity to act accordingly. This is almost pure Sellars. But the analysis is developed more fully here than it has been by Sellars, and it is related interestingly to other theories about the causes of intelligent behaviour.

Chapter three is an uncommonly clear discussion of Aristotle's view of practical inference, as well as of the theories of Von Wright, Gauthier and Kenny. Aune's own theory, again built upon suggestions of Sellars, is impressive, particularly in the way it sets out conditions of rational choice between alternative methods of achieving an intended goal and relates these to familiar models of practical inference.

Finally, in chapter four Aune defends the thesis that practical inferences can be validated by ordinary indicative logic, and do not require a special logic of practical reasoning. Again, the idea was originally Sellars's, but Aune argues that his account is simpler and clearer than that of Sellars. Aune considers Binkley's and Castaneda's theories of practical reasoning at considerable length, to show that 'they have not made a convincing case for a special system of practical logic'.

Anyone interested in the subjects which Aune has discussed is sure to find this book useful.

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Ziman, John, *Reliable Knowledge: An Exploration of the Grounds for Belief in Science,* Cambridge, England, Cambridge University Press, 1978. ix, 197 pp. £7.95.

John Ziman published his first work on the social dimension of science, *Public Knowledge,* in 1968, at a time when that formerly neglected area was increasingly becoming one of serious philosophical enquiry. At that time, Ziman's contribution, coming as it did from an experienced physicist of some standing, was informative and challenging. From a philosophical, and more specifically, epistemological point of view the work was decidedly underdeveloped and sketchy, its merits lying more in the line of investigation that it opened up, rather than in the achievement of a sound philosophical thesis. What is disappointing about the book under review is that the themes of the earlier work are not developed in a *philosophically* interesting way, and compare unfavourably in this respect with the work that has been done along the same lines by J. R. Ravetz, for example.

Ziman refers to the question 'is scientific knowledge reliable?' as 'the basic epistemological problem' (p. 77), but he does not really offer an adequate answer to that question, however interesting his observations about some aspects of the practice of physics might otherwise be. According to the author, 'the goal of science is a consensus of rational opinion over the widest possible field'. (p. 3) A fundamental problem that poses itself here concerns the grounds on which it can be argued that theories best able to command the consensus of a particular community correspond to those that are most reliable, in the sense of offering approximately true descriptions of, or being in some sense able to adequately come to grips with, some aspects of our world. Ziman discusses at some length and in some detail the social organisation of scientific communities together with some aspects of theories which strongly influence the consensus reached by those communities. For instance, the importance of novel predictions is discussed in terms of their psychological affect on the scientific community (p. 31) and we are told that 'what makes a discovery important, and scientifically exciting is the degree of surprise that it occasions. (p. 71) Even if Ziman's psychological and sociological observations are correct, the problem of why it is that theories which are able to evoke certain responses from the scientific community are especially reliable when applied to the world remains unanswered.

The weakness in Ziman's position to which I have referred is very much in evidence in his discussion of the fact that physical theories are in the main *mathematical* theories. Chapter 2 of the book, in which the matter is taken up, is headed by a quotation from Galileo. 'Nature is written in mathematical language.' Galileo's statement would seem to imply that it is the physical world itself that is in some sense mathematical. If this is a correct interpretation, Ziman's discussion is totally out of keeping with it because he justifies the use of mathematics in physics largely in sociological terms. Mathematics is 'the ideal language for scientific communication' because it is unambiguous and is a 'device for constructing messages with the maximum degree of clarity and precision'. (p. 13) The question of why it is that such theories should be reliable when applied to our physical world again remains unanswered, although I am certainly not suggesting that there is an easy answer.

There is the hint of a quite different account of why it is that physical theories are mathematically formulated, one that is more in keeping with Galileo's utterance, embodied in Ziman's remark that within a physical theory, 'the formal properties of the mathematical symbols must be isomorphous with the empirical relations of the categories they purport to represent in the real world'. (p. 163) If we take this remark seriously then it is possible to say that the real world either contains or does not contain certain categories that are mathematically related and it makes sense to say that a particular theory may or may not have correctly identified those categories. But if we admit this, then we must further admit that it is possible for the consensus of opinion reached by scientists to be wrong, so that the reliability of science cannot be justified mainly in terms of the extent to which it commands a consensus. Ziman indicates that

he is to some extent aware of the problem when he considers a position he refers to as 'cultural relativism' according to which science has no special privilege by comparison with any other systematised scheme to which a social group might subscribe. (p. 120) In order to distinguish science from other schemes and argue for its superiority over those schemes Ziman resorts to an appeal to a domain of common-sense knowledge able to command an almost universal consensus. We are essentially back to the observation language of the positivists which is notoriously inadequate to serve the purpose Ziman demands of it. In particular, it is impossible from that standpoint to cope with the major departures from common sense exemplified in modern physics, some of them well-described by Ziman himself elsewhere in his book.

