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LOGIC OF PARADOX REVISITED

1. INTRODUCTION

If all the papers that have been written on the logical paradoxes since Russell's famous letter to Frege were laid end to end, they would stretch from Cambridge to Jena. And then some. Somewhere along this track would be a paper of mine, 'Logic of Paradox'.[1] In it I argued that the most satisfactory account of the paradoxes is to view them as what they appear, *prima facie*, to be, true contradictions, i.e., sentences such that both they and their negations are true.[2] The papers by Chihara, Dowden, and Woodruff[3] in this volume, which bear in one way or another on this idea, provide a welcome occasion to reconsider the paper and its main idea. I am under no illusion that this will bring the paper trail to an end. However, I hope it will advance the debate further, if only by preventing unnecessary meanderings.

That there are true contradictions will seem to many a radical proposal. However, those reasonably familiar with the history of philosophy will know that it is not a novel one.[4] The logical paradoxes are not the only considerations which can be adduced in favour of this position. For example, analyses of motion, of inconsistent legal corpuses, and of multi-criterial terms have also been known to issue in this conclusion.[5] However, the logical paradoxes provide perhaps the most convincing and novel argument to this end.

The reasons for supposing the logical paradoxes to be true contradictions are at least two-fold. The major reason is that all attempts to treat them as anything else have been singularly unsuccessful, or at any rate a good deal less successful than the present proposal. Nor is this merely an inductive argument. For there are substantial reasons why no consistent approach to the paradoxes can work.[6] One of these concerns paradoxes of the extended variety, which I will discuss further below. A second reason for supposing the paradoxes to be true contradictions can be derived from considerations concerning Gödel's first incompleteness theorem. This

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argument I gave in *LP* and Chihara criticises it in his article. I will discuss this too in detail below.

An important and obvious corollary to the approach to the paradoxes that I advocate is that the received formal logical theory must be changed. Showing that it is possible to do this in a satisfactory way was an important aim of *LP*. For if this cannot be done, the proposal is obviously not workable. However, it is important to notice that the received theory is not blindly destroyed. As in so many scientific changes, the received theory can be seen as, in a certain sense, a special case of the more novel theory. Classical logic is correct provided we are in the restricted domain of consistent situations. It is, therefore, to use some Hegelean jargon, *aufgehoben.*[7]

None the less, important and even central parts of the received view of logic must be acknowledged to be wrong: crucially, the law of non-contradiction. One must be careful here, however, about what exactly this means. What it does not mean is that the formula $\neg(A \wedge \neg A)$ is not logically valid (i.e., true in all interpretations). Indeed this formula is valid in the semantics of *LP*. What it does mean is that we must come to accept some formulas of the form $A \wedge \neg A$, since some of these are indeed true.

In view of the fact that many would take this to be absurd, it is important to ask what *arguments* can be brought against it. For the truth is that, by and large, philosophers have done nothing but *assume*, usually dogmatically, the law of non-contradiction (in the sense in question).[8] Arguments are, in fact, much harder to come by than most have assumed, and cogent arguments seemingly impossible. Most of the arguments there are derive, in effect, from Aristotle's *Metaphysics* book Γ. These were already convincingly destroyed by Lukaciewicz in 1910, and little has been done in the succeeding 70 years to improve on them.[9] It is the law of contradiction, therefore, and not the positions which challenge it, which are founded on sand.[10]

2. ON THE CONSTRUCTION OF SEMANTICALLY CLOSED THEORIES

With these preliminary remarks out of the way, we can now turn to some of the issues raised by Dowden, Chihara and Woodruff.

It is conventionally done to divide the logical paradoxes into the set theoretic ones and the semantic ones. The basis for this division is the vocabulary used in the paradoxical sentence. One might well question whether this is a significant distinction, especially since in modern model theory, semantic notions such as truth and satisfaction are defined in set theoretic terms.[11] However, let us accept this dichotomy for present purposes. Corresponding to this division, the approach to the paradoxes that I advocate has distinctive applications to set theory and semantics. First set theory: The naive, and correct, conception of set is that of a set as the extension of an *arbitrary* property. Thus, the naive abstraction axiom $\exists x\forall y(y \in x \leftrightarrow \phi)$ will be valid (possibly with restrictions on the occurrence of x in ϕ) and also the extensionality axiom. The theory will be inconsistent; indeed the whole point of this approach to the paradoxes is that some of our conceptions *are* inconsistent. However, it had better not be trivial, since trivial concepts are pointless. This form of set theory has been explored by Routley, and its non-triviality has been shown by Brady.[12]

Turning to semantics, the idea is this: a natural language, such as English, appears to be (and in fact is) a language which can discuss its own semantics, and, in particular, give an account of the truth of its own sentences. We need a theoretical understanding of how this is possible. To this end we need to construct semantically closed formal theories which "model" the phenomenon. What account of truth as such is given, may be a matter of dispute. However, any account should satisfy Tarski's adequacy condition, i.e., all instances of the T-scheme, $T\ulcorner\phi\urcorner \leftrightarrow \phi$, should be provable. For this does indeed characterise, at least in a weak sense, our naive notion of truth. (The characterisation is at least an extensional characterization. Whether there is more to a complete characterisation, one can dispute. I think that formally, there is not.) The theory involved will be inconsistent but, one would hope, non-trivial. In *LP* §II.5 I indicated what such a theory might be like, but this was only an outline and I did not raise the problem of proving non-triviality.

