Truth. By ALAN R. WHITE. (Garden City, N.Y.: Doubleday, 1970. London: Macmillan, 1971. Pp. ix + 150. Pa. $1.45; Hardcover £1.95.)

This is an excellent introduction to the philosophy of truth. It covers a lot of ground economically, with clear critical accounts of different theories of truth. The author's own views are generally interesting and sometimes provoking. His judgment is not always, in my opinion, as impartial as it might be. There is a tendency to give nothing but the arguments for or the arguments against a view. I think he is too polemical against the Fregeans and does not present Strawson's description of ' the ' at all plausibly. His style is in the main clear and emphatic, though he begins with, and once or twice lapses into, Moore-like pedantry. The principal discussion of truth, fact, presupposition and certain kinds of truth is well done. The case for future and moral truths is in each instance persuasive, the former being coupled with a very lucid analysis of reasons for denying that statements about the future can have truth-value.

Professor White's view is that truth lies in one kind of saying, and that is saying how things are. It is not just the utterance of a certain kind of sentence, which, depending on the sentence and the circumstances, may embody what is said. What is said, some statement or proposition, is not the saying of it. It must further be distinguished from that whose content it is, e.g., an assumption, objection or fear. I want to agree, but feel that this view faces two difficulties. One is that something may not have been said and may yet have a truth-value. The other is that a proposition may be part of the content of what is said without itself being said. Whether anyone has said that the Scott Monument weighs over two thousand tons, it is true or false that it does weigh more than this amount. This truth or falsehood is independent of what is actually said. Secondly, in saying that I fear that some train will be late, I am not saying that some train will be late. But I ought to be, if I am expressing the proposition that some train will be late and if a proposition is what is actually said. It would seem preferable, therefore, to identify what is true or false with what *can be* said, rather than with what is said. It can be, and may never be, said that the Scott Monument weighs over two thousand tons. The proposition that some train will be late is the content of the fear that some train will be late, because it can be said, though it is not here said, that some train will be late.

Could something be said before the existence of language? If not, was anything then true? White can deal with this problem, as his consideration of time and truth clearly shows. He argues for the timelessness of truth on the ground that ' True at t_n that p ' is equivalent to ' True that at t_n p ' e.g., ' It soon will be true that naval battles take place only on Sundays ' is equivalent to ' It is true that naval battles soon will take place only on Sundays '. ' It was true before the existence of language that man had tools ' thus becomes ' It is true that before the existence of language man had tools '.

Connected with time is the question of different utterances embodying the same proposition. I did not find satisfaction on this topic, as no decision was given on how reference affects what is said. Two, dissimilar, examples are given, of referring to a subject in different ways, while saying the same thing of it. 1 (*a*) My father is ill; (*b*) My husband is ill. 2 (*a*) I am rich; (*b*) You are rich; (*c*) He is rich.

As the author says, in one sense the speakers have said the same thing. They have said the same *of* someone. But, while admitting that, in another sense, statements like 1 (*a*) and 1 (*b*) say different things, White offers no explanation or theory. I would suggest that if two speakers refer to the same thing in terms whose meaning is either identical, or, where different, the difference being explained by either change in time or by transposing ' I ' for ' you ', difference of meaning is irrelevant to saying the same. This would allow only 2 (*a*) and 2 (*b*) to say the same. The general reason for this is that differences in meaning may be purely referential and not descriptive. ' I am rich ' and ' You are rich ' are different in meaning, but the difference is referential and not descriptive. The difference between ' he ' and ' I ' is both referential and descriptive. In using ' he ' the speaker implies masculinity; one can even use it predicatively.

But it would hardly be reasonable to expect the author to solve all the problems he successfully interests us in.

V. HOPE

The Conceptual Foundations of Contemporary Relativity Theory. By JOHN COWPERTHWAITE GRAVES. (Cambridge, Mass.: M.I.T. Press. 1971. Pp. xiii + 361. Price $15.00.)

