ascribing a Fregean 'depth-grammar' to the child, the variables of which get 'transformed' to the 'surface structure' of English or Japanese. He attributes skills and dispositions, not expressible knowledge. 'The child does not really know about variables . . . the lie is distinctly a white one'. (p. 124) But is this problem about Quine's empirical correctness important? I once thought so, but not now. Empiricism has long been regarded as an inadequate basis for the explanation of linguistic knowledge: witness notably the influential arguments Chomsky referred to above. Quine has rebutted these sceptical attacks on empiricism by demonstrating a way, consistent with an austere empiricism (his own), in which language could be learned. But the relevance of such a description as Quine's of how we acquire our linguistic skills, to the normative epistemological question of the justification of the theories those skills enable us to express, is harder to make out. Part of the 'challenge to natural science that arises from within natural science . . . runs as follows . . . if our science were true, how could we know it?' (p.2) Not just formulate it, but know it. Quine's description of language acquistion, as he frequently points out, in no way justifies each extension to the linguistic repertoire. To the contrary, examples I have used above show that extensions can depend on use-mention confusions, slipping expressions into syntactical positions where the equivalences by which the expressions were learned must fail, and mere transfer of conditioning. While these tricks are no bar to the understanding of the locutions thus derived, they give us no reason at all to suppose the ontological presuppositons of successive styles of locutions (bodies, determinables, individuals, sets, properties, numbers, abstractions of any order . . .) exist. Quine recognises this. He must. Near the end he says 'I asked how we developed our abstract, set-theoretic style of talk. I could ask, in the same spirit, how we developed our religious talk, . . . our talk of . . . logical modalities. . . . [W]e would find that every here and there the learner had made a little leap on the strength of analogy or conjecture of confusion; but then the same seemed to be true of our learning to talk of bodies. In short, I speculated on causes and not on values. Sheep are caused and goats are caused, and they are caused in similar ways.' (pp. 136-7) Quine summarises the position in the next paragraph: 'I have been concerned more with the nature and meaning of what [the learner] is doing than with what he or we ought to do. How then should we settle our ontology?' (p. 137)

That, the basic question of the 'new epistemology', as I described it in the first paragraph of this review (or indeed of any epistemology) is still to be answered. The answer Quine gives in the final pages is familiar: the holistic approach dating from 'Two Dogmas' in which we accommodate wayward observations by playing off simplicity against conservatism. But then, how does the stepwise story of our acquisition of language relate to this old theme? Or is it all irrelevant to this fundamental question? Quine sees each of the little leaps in the child's learning of language as a private scientific revolution, and thus to be judged favourably if it 'conduces to simplicity in the child's evolving conceptual scheme. . . . If it is a short leap, then again it is good, on the score of conservatism.' (p. 138) Why is conservatism good? Because of the 'maxim of *relative empiricism*: Don't venture farther from the sensory evidence than you need to'. (p. 138) The pragmatic joker backing up the realist trumps, saved for the last card.

So what is the net value of the book? A timely and important defence of scientific empiricism against internal scepticism, which at the same time is a reassertion of empiricist linguistic theory against the prevailing rationalist positions. And secondly a new tool for the normative epistemologist: to compare world views or theories, compare the simplicity or the shortness of the leaps involved in learning the language needed to express the theories. But I don't think this tool cuts deep, nor, given the sketchy data on how we do learn a language, is it a tool that we know how to wield. In the end Quine himself fails to wield it, even in his own defence.

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Laudan, L. *Progress and its Problems. Towards a Theory of Scientific Growth*, Berkeley and Los Angeles, University of California Press, 1977. Pp. x + 257 U.S.$1 . 5.

This book gives a very clear account of some of the most widely discussed problems in contemporary philosophy of science, and it deals lucidly with issues recently highlighted in the debate between Feyerabend, Kuhn and Lakatos concerning, for example, the rationality of scientific revolutions, the relations between the history and the philosophy of science, and the possibility of a cognitive sociology of science. The book is not, however, intended as a textbook on the subject, but rather as a contribution to the solution of these and other problems. It identifies many of the deficiencies in current theories about the methodology of science; but does the author's approach solve any of the problems which have eluded other theories?