The papers by Dowden and Woodruff[13] provide interesting model theoretic constructions for producing semantically closed theories – or at least first approximations thereto. Any interpretation, J, in which the truth predicate is (in Woodruff's terms) perfect and complete (such as $\cup S$) will verify the T-scheme, at least in the form of a material equivalence. Dowden produces a particular such example. Elsewhere[14] I have given a somewhat different approach to the problem, which is axiomatic, rather than model theoretic, and which uses a relevant extension of the logic of *LP* rather than that of *LP* itself. The constructions used by Dowden and Woodruff are obviously elegant. One of the advantages of the model theoretic approach is that *Th*(*J*) comes complete with non-triviality proof, even where *Th*(*J*) contains first order arithmetic. (My approach does not.) On the negative side one might point to the fact that we have in general no axiomatic control over such theories. This is not much of a problem, for we can always take a suitable axiomatic sub-theory if we need one. Perhaps a bigger problem is that in the theory the *T*-scheme, $T\ulcorner\phi\urcorner \leftrightarrow \phi$, is formulated in terms of a material equivalence. This means that in proof theoretical terms the use of the *T*-scheme is too limited. For because of the invalidity of the disjunctive syllogism, it is impossible to infer the truth of ϕ from that of $T\ulcorner\phi\urcorner$ (and vice versa), which is frequently necessary in ordinary reasoning concerning truth. (In the approach I have given the *T*-scheme is a relevant one, and hence these inferences are valid.) In virtue of the close connection between truth value gaps and gluts and relevant logic, it may be possible to use Woodruff's construction for a language which contains a (relevant) implication operator – at least a first degree one – and hence produce interpretations which verify a relevant *T*-scheme. This would certainly be a welcome extension.

However, I think the main drawback of Dowden and Woodruff's constructions is the fact that for such *J*, *Th*(*J*) is semantically closed in only a very attenuated sense. The theory can characterise its own truth predicate (and related notions); however, there is obviously more to the semantics of the theory than this. For the semantics involves the whole jump-generated hierarchy, the notion of level, the relation of holding at a level, etc. Thus the object-language/meta-language distinction is retained, and the theory is not semantically closed in the spirit of the law. (In the approach I have given, the semantics are simple Tarski-type truth conditional semantics, and thus all the semantic notions employed are expressible in the theory itself. Object and meta-language are therefore identical.) Now it seems to me that it may well be possible to extend this approach to overcome this problem. What is required is to take the language to be the language of set theory (and not just – as in Dowden's case – the language of arithmetic) – so that the whole construction can be described in *Th*(*J*) itself. (Of course, if *Th*(*J*) were consistent this would be impossible, but it is not.) To show this is possible (if it is) will be no easy matter. However, it would well reward the effort. If it were to succeed it would, I think, finally destroy the distinction between the set theoretic and the semantic paradoxes.

Another interesting feature of the Dowden and Woodruff constructions is that they establish a certain precise duality between truth value gap semantics and the truth value "glut" semantics of *LP*. However, it would be a mistake to think that this shows that there is no real difference between the approaches, and that there is nothing to choose between them. Here are matters of some substance.

First, the duality holds only whilst we restrict ourselves to the extensional connectives such as conjunction and negation. However, as I have already noted, these connectives are not really adequate to express some important logical principles, such as the *T*-scheme (or for that matter, the abstraction scheme of set theory); for the material biconditional is not a detachable one.[15] To overcome this problem a genuine conditional connective has to be added to the language.[16] There are many and well known reasons for supposing that such a connective should be relevant. Once one makes this move, simple intertranslatability of gluts and gaps vanishes. Indeed, in the standard semantics of relevant logic,[17] gaps and gluts play very different roles, the former serving to destroy "positive" paradoxes of implication, and the latter "negative" ones. As Woodruff's paper makes very clear, gaps and gluts are conceptually very different, the former corresponding to incomplete theories and the latter to inconsistent ones. Moreover, as Woodruff points out, both gaps and gluts are necessary for the general construction.