The dominant scientific picture of space and matter has been a dualistic one. Newton saw space as a container for matter; recent science has modified this picture but has not changed its essence, for although we must now speak of space-time instead of space and time, and of mass-energy instead of mass and energy, space-time remains an arena for various kinds of processes involving mass-energy. Some philosophers have sought a unification by way of reduction, the direction of the reduction being dictated by a general philosophical prejudice towards absolutist conceptions of space. Thus, Leibniz sought to construct a relational theory of space in which space is nothing over and above the mutual relations among bodies ; and the Leibnizian spirit is still very much alive in contemporary philosophy (cf. the writing of Adolf Grünbaum and B. C. van Fraassen). It is my opinion that this approach faces serious if not insurmountable difficulties of both a philosophical and physical nature ; but this opinion is admittedly a controversial one. Geometro-dynamics (GMD) also tries to affect a unification by running the reduction the other way ; the vision of GMD is of space-time not as an arena but as everything. As far as I know, *The Conceptual Foundations of Contemporary Relativity Theory* is the first published attempt to provide a comprehensive account of the philosophically significant aspects of GMD. But this book is much more than an account of a particular scientific theory, for it contains substantial contributions to the philosophy of scientific methodology and the history of science. Moreover, one witnesses a phenomenon all too rare in contemporary philosophy of science : the application of a systematic philosophy of scientific methodology to actual science as opposed to cooked-up examples drawn from popularized accounts of science.

The book falls into four major parts. The first part contains a defence of scientific realism, the doctrine that scientific theories have ontological import, that evidence for a theory should be taken as evidence that the entities and properties to which the terms of the theory purport to refer do exist. The second part deals with the views on space and time of Plato, Descartes, and Newton ; the author argues that the former two figures can be regarded as precursors of GMD. The third part surveys classical general relativity theory, and the fourth part contains an account of GMD proper. Since the general reader is apt to be unfamiliar with the material of this fourth and most important section, I will concentrate my remarks on it.

Graves's account of GMD is restricted to the classical version. His treatment of it is fair in the sense that he mentions the difficulties which confront it, but I feel that he does not appreciate just how serious these difficulties are ; in particular, he tends to leave the reader with the impression that these difficulties are ones which further research may overcome, but in some cases I think it is clear that the difficulties can be overcome only by drastically revising the programme of classical GMD. Let us begin with the " already unified " theory of gravitation and electromagnetism ; this case provides one of the most dramatic successes of classical GMD, but it also embodies one of its greatest failings. The basic idea here is that the (source-free) electromagnetic field may leave such a unique imprint on the geometry of space-time that, given the four-geometry, we may be able to recover the electromagnetic field. In more detail, we may ask which conditions the four-geometry must satisfy in order that the Einstein-Maxwell equations hold. The answer was first provided by Ranich and then rediscovered by Misner and Wheeler. The second question is whether these conditions are sufficient to permit us uniquely to determine from the four-geometry the electromagnetic field ; if the answer is yes, we can claim to have geometricized the source-free electromagnetic field. And the answer is yes, except perhaps in the case when the electromagnetic field is null (classically, $\mathbf{E}.\mathbf{H}=0$, $\mathbf{E}^2-\mathbf{H}^2=0$). Graves is somewhat vague on what this " perhaps " holds. In a paper by R. Geroch (*Ann. Phys.*, 1966) to which Graves does not refer, it is shown that, except for a special set of solutions to the Einstein-Maxwell equations where the null region is four-dimensional, the electromagnetic field does follow uniquely from the four-geometry ; the exceptional cases are displayed explicitly. If one wants to hold on to the view that space-time is everything, one can argue that the arbitrariness in the electromagnetic field which occurs in these special null regions can just be forgotten. For in the first place, we cannot measure the electromagnetic field in these null regions by bringing in test charges without destroying the null character of the region ; and in the second place, this arbitrariness does not affect any physical measurements which can be expressed in purely geometrical terms. However, this defence of GMD succeeds only by changing the rules of the game ; of course, there may be independent reasons for changing the game, but until they are supplied, the defence will seem an *ad hoc* face-saving manoeuvre. Also, there is the fact that the treatment of null regions which can be fully geometricized is not only mathematically very complicated, but much different from that of the non-null regions, so much so that one is left with the feeling, as Geroch puts it, that this is not how Nature operates.