The main thesis of the book is that the whole tradition of looking at scientific theories in terms of deductive explanations is mistaken, and needs to be replaced by a picture of science as a set of solutions to problems. Now some philosophers of science, for example Popper, have seen no conflict between the explanatory model and the problem-solving model; the idea of severe tests and degrees of corroboration in Popper's philosophy can be seen as an explication of the problem-solving power of a theory in terms of the non-*ad hoc*ness of its explanations. But according to the author, to suppose that there is a natural translation procedure between explanation and problem-solving would be to 'misconstrue the enterprise'. (p. 16) He gives some examples of situations which, he says, cannot adequately be described or dealt with in terms of the fact-explanation model. For instance, certain problems do not correspond to any actual state of affairs, as in the case, for instance, of reports about sea serpents; yet good or bad solutions to such problems can be found. This seems to me to rely on blurring a perfectly natural distinction between real and apparent problems. To say that a problem does not correspond to any fact surely means that there is no real problem there, only a putative one. The author says that 'So long as we insist that theories are designed only to explain "facts" (i.e. true statements about the world) we shall find ourselves unable to explain most of the theoretical activity which takes place in science.' (p. 16) But this is not the case. If we construe scientific activity as the attempt to solve putative problems, then we can just as easily construe it as an attempt to explain putative facts. Conversely, the author claims, there are many facts which are not problems, simply because they are unknown. Again, a distinction is being blurred here. Just as there are unknown facts there are unknown problems. Problems are not invented, they are discovered, at least in the sense that it is not up to us to decide what our problems are: they often force themselves upon the investigator, whose life would be easier without them. What is true, and what the author points out, is that many *known* facts do not constitute problems. But the reason for this is that they are facts which have already been explained by some theory. But the author wants to go further than this, since he classes under the heading of unproblematic facts those facts of which we are unaware. But why not say instead that corresponding to such facts are *problems* of which we are unaware?

The author gives another argument for the superiority of the problem-solving approach; in order to say that a fact has been explained, we must achieve an experimental outcome, a statement of which exactly matches the statement deduced from the premisses of the explanation. In other words, an explanation has to be an exact explanation. This rarely happens in experimental science. On the other hand, the argument continues, we can say that a problem has been solved when we have 'only an approximate resemblance between theoretical results and experimental ones'. (p. 23) I fail to see the force of this argument, because there is nothing to prevent us from saying that a theory *approximately* explains a fact if the experimental result is very close to the deduced result (and this is typically what we do say). The author tries to forestall this objection by saying that 'on the standard model of explanation, something either is or def   y is not an explanation—degrees of explanatory adequacy are not countenanced'. .. 24) I am not sure what 'standard model' the author has in mind here, but certainly, according to Popper's well known account of explanation, theories can be said to give better or worse explanations according to the accuracy of their predictions. And the author gives us no *argument* as to why explanations cannot be said to be better or worse according to their accuracy.

I am not suggesting that it is important to preserve the terminology of fact-explanation against that of problem-solution, but it is important to recognise that the author has not been able to replace the conceptual apparatus of the fact-explanation model with anything better. And when he gives his own definition of problem-solving, it seems that he cannot help but couch it in terms of the deductive-explanatory model. His definition amounts to saying that a theory solves a problem when a statement of that theory functions in a deductive argument, the conclusion of which is a statement of the problem. (see p. 25) But surely the statement of a problem ought, if it is to differ from the statement of a fact, at least to have interrogative form. But such cannot be the form of the conclusion of a normal deductive argument. Such a conclusion would better be described as a statement of a (putative) fact. And simply by using the notion of a deductive argument from premisses to conclusions, the author has taken over the main feature of the explanatory model without adding anything to it. I think we should conclude from this that nothing more radical than a terminological alteration has been achieved by the author's suggestions.

There are, however, more substantial objections to be brought against his definition of problem-solving, for it takes no account of the distinction between *ad hoc* and non-*ad hoc* solutions to a problem. The author is quite explicit about this; according to him there is little point in distinguishing between two theories on the grounds that the one is *ad hoc* and the other not, because they may still have the same problem-solving power. This seems to me to be a difficutly of the author's own making, brought about by his separation of problem-solving from explanation. Someone who identifies the two will say that an *ad hoc* explanation is a poor solution to a problem, and this is surely the more intuitive way to proceed. Separating the two only complicates matters here, for we now have to find other grounds for distinguishing between good and bad solutions to problems. The author adds an historical consideration to his claim that *ad hoc*ness is unimportant; that most of the historically successful theories of science have been *ad hoc*. This, it seems to me, is a misleading statement. Theories are rarely *ad hoc* in an absolute sense; they give good solutions to some problems and bad solutions to others. Even the best theories will solve some of their problems in an unsatisfactory way, or even fail to solve them at all (this has been stressed by Lakatos). But successful theories, like Newton's, are distinguished by the fact that they solve *certain* of their problems in a highly successful way. The question is not, are intuitively good scientific theories *ad hoc*?, but, do such theories contain important elements of non-*ad hoc*ness?