However, the differences between a gap approach and a glut approach to the paradoxes show up already at an extensional level. For the gap/glut isomorphism preserves only *some* logically relevant features, and there are others that it does not. Crucially, the isomorphism does not preserve designated semantic values. On the gap approach the g-value will not be designated, whilst on the glut approach it will be. For something that is both true and false, is at least true, whilst something that is neither true nor false is not.[18] And, of course, a difference in the designated values will give rise to a different relation of logical consequence. In particular, and centrally, defining validity in the usual way in terms of material designation-preservation, on the glut approach $A \wedge \neg A/B$, $A \equiv \neg A/B$ will be invalid, whilst on the gap approach they will be valid. This is important since gap approaches will not be inconsistency-tolerating whilst glut approaches will. In particular therefore, simple formulations of set theory and semantics which retain the naive abstraction scheme and the naive *T*-scheme are not open to the gap theorist. This is to the distinct advantage of the glut theorist. Not only are these principles intuitively correct characterizations of sethood and truth, but the gap view requires us to reject both our naive views of set/truth and also the received logical theory. The glut view requires only the modification of the latter.[19]

3. EXTENDED PARADOXES

The consistency toleration of the glut view is connected with another, and I think perhaps the most crucial reason why the gap approach – or any other consistent approach – will not work, whilst the glut approach will. This concerns the extended paradoxes. According to Dowden (p. 125), neither the truth-value gap approach to the paradoxes, nor my approach, can handle paradoxical sentences of the form: 'This sentence is not true'. However this is incorrect.

To see why, let us consider an example: the liar paradox. All logical paradoxes have extended, or strengthened, forms and exactly similar considerations will apply to the others. We may take the ordinary liar paradox to be the sentence 'This sentence is false'. It will pay us to be careful about what 'false' means here. So we will take 'false' to mean 'has a true negation'. Now extended paradoxes are motivated by the thought that truth and falsity may not be exhaustive or exclusive. Because of this, they formulate a paradoxical sentence which asserts of itself that it is in the genuine complement of the truths *however that is described.* Thus, in the simple gap case, this would be 'This sentence is either false or neither true nor false', and in the simple glut case it would be 'This sentence is false only (i.e., false, and not true)'. We can catch these both under the same rubric provided we use the notion of exclusion negation, i.e., a functor $*$ such that $\ulcorner * A \urcorner$ is true precisely if $\ulcorner A \urcorner$ is not true and false otherwise.[20] The extended liar is then simply the sentence '$*$This sentence is true' (α). Now a contradiction is quickly forthcoming. For if (α) is within the set of true sentences, it is in the complement of this set. And if it is without the set of true sentences then (α) is true, and so within the set of true sentences.[21] The extended contradiction obviously shows that the gap theorist has gained little by his/her manoeuverings. S/he has still ended up inconsistent. (Of course, it is always possible for the gap theorist to maintain that exclusion negation is not expressible in the language in question. However, this is obviously unsatisfactory. For consistency is purchased only at the price of castrating the language, so that something which can be said cannot be said *in it.* If the language does not contain an exclusion negation there can be no objection to simply adding one to it, i.e., extending it by a new functor and stipulating this to have the appropriate truth condition.) What is not clear is why it should be thought that it sinks the glut theorist too. After all, the aim of the glut theorist is not to avoid contradictions, but precisely to allow for them in a way that does not lead to disaster.[22]

One reason (perhaps ultimately the only reason) why the extended paradox might be thought to sink the gap approach is that it leads to triviality. We might attempt to argue this as follows: The extended liar gives a sentence of the form $T\ulcorner A\urcorner \wedge *T\ulcorner A\urcorner$, but $\{T\ulcorner A\urcorner \wedge *T\ulcorner A\urcorner\} \models B$; hence everything follows. To support the claim of semantic consequence one might argue as follows. Whatever truth value A has (whether it be true, false, both true and false, neither true nor false, or whatever) one of $T\ulcorner A\urcorner$ and $*T\ulcorner A\urcorner$ is not true, by the truth conditions of $*$. (And it is just here that the extended liar might be thought to differ from the ordinary one.) Hence, their conjunction must fail to be true too. Thus everything is a semantic consequence of such a contradiction. What we are to make of this argument depends on how exactly '$\models$' is taken to be defined. If we take '$\models$' to be defined in terms of material truth preservation, i.e, $\{A\} \models B$ iff there is no semantic evaluation which makes A true and B false, then indeed $\{T\ulcorner A\urcorner \wedge *T\ulcorner A\urcorner\} \models B$. However, this fact is harmless enough. For such a relation of consequence does not support the detachment of B. Such a detachment would be, in effect, an application of the disjunctive syllogism "at the metalevel", and the disjunctive syllogism is not a valid inference (certainly according to the *LP* semantics, but also more generally in relevant logic.) Alternatively, we may define '$\{A\} \models B$' in a more satisfactory way in terms of genuine truth preservation thus: $\{A\} \models B$ iff every semantic evaluation which makes A true makes B true, i.e., for any evaluation, if A is true under that evaluation, then B is.[23] (where the 'if . . . then' is a relevantly defined notion.) In this case validity does licence detachment, but it is no longer true that $\{T\ulcorner A\urcorner \wedge *T\ulcorner A\urcorner\} \models B$. For to argue that it does, on the ground that the premise is never true, would simply be a fallacy of relevance. It uses, in effect, a paradox of material implication. Hence, in either case, the argument to triviality does not work. A final desperate measure is to take logical consequence to be defined proof-theoretically and simply *specify* that $A \wedge *A/B$ is an acceptable rule of inference. However, the answer to this is the same as that given by the doctor to the patient who came to see her saying "Doctor, it hurts when I do this" . . . .[