Null regions also make trouble for the initial value problem in GMD, as pointed out by R. Penrose. Suppose we are given a time slice on which two non-null regions are separated by a null region. From geometrical measurements made on or in a sufficiently small neighbourhood of this slice it will not be possible to determine the relative " complexion " of the non-null regions ; but if at some later time these regions do merge, the relative complexion must be known if the state of the overlap is to be predicted. Graves reports Wheeler's suggestion that this difficulty shows that if geometry is everything, then the concept of geometry must be taken to include global properties in addition to those of local differential geometry. This suggestion evades rather than meets the main point. Even before the Penrose problem was raised, there was no hope of vindicating the slogan " Geometry is all " without appealing to global geometric concepts. What the Penrose problem does is to strike at the heart of the plan of GMD, which is to determine the four-geometry from initial data on some time slice (or on two adjacent slices) ; for this plan would not hold much interest unless the data are such as can be ascertained by local measurements.

So far we have been considering the source-free case. In order to treat the general case without admitting non-geometrical elements, Wheeler introduces multiple connectedness in the form of " wormholes." As Graves points out, wormholes raise the spectre of acausality ; but it seems to me that Graves's discussion, which is based on an old paper by Wheeler, is muddled. Graves is worried that wormhole identifications will lead to a violation of the postulate that the velocity of a causal signal cannot exceed the velocity of light ; if this is taken to mean that no causal signal can propagate outside the light cone, then if it is satisfied before wormhole identifications, it will be satisfied after. The sort of acausality to which wormhole identifications can give rise comes in another form ; namely closed timelike or null curves. In the case of the Schwarzschild space-time, the type of wormhole identification which Wheeler proposes does not give rise to closed causal loops ; but in the case of other space-times, wormhole-type identifications do lead to closed causal loops. Wormholes can also give rise to this type of acausality in another way. The pinching off of a wormhole handle and the coalescence or fission of wormhole mouths mean a change in topology ; but in a spatially closed universe, a change in topology can take place only at the expense of closed timelike curves. And in some versions of GMD, Wheeler imposes the condition of spatial closure. (He sees this condition as an expression of " Mach's Principle " ; this is an interesting matter, but one I cannot discuss here.)

Acausality in the form of closed timelike curves poses a problem for the plan of GMD ; for the existence of such curves imposes strong consistency conditions on the equations of motion, and what these conditions are cannot be specified without an advance knowledge of the global structure of space-time. And the existence of such curves can make it unreasonable to expect, contrary to Graves, that space-time can always be broken up into time slices ; for if such a curve is homotopic to zero, no global time slice can intersect it.

Even in the absence of acausal features, classical Laplacian determinism and the plan of GMD can fail in another way : the space-time can fail to possess a Cauchy surface. (A Cauchy surface is a time slice S whose domain of dependence is the whole space-time. The domain of dependence of S is the union of the future and past domains of dependence of S, the former (latter) consisting of all those points p such that every past (future) directed timelike curve which passes through p and which does not have a past (future) endpoint intersects S.) If the slice S is not a Cauchy surface, it can happen that no amount of instantaneous data on S will suffice to determine the whole history of the universe. Several examples of physically interesting space-times without Cauchy surfaces are known.

Because of the above-mentioned difficulties and because there does not appear to be any satisfactory way of treating fields other than the electromagnetic field within classical GMD, it seems fair to say that there is little hope that classical GMD can bring about the sought after unification ; at the classical level, we seem to be stuck with the dualism mentioned at the beginning. In apparent agreement with this judgment, Wheeler has in recent years turned his attention toward quantum GMD. About the prospects and problems of quantum GMD, Graves's book has almost nothing to say ; but this is not a shortcoming, for it is too early to make any balanced assessment on this matter. When the time comes, I hope Graves will write a second volume on quantum GMD which is as informative and stimulating as the present volume.

JOHN EARMAN

Law and Explanation : An Essay in the Philosophy of Science. By PETER ACHINSTEIN. (Oxford : Clarendon Press : Oxford University Press. 1971. Pp. xii + 168. Price £2.75.)

This book examines the status of laws and explanations in science ; in particular, what it means to say that some proposition is a scientific law, what it means to explain an event, whether laws or regularities are essential parts of any scientific explanation,