Another difficulty with this book is its definition of what is to count as a problem. The author says that something is not properly a problem for a theory unless it has already been solved by some other theory.

> The only reliable guide to the problems relevant to a particular theory is an examination of the problems which predecessor—and competing—theories in that domain (including the theory itself) have already solved. (p. 21, italics throughout in the original)

His argument for this conclusion is that a problem which lacks any solution is often a result of vague and ill-defined phenomena; it may be due to some extraneous and as yet unidentified factor, and it may even not be clear to which domain the phenomenon — if it should turn out to be genuine — belongs. For a long time, for instance, it was unclear whether certain effects belonged to the domain of astronomy, to physiological optics, or to the theory of the atmosphere. There is, no doubt, some truth in this, but it

does not seem to me to sustain the *general* claim that it is no deficiency in a th   if it fails to account for some effect, so long as no other theory accounts for it either. What are we to say of the many difficulties which beset Newton's theory, difficulties which its Cartesian rival could not even begin to solve, such as the details of the moon's motion? On the author's account we would have to say that such things were really not problems at all for the theory. And do we not want to say, with hindsight, that there are a geat many problems associated with some theory or other, but that these problems were not recognised by anybody at the time? In accordance with the author's definition of a problem, we could not even sensibly formulate such a statement.

A difficult problem in the philosophy of science has been that of specifying what is to count as rational behaviour. What the author says about this seems to me to leave unanswered the problems which have made the theories of rationality proposed so far unacceptable. First of all, he distinguishes between the acceptance of a theory, and pursuit of a theory. To accept a theory is, on his account 'to treat it as if it were true' (p. 108), and he says that such acceptance is rational if the theory in question has the highest capacity for problem solving among extant theories. (p. 109) There are two important objections to be levelled against this proposal. The first is that there are no obvious reasons why one should treat a theory as if it were true on the grounds that it has this problem-solving capacity. As the author is at pains to point out, problem-solving capacity is not a guide to whether or not a theory *is* true, so why should it be a guide to whether or not the theory should be treated *as if it were* true? Second, it is far from clear that the problem-solving power of a theory is something which can be determined by us and thereby made the basis of a rational choice. What we can know is how well a theory has solved its problems up until now, but this may be a wholly contingent matter, dependent upon a choice of experimental tests which artificially favour one theory rather than another, and is therefore not a fair indication of the power of either.

As to the question of when it is rational to pursue (that is, work on) a theory, the author suggests that it is rational to do so if that theory has a higher rate of progress than its rivals, even though its absolute problem-solving effectiveness is so far less than that of others in the field. Can we take it then, that behaviour other than that here specified as rational is actually irrational? It is easy for philosophers to find paradigms of rational behaviour in science; less easy for them to give a defensible characterisation of behaviour which they would condemn as irrational. There cannot be much interest in a theory of rationality according to which *all* behaviour within a given domain is rational. The author does not say whether he would count it as irrational to pursue a theory the rate of progress of which was slower than that of one of its rivals. If he did say so his position would be open to some serious and obvious objections, for what is to say that a theory with a currently slow rate of problem-solving will not speed up in the future? And if we take into account something emphasised by Feyerabend, that the existence of rivals to a theory may speed up the discovery (and indirectly the solution) of problems, then we may be inclined to say that the pursuit of some rival theory, however decrepit, is to be encouraged. For these reasons I do not think that the author's approach advances the problem very far.

It is often felt that some problems (or, I am tempted to add, some unexplained facts) are more important than others in the sense that solving them redounds more to the credit of a theory than would solving other problems. This means that, when assessing the relative merits of rival theories we cannot simply count the number of problems solved in each case. What the author says about the weighting of different problems is interesting and new. He outlines various reasons why one problem may be considered more important than another, and what he says is very useful. (p. 64) His suggestions do not amount to a method for assigning numerical values to the weights of problems—this is hardly surprising—but his definition of the problem solving power of a theory makes sense only if we assume that such assignments can be given. The ving is supposed to define an 'appraisal measure':

> . . . the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and *deducting* therefrom the number and importance of the anomalies and conceptual problems which the theory generates. (p. 68, italics throughout in the original)

Clearly this measure will assign a value to a theory only if we can quantify the importance of anomalies and problems, and it is difficult to see how this could be done.