'How much ought we to believe of what science might tell us about man as a conscious social being, subject to unreasonable emotions and irrational institutions?' This, according to Ziman, is the fundamental question of the book and he confronts it in the final chapter. What is offered is most disappointing and rather superficial. It is observed for example that the social domain, unlike the physical domain, lacks sharp categories, that it is not possible to experiment on people and society in the same way that it is possible to experiment on the physical world and that there is need for some empathy between the social scientist and the people that are the object of his investigation. None of these points is developed in a way that is particularly revealing from a philosophical point of view.

I have reviewed this book from an epistemological standpoint and found it wanting. However, when viewing it from the point of view of a general reader I find much in it that is informative and fascinating, as, for example, the sections on perception and pattern recognition. Perhaps the latter viewpoint is the one from which the book is most appropriately judged.

Alan Chalmers *University of Sydney*

Platts, Mark, *Ways of Meaning*, London Henley and Boston: Routledge and Kegan Paul, 1979, xii, 272 pp., $3.95.

Theories of meaning have played a key role in 20th century philosophy. A theory of meaning (though a different one in each case) lies at the heart of logical atomism, logical positivism, Wittgenstein's *Investigations*, etc. However, in recent years, we have seen the theory of meaning pure and simple become the target of philosophical investigation. Papers by Donald Davidson, Paul Grice, Saul Kripke and many others, have made the philosophy of language a distinct and distinctive branch of philosophy. However, the papers occur in widely scattered places and many of them make few concessions to the non-cognoscenti. While this may pose no problem for the professional philosopher, it certainly creates one for students and it is for this reason that this book is to be welcomed. Based (I presume) on Platt's lecture notes, the book sets out to initiate the novice into the world of T-schemes, the causal theory of reference and the graspability of sense. By and large it succeeds.

The book starts with an elementary account of Tarski's theory of truth, which it then uses to argue for a variant of Davidson's theory of meaning. The next chapter deals mainly with Grice's contributions to the theory of meaning: a discussion of conversational implicature (the relevance of which to the main thread of the argument is somewhat obscure) and meaning$_{NN}$. The middle part of the book comprises chapters each of which discusses the issues involved in formulating the semantics of some particular grammatical feature: quantifiers, indirect discourse, proper names, adjectives and adverbs. In each of these chapters Platts gives a good guide to the current literature on the topics, though the work of Richard Montague is strangely neglected. The

penultimate chapter of the book discusses the connection between meaning, understanding and verification and the final chapter (somewhat tenuously connected with the rest of the book) discusses moral realism.

I have a number of minor criticisms of the book. What discussion there is of logical matters is often superficial (e.g. substitutional quantification p. 15, quotation p. 24) and there are occasional logical mistakes (e.g. on p. 31 Platts seems to think that sequences can be conjoined and disjoined). Another unfortunate feature of the book is the absence of any T-scheme from the middle part. In the first part of the book, great play is made of the fact that any adequate semantic account of a linguistic feature should deliver us the appropriate T-schemes. Despite this in the middle part, whenever Platts gives a semantic account of features which he claims to be correct, he never shows that this delivers the appropriate T-schemes. Sometimes, e.g. with certain non-standard quantifiers, this may be pretty obvious. Others, e.g. with the paratactic analysis of indirect discourse, it certainly is not.

A more serious flaw, especially in a text book, is the speed with which some of the discussion is conducted. Brevity is, of course, not necessarily a defect but sometimes it can be misleading and this it certainly is in Platts' discussion of Davidson and Davidson-type theories. On p. 56 Platts dismisses Davidson's account of meaning in one paragraph. A theory of meaning cannot be a theory of truth. For a theory of truth presupposes the notion of translation (in the metalanguage) and this smuggles in the notion of meaning, of which we are trying to give an account. The reason this is misleading is that Davidson insists that certain empirical constraints must be satisfied if a theory is to be a truth theory. Translation is not presupposed, but delivered by the empirical constraints. The references to the empirical constraints in the early Davidson papers such as 'Truth and Meaning' are quite brief, but later papers such as 'Belief and the Basis of Meaning' make it quite clear. Platts himself gives essentially McDowell's account of how the empirical constraints are to work, and here the student will find Platts' brevity more than a little confusing. Given a putative truth theory for the language of a particular group of users, we test it by taking the T-scheme for a certain sentence s, $\ulcorner s$ is true iff $p\urcorner$, and seeing whether the users utter s assertively when they may reasonably be expected to believe that p, utter s demandingly when they may reasonably be expected to desire that p and so on. So far so good. But how is one supposed to know which of the infinitely many sentences of the form $\ulcorner s$ is true iff $p\urcorner$ delivered by the theory is the T-scheme for s? On this point Platts is silent. (Indeed it is not at all obvious that he realises that there is a problem here.) The answer, presumably, is that the T-sentence is to be chosen with respect to some canonical proof theory. (I am grateful to Barry Taylor for a discussion of this point.) Briefly certain parts of the metatheory (e.g. sequences) are officially designated as auxiliary devices. We suppose that we have some standard order in which theorems of the theory are proved. We take the first theorem in such an ordering of the form $\ulcorner s$ is true iff $p\urcorner$, eliminate the auxiliary machinery in $\ulcorner p\urcorner$, again in some standard way, and take that to be the right hand side of the T-scheme for s given by the theory. If, in fact, the details of such a procedure can be spelt out (and I am by no means convinced that they can) it will be only with some difficulty and no little logical acumen. None the less it is essential to say something about this process or Davidson-type theories cannot even get to first base.