Despite these reservations, the book is valuable for its clear statment of problems and for certain of the suggestions which it contains, particularly concerning incommensurability and the value of non-empirical problems in making methodological appraisals.

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Williams, C. J. F., *What Is Truth?* London, New York, Melbourne, Cambridge University Press, 1976, xvi, 98 pp.

In attempting to answer his title question, the author presents an analysis of 'What Percy says is true.' According to Williams, "Somewhat simplified, the thesis of this book is that propostions like 'Percy says that Mabel has measles and Mabel has measles' stand to propositions like 'What Percy says is true' in the same relation as 'Michael is coming to dinner' stands to 'Someone is coming to dinner'. In each case, the former proposition is a verifier of the latter." While developing his analysis, Williams engages in a non-Hegelian dialectic, presenting and criticising each of a series of analyses of increasing sophistication.

The first thesis, that 'What Percy says is true' means the same as (1) 'For some *p*, Percy says that *p* and *p*', is duly defended against various objections to quantifying over propostions. In the second chapter we go on to consider the question of whether truth is properly a predicate. The ascriptivist (performative) and redundancy theories fail to eliminate 'true' as a pedicate (within the realm of ordinary language), but Strawson's analysis of 'What *A* says is true' as 'Things are as *A* says they are' is said to eliminate 'true'. Of course Strawson grants the 'undisputed thesis' that someone who says that a certain statement is true thereby makes a statement about a statement, but Williams cautions us that the statement about the statement may *not* be that it is true. What '*A*'s statement, that *X* is eligible, is true' says about *A*'s statement may be only that it states that *X* is eligible. Similarly for (9) '*A*'s statement states that *X* is eligible, and *X* is eligible'. However, Williams eventually *does* conclude that the truth predicate is implicit in this conjunction. Yet (9) does say more about *A*'s statement than that it is true — it describes its content. Accordingly, in the interests of our analysis, (9) gives way to its existential generalisation, (16) 'For some *p* both *A*'s statement states that *p* and *p*'. In the interests of a more general analysis, we are given (17) 'For some *p*, both—states that *p* and *p*'. Yet (17) won't do as a definition of truth, since people state and statements state. Naturally, we don't want a definition that makes Percy himself true as well as his statement. So, we eventually come to

(25) For some *p*, for every *q*, both the proposition that *p* is the same proposition as the proposition that *q* if, and only if, Percy says *q*, and *p*.

(25) sorts Percy from his propositions, in the manner of A.N. Prior.

At this point Williams makes an extensive side trip to consider the nature of that which is true. He draws an analogy between 'What Percy says' and 'What the postman brought'. Both are incomplete symbols, which do not refer to anything in

particular. Yet, 'What the postman brought' does refer to so[illegible]ng (typically letters indirectly, and 'What the postman brought is on the mantelpiece' has particula verifiers of the form: 'So-and-so is on the mantelpiece'. 'Is on the mantelpiece' is still i the story. However, 'What Percy says is true' has as a verifier something like 'Perc says that Mabel has measles and Mabel has measles'. 'Is true' is no longer in the story Williams accepts Prior's point that 'Percy says that Mabel has measles' is about Percy Mabel, and the measles, only. 'What Percy says' has evaporated, as has 'is true', whic was there only as its linguistic complement — a pseudo predicate of a pseudo subjec which is lost in analysis. The Prior-Williams argument can be debated, particularly a employed here. 'Percy says that Mabel has measles' might be taken to be about th sentence or proposition '(that) Mabel has measles', particularly if propositions ar construed as sentences as employed. Such a tack might well be taken in defence of a Austinean theory. It would be inappropriate to debate the point further here, but would not have been inappropriate for Williams to explore it further.

Williams notes that (25) is open to attack on the grounds that if Percy says nothin at all, we would have to conclude that 'What Percy says is false' would be true, sinc 'What Percy says is true' would not be— assuming that truth and falsity ar contradictories. It is then decided that the analysis must *presuppose* rather than jus state that Percy says some (one) thing. Truth and falsity are contraries—an contradictories on the basis of the presupposition. Fair enough, but that seems lik rather a scanty pay-load for one of the book's five chapters. (To my mind, the mos interesting part of the chapter was a two page side-discussion of the proper reading o Aristotle's analysis of truth.) We are given the final analysis of 'What Percy says i true' as (30) '$\vdash_2 \Sigma p \Pi q E I p q J q$, $\Pi r C J r r$' where '$\vdash_2$' is used to indicate that of th following two statements, both are asserted and the second presupposes the first, an where *Jp* means Percy says that *p*. This comes to 'For some *p*, for every *q*, th proposition that *p* is the same proposition that *q* if, and only if, Percy says *q* [th preceding presupposed], and, *p*'.

In the final chapter, Williams considers the bearing of his analysis on th correspondence theory of truth. According to his conception, 'For some *x*, *x* is a fac and Percy's statement corresponds to *x*' is to be understood as 'Things are as Percy' statement says they are', or (30). The relationship of correspondence is analysed i terms of truth — rather than vice versa — and is seen to disappear in analysis. In fac as readers of recent controversies will recall, Williams denies that truth is a relationa predicate at all. I applaud his desire to weed ontologically bogus entities such as fact and propositions out of truth theory. I believe, though, that he has not done justice t the basic intuition that truth is a matter of the relationship between words and worl There are a number of points where I would question his argument, but I will conten myself with just one objection. Williams takes up the argument that a correspondenc theory cannot accommodate negative facts. "The facts that 'Toby sighed' fails t correspond to when it is false is like", we are told, "the 'someone' whom a woman fail to be married to if she is a spinster." I can imagine a (neo?) Austinean arguing tha 'Toby sighed' being false is a case of the referent *not* being as described under th descriptive conventions, and that the statement is false by virtue of as specific worldly correlate as would have made it true in the contrary case. Of course this woul take a discussion of what is, directly or indirectly, the worldly correlate. Perhap Williams could establish his case here, but here, as elsewhere in the final chapter, hi arguments need strengthening, at best. At worst, I believe them misguided.

In spite of its engagingly general title, I could not recommend the book to th student seeking a general introduction to truth theory, though it would be of som value for a person doing advanced work in the area. As one would expect of th former editor of *Analysis*, Williams is a highly skilled analytical tactician. In terms o strategy, however, his offering leaves something to be desired. This is true in th expository sense that he often leaves one to wonder just what he is *trying* to do. Thi ascribing a Fregean 'depth-grammar' to the child, the variables of which get 'transformed' to the 'surface structure' of English or Japanese. He attributes skills and dispositions, not expressible knowledge. 'The child does not really know about variables . . . the lie is distinctly a white one'. (p. 124) But is this problem about Quine's empirical correctness important? I once thought so, but not now. Empiricism has long been regarded as an inadequate basis for the explanation of linguistic knowledge: witness notably the influential arguments Chomsky referred to above. Quine has rebutted these sceptical attacks on empiricism by demonstrating a way, consistent with an austere empiricism (his own), in which language could be learned. But the relevance of such a description as Quine's of how we acquire our linguistic skills, to the normative epistemological question of the justification of the theories those skills enable us to express, is harder to make out. Part of the 'challenge to natural science that arises from within natural science . . . runs as follows . . . if our science were true, how could we know it?' (p.2) Not just formulate it, but know it. Quine's description of language acquistion, as he frequently points out, in no way justifies each extension to the linguistic repertoire. To the contrary, examples I have used above show that extensions can depend on use-mention confusions, slipping expressions into syntactical positions where the equivalences by which the expressions were learned must fail, and mere transfer of conditioning. While these tricks are no bar to the understanding of the locutions thus derived, they give us no reason at all to suppose the ontological presuppositons of successive styles of locutions (bodies, determinables, individuals, sets, properties, numbers, abstractions of any order . . .) exist. Quine recognises this. He must. Near the end he says 'I asked how we developed our abstract, set-theoretic style of talk. I could ask, in the same spirit, how we developed our religious talk, . . . our talk of . . . logical modalities. . . . [W]e would find that every here and there the learner had made a little leap on the strength of analogy or conjecture of confusion; but then the same seemed to be true of our learning to talk of bodies. In short, I speculated on causes and not on values. Sheep are caused and goats are caused, and they are caused in similar ways.' (pp. 136-7) Quine summarises the position in the next paragraph: 'I have been concerned more with the nature and meaning of what [the learner] is doing than with what he or we ought to do. How then should we settle our ontology?' (p. 137)

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Thomas J. Richards *La Trobe University*

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The author gives another argument for the superiority of the problem-solving approach; in order to say that a fact has been explained, we must achieve an experimental outcome, a statement of which exactly matches the statement deduced from the premisses of the explanation. In other words, an explanation has to be an exact explanation. This rarely happens in experimental science. On the other hand, the argument continues, we can say that a problem has been solved when we have 'only an approximate resemblance between theoretical results and experimental ones'. (p. 23) I fail to see the force of this argument, because there is nothing to prevent us from saying that a theory *approximately* explains a fact if the experimental result is very close to the deduced result (and this is typically what we do say). The author tries to forestall this objection by saying that 'on the standard model of explanation, something either is or de   ly is not an explanation—degrees of explanatory adequacy are not countenanced'. . . 24) I am not sure what 'standard model' the author has in mind here, but certainly, according to Popper's well known account of explanation, theories can be said to give better or worse explanations according to the accuracy of their predictions. And the author gives us no *argument* as to why explanations cannot be said to be better or worse according to their accuracy.