So much for the way the book is written. There remain Platts' views on various issues. Clearly this is not the place to say a great deal, but I would like to comment on Platts' position with respect to the semantic paradoxes. Platts says that the best way to deal with these is to accept only a language-relativised truth predicate and 'to hold in addition that the predicate *is true in L* cannot be part of L'. (p. 41) This is no doubt, the orthodox position, but in the present context it is thoroughly inadequate. No one doubts that it is possible to construct an artificial language which does not contain its own truth predicate, but the problem that Platts faces is to give a semantics for natural languages and, in particular, English. Quite manifestly 'is a true sentence of English' is an English predicate and we are not at liberty to exclude sentences containing this predicate by fiat.

Neither is another 'solution' of the semantic paradoxes open to Davidsonians, viz. rejection of T-schemes. Not only is the holding of all T-schemes (for sentences without indexicals) a condition of adequacy on Tarski-type truth theories but even if it were not, a truth theory for which it failed could hardly be a theory of meaning. For it is precisely the T-scheme for a sentence which gives its meaning. The semantic paradoxes prove a very difficult problem for Davidsonians and one, moreover, that they have avoided rather than faced. Doubtless this and other issues aired by Platts' book will be discussed in the literature for a good time to come, and I have no doubt that Platts' book will help promote the discussion.

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Kroy, Moshe, *Mentalism and Modal Logic,* Wiesbaden, Athenaion, 1976, x, 279 pp. (No price supplied)

This book contains some interesting arguments for some overstated conclusions. On the side of truth, the author has mentalism, minds, souls, spirit, Subjects, modal logic, Chomsky linguistics, propositions, propositional attitudes, the imagination, and phantasy. On the side of falsehood, he has physicalism, behaviourism, modern academic philosophy, classical quantification logic, physical determinism, mechanism, and Tarski's theory of truth.

Physicalism, strictly, is the doctrine that everything is physical. It might help if we adopt a working account of physicalism, as the thesis that at least one true and complete description of the universe contains only terms from physics and chemistry. This squares more or less with fairly common philosophical usage (e.g. Feigl, Smart). The author characterises physicalism differently, indeed in a number of ways: (1) the thesis that all entities have spatio-temporal location (p. 6), (2) the thesis that only objects defined as entities which bear only properties and relations to other objects exist (p. 49), (3) the thesis that there are no Subjects, or that there is no viable concept of Subject (p. 47), (4) the thesis that all regularities are causal (p. 58), (5) the thesis that the correct description of reality needs only languages with a Tarskian truth theory (=extensional languages?) (55). The author argues that this disparate collection of doctrines is false. His view is that they are false because there are Subjects. Subjects are those entities denoted by the subject terms of true propositional attitude sentences. Propositional attitude sentences are at first identified syntactically (p. 20), but later, true ascriptions of propositional attitudes are conditioned by the further requirement that they entail that the Subject in question have a mind (p. 24). The author claims that to be a Subject, to be subject to propositional attitudes, to have a mind, is something which physicalism cannot accommodate. Nevertheless, there appears to be no particular reason why physicalism as ordinarily understood should accept (3) above. Many people calling themselves physicalists have held that there are minds, wants, beliefs, etc. Let us grant that physicalism needs some sort of reductive account of minds and propositional attitudes, such as a functionalist one or some sort of contingent identificational strategy. But it isn't as if physicalism has not busied itself with such accounts. The author makes no attempt to discuss these accounts. Moreover he can hardly object to such reductionist strategies, since he offers a reduction of sorts of the propositional attitudes himself. Hence, *prima facie* at least, physicalism need not accept (3) above.