I am not suggesting that it is important to preserve the terminology of fact-explanation against that of problem-solution, but it is important to recognise that the author has not been able to replace the conceptual apparatus of the fact-explanation model with anything better. And when he gives his own definition of problem-solving, it seems that he cannot help but couch it in terms of the deductive-explanatory model. His definition amounts to saying that a theory solves a problem when a statement of that theory functions in a deductive argument, the conclusion of which is a statement of the problem. (see p. 25) But surely the statement of a problem ought, if it is to differ from the statement of a fact, at least to have interrogative form. But such cannot be the form of the conclusion of a normal deductive argument. Such a conclusion would better be described as a statement of a (putative) fact. And simply by using the notion of a deductive argument from premisses to conclusions, the author has taken over the main feature of the explanatory model without adding anything to it. I think we should conclude from this that nothing more radical than a terminological alteration has been achieved by the author's suggestions. ?

There are, however, more substantial objections to be brought against his definition of problem-solving, for it takes no account of the distinction between *ad hoc* and non-*ad hoc* solutions to a problem. The author is quite explicit about this; according to him there is little point in distinguishing between two theories on the grounds that the one is *ad hoc* and the other not, because they may still have the same problem-solving power. This seems to me to be a difficulty of the author's own making, brought about by his separation of problem-solving from explanation. Someone who identifies the two will say that an *ad hoc* explanation is a poor solution to a problem, and this is surely the more intuitive way to proceed. Separating the two only complicates matters here, for we now have to find other grounds for distinguishing between good and bad solutions to problems. The author adds an historical consideration to his claim that *ad hoc*ness is unimportant; that most of the historically successful theories of science have been *ad hoc*. This, it seems to me, is a misleading statement. Theories are rarely *ad hoc* in an absolute sense; they give good solutions to some problems and bad solutions to others. Even the best theories will solve some of their problems in an unsatisfactory way, or even fail to solve them at all (this has been stressed by Lakatos). But successful theories, like Newton's, are distinguished by the fact that they solve *certain* of their problems in a highly successful way. The question is not, are intuitively good scientific theories *ad hoc*?, but, do such theories contain important elements of non-*ad hoc*ness?

Another difficulty with this book is its definition of what is to count as a problem. The author says that something is not properly a problem for a theory unless it has already been solved by some other theory.

> The only reliable guide to the problems relevant to a particular theory is an examination of the problems which predecessor—and competing—theories in that domain (including the theory itself) have already solved. (p. 21, italics throughout in the original)

His argument for this conclusion is that a problem which lacks any solution is often a result of vague and ill-defined phenomena; it may be due to some extraneous and as yet unidentified factor, and it may even not be clear to which domain the phenomenon — if it should turn out to be genuine — belongs. For a long time, for instance, it was unclear whether certain effects belonged to the domain of astronomy, to physiological optics, or to the theory of the atmosphere. There is, no doubt, some truth in this, but it

oes not seem to me to sustain the *general* claim that it is no deficiency in a th   f it fails to account for some effect, so long as no other theory accounts for it eithe. . hat are we to say of the many difficulties which beset Newton's theory, difficulties which its Cartesian rival could not even begin to solve, such as the details of the moon's motion? On the author's account we would have to say that such things were really not problems at all for the theory. And do we not want to say, with hindsight, that there are a geat many problems associated with some theory or other, but that these problems were not recognised by anybody at the time? In accordance with the author's definition of a problem, we could not even sensibly formulate such a statement.

A difficult problem in the philosophy of science has been that of specifying what is to count as rational behaviour. What the author says about this seems to me to leave unanswered the problems which have made the theories of rationality proposed so far unacceptable. First of all, he distinguishes between the acceptance of a theory, and pursuit of a theory. To accept a theory is, on his account 'to treat it as if it were true' (p. 108), and he says that such acceptance is rational if the theory in question has the highest capacity for problem solving among extant theories. (p. 109) There are two important objections to be levelled against this proposal. The first is that there are no obvious reasons why one should treat a theory as if it were true on the grounds that it has this problem-solving capacity. As the author is at pains to point out, problem-solving capacity is not a guide to whether or not a theory *is* true, so why should it be a guide to whether or not the theory should be treated *as if it were* true? Second, it is far from clear that the problem-solving power of a theory is something which can be determined by us and thereby made the basis of a rational choice. What we can know is how well a theory has solved its problems up until now, but this may be a wholly contingent matter, dependent upon a choice of experimental tests which artificially favour one theory rather than another, and is therefore not a fair indication of the power of either.

As to the question of when it is rational to pursue (that is, work on) a theory, the author suggests that it is rational to do so if that theory has a higher rate of progress than its rivals, even though its absolute problem-solving effectiveness is so far less than that of others in the field. Can we take it then, that behaviour other than that here specified as rational is actually irrational? It is easy for philosophers to find paradigms of rational behaviour in science; less easy for them to give a defensible characterisation of behaviour which they would condemn as irrational. There cannot be much interest in a theory of rationality according to which *all* behaviour within a given domain is rational. The author does not say whether he would count it as irrational to pursue a theory the rate of progress of which was slower than that of one of its rivals. If he did say so his position would be open to some serious and obvious objections, for what is to say that a theory with a currently slow rate of problem-solving will not speed up in the future? And if we take into account something emphasised by Feyerabend, that the existence of rivals to a theory may speed up the discovery (and indirectly the solution) of problems, then we may be inclined to say that the pursuit of some rival theory, however decrepit, is to be encouraged. For these reasons I do not think that the author's approach advances the problem very far.

It is often felt that some problems (or, I am tempted to add, some unexplained facts) are more important than others in the sense that solving them redounds more to the credit of a theory than would solving other problems. This means that, when assessing the relative merits of rival theories we cannot simply count the number of problems solved in each case. What the author says about the weighting of different problems is interesting and new. He outlines various reasons why one problem may be considered more important than another, and what he says is very useful. (p. 64) His suggestions do not amount to a method for assigning numerical values to the weights of problems—this is hardly surprising—but his definition of the problem solving power of a theory makes sense only if we assume that such assignments can be given. The king is supposed to define an 'appraisal measure':

> . . . the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and *deducting* therefrom the number and importance of the anomalies and conceptual problems which the theory generates. (p. 68, italics throughout in the original)

Clearly this measure will assign a value to a theory only if we can quantify the importance of anomalies and problems, and it is difficult to see how this could be done.

Despite these reservations, the book is valuable for its clear statment of problems and for certain of the suggestions which it contains, particularly concerning incommensurability and the value of non-empirical problems in making methodological appraisals.

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Williams, C. J. F., *What Is Truth*? London, New York, Melbourne, Cambridge University Press, 1976, xvi, 98 pp.

In attempting to answer his title question, the author presents an analysis of 'What Percy says is true.' According to Williams, "Somewhat simplified, the thesis of this book is that propostions like 'Percy says that Mabel has measles and Mabel has measles' stand to propositions like 'What Percy says is true' in the same relation as 'Michael is coming to dinner' stands to 'Someone is coming to dinner'. In each case, the former proposition is a verifier of the latter." While developing his analysis, Williams engages in a non-Hegelian dialectic, presenting and criticising each of a series of analyses of increasing sophistication.

The first thesis, that 'What Percy says is true' means the same as (1) 'For some *p*, Percy says that *p* and *p*', is duly defended against various objections to quantifying over propostions. In the second chapter we go on to consider the question of whether truth is properly a predicate. The ascriptivist (performative) and redundancy theories fail to eliminate 'true' as a pedicate (within the realm of ordinary language), but Strawson's analysis of 'What *A* says is true' as 'Things are as *A* says they are' is said to eliminate 'true'. Of course Strawson grants the 'undisputed thesis' that someone who says that a certain statement is true thereby makes a statement about a statement, but Williams cautions us that the statement about the statement may *not* be that it is true. What '*A*'s statement, that *X* is eligible, is true' says about *A*'s statement may be only that it states that *X* is eligible. Similarly for (9) '*A*'s statement states that *X* is eligible, and *X* is eligible'. However, Williams eventually *does* conclude that the truth predicate is implicit in this conjunction. Yet (9) does say more about *A*'s statement than that it is true — it describes its content. Accordingly, in the interests of our analysis, (9) gives way to its existential generalisation, (16) 'For some *p* both *A*'s statement states that *p* and *p*'. In the interests of a more general analysis, we are given (17) 'For some *p*, both—states that *p* and *p*'. Yet (17) won't do as a definition of truth, since people state and statements state. Naturally, we don't want a definition that makes Percy himself true as well as his statement. So, we eventually come to

(25) For some *p*, for every *q*, both the proposition that *p* is the same proposition as the proposition that *q* if, and only if, Percy says *q*, and *p*.