Is Physicalism as currently understood (not Kroy's version, that is) committed to any of (1) - (5) above? Perhaps something could be said for (1), though (1) would need to be more carefully stated than Kroy does in order to avoid puzzles about such entities as fields, or the mathematical entities needed for physics. Thesis (2) is, of course, circular, and I am unable to find a noncircular account of objects; unless perhaps they are defined

negatively, that is as non-Subjects. But to do so would make (2) equivalent to (3), and so something physicalism need not accept. We will examine (4) briefly, later. As to (5), we will see that Kroy advances a case for its falsity, though its connection with physicalism, especially in the light of Kroy's mentalistic interpretation of the possible worlds semantics for modal logic, is unclear. More on this later too.

The charge that Kroy has physicalism wrong would be a rather verbal one, if it were made clearer just who the enemies are. Quine gets a mention, and I suppose that a case could be made out that he is associated with one or more of (1) - (5) above. Attempts by the early Wittgenstein and Russell to deal with the propositional attitudes in the name of extensionalism (not physicalism) are indicated. It is not clear, however, whether these count as attempts to *abolish* propositional attitudes and minds, so much as proposals to *analyse* them in a certain way. Perhaps the nearest we get to an answer to the question of who Kroy's opponents are is that it is the academics, philosophers and psychologists especially. '. . . many modern philosophers have no use for the concept of the subject (soul, mind, spirit, self)'. (p. 37) "The term 'imagination' is part of a mentalistic framework, hence is academically unacceptable nowadays". (p. 101) '. . . Physicalism is academically accepted in an uncritical fashion'. (p. 102) 'Only the academic philosopher *ex cathedra* pretends not to understand'. (p. 102)

The reason Kroy has for thinking that extensional languages are unacceptable is the familiar one that the logic of propositional attitudes is intensional. He adopts an analysis of the propositional attitudes in terms of possible worlds. He avoids the problems engendered for some such accounts by the fact that possible worlds are complete by use of Hintikka-style model sets. In some of the best sections of the book he analyses the concepts of the imagination and of mental planning in order to show how this sort of approach can give a not unreasonable theory of their operations, and a sense to the idea of creativity.

There is an epistemological problem for any such view, as Kroy acknowledges, unless other possible worlds can be causally connected to our own. Kroy proposes the reduction (or replacement, it is not clear which) of the various world-type structures he needs to sentences in the head. He introduces the idea of a model system, which is a finite cut-down of a model set and so can be contained in a finite head. But it seems to me that he is in a dilemma. Either finite, sentence-in-the-head model systems in our heads in this world are sufficient to account for the propositional attitudes, or they are not. If they are not, then Kroy's 'mentalistic' interpretation of possible worlds is false, and he still has the epistemological problem. If they are, then physicalism is safe. There is no problem for physicalism, even as interpreted by Kroy, about sentences and pictures in the head; physicalism should welcome Kroy's reductive account. Worse, if this horn of the dilemma is grasped, then various of Kroy's claims about the relationship between determinism and physicalism (of type (4) above) are in trouble. For example, he claims that there are regularities (involving propositional attitudes) which connect 'sets of facts which obtain in different possible worlds, hence cannot be made to fit the physicalist constraint' (p. 65). 'A subject is an entity whose characterisations within a given possible world are not just determined by the facts of this possible world, but also what takes place in other possible worlds.' (p. 97) '. . . subjects can react not only to the actual but also to the possible'. (p. 245)

There are many other things in the book which I object to, but I will mention only a few here. One is the assumption that physicalism (physics) must reject possible worlds unless some reductive account is given. Following Bressan, Everett and Wheeler, this must be taken as questionable. Another is an argument for the conclusion that the workings of the imagination cannot be simulated by a Turing machine. If I understand it, the idea is that the recognition that an argument is valid depends on our ability to imagine a counterexample (a doubtful premiss, but let it go). Now any valid argument can be seen to be valid by humans. But no Turing machine can produce all the invalidities of first order logic, since it is undecidable. Hence, humans are not entirely Turing machines. However, even granting humans such remarkable validity-discerning

powers, any valid argument (of first order logic) can be seen to be valid by some (one) Turing machine too, since the set of theorems of first order logic is recursively enumerable. Kroy needs the extra premiss that any invalid argument can be seen to be invalid by the human imagination constructing counterexamples for it. But not only is this highly doubtful, but his own theory of the strictly finite workings of the imagination gives no reason to think it true. A further matter in the book to object to is the claim that because the (usual) intuitionistic propositional calculus can be mapped into S4, intuitionism is a 'disguised modal logic'. (p. 187) One might as well argue that since there is also a reverse mapping, S4 is disguised intuitionism; or that since both can be mapped to closure algebras, S4 is really topology or topology is really intuitionism.

The book is well produced and clearly written.

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