(25) sorts Percy from his propositions, in the manner of A.N. Prior.

At this point Williams makes an extensive side trip to consider the nature of that which is true. He draws an analogy between 'What Percy says' and 'What the postman brought'. Both are incomplete symbols, which do not refer to anything in

particular. Yet, 'What the postman brought' does refer to so ... hing (typically lett indirectly, and 'What the postman brought is on the mantelpiece' has partic verifiers of the form: 'So-and-so is on the mantelpiece'. 'Is on the mantelpiece' is stil the story. However, 'What Percy says is true' has as a verifier something like 'Pe says that Mabel has measles and Mabel has measles'. 'Is true' is no longer in the st Williams accepts Prior's point that 'Percy says that Mabel has measles' is about Pe Mabel, and the measles, only. 'What Percy says' has evaporated, as has 'is true', wh was there only as its linguistic complement — a pseudo predicate of a pseudo subj which is lost in analysis. The Prior-Williams argument can be debated, particularl employed here. 'Percy says that Mabel has measles' might be taken to be about sentence or proposition '(that) Mabel has measles', particularly if propositions construed as sentences as employed. Such a tack might well be taken in defence of Austinean theory. It would be inappropriate to debate the point further here, bu would not have been inappropriate for Williams to explore it further.

Williams notes that (25) is open to attack on the grounds that if Percy says noth at all, we would have to conclude that 'What Percy says is false' would be true, si 'What Percy says is true' would not be— assuming that truth and falsity contradictories. It is then decided that the analysis must *presuppose* rather than j state that Percy says some (one) thing. Truth and falsity are contraries—a contradictories on the basis of the presupposition. Fair enough, but that seems rather a scanty pay-load for one of the book's five chapters. (To my mind, the m interesting part of the chapter was a two page side-discussion of the proper reading Aristotle's analysis of truth.) We are given the final analysis of 'What Percy says true' as (30) '$\vdash_2 \Sigma p \Pi q E I p q J q$, $\Pi r C J r r$' where '$\vdash_2$' is used to indicate that of following two statements, both are asserted and the second presupposes the first, a where *Jp* means Percy says that *p*. This comes to 'For some *p*, for every *q*, proposition that *p* is the same proposition that *q* if, and only if, Percy says *q* [t preceding presupposed], and, *p*'.

In the final chapter, Williams considers the bearing of his analysis on correspondence theory of truth. According to his conception, 'For some *x*, *x* is a f and Percy's statement corresponds to *x*' is to be understood as 'Things are as Perc statement says they are', or (30). The relationship of correspondence is analysed terms of truth — rather than vice versa — and is seen to disappear in analysis. In f as readers of recent controversies will recall, Williams denies that truth is a relatio predicate at all. I applaud his desire to weed ontologically bogus entities such as f and propositions out of truth theory. I believe, though, that he has not done justice the basic intuition that truth is a matter of the relationship between words and wor There are a number of points where I would question his argument, but I will cont myself with just one objection. Williams takes up the argument that a corresponder theory cannot accommodate negative facts. "The facts that 'Toby sighed' fails correspond to when it is false is like", we are told, "the 'someone' whom a woman f to be married to if she is a spinster." I can imagine a (neo?) Austinean arguing t 'Toby sighed' being false is a case of the referent *not* being as described under descriptive conventions, and that the statement is false by virtue of as specifi worldly correlate as would have made it true in the contrary case. Of course this wo take a discussion of what is, directly or indirectly, the worldly correlate. Perh Williams could establish his case here, but here, as elsewhere in the final chapter, arguments need strengthening, at best. At worst, I believe them misguided.

In spite of its engagingly general title, I could not recommend the book to student seeking a general introduction to truth theory, though it would be of so value for a person doing advanced work in the area. As one would expect of former editor of *Analysis*, Williams is a highly skilled analytical tactician. In terms strategy, however, his offering leaves something to be desired. This is true in expository sense that he often leaves one to wonder just what he is *trying* to do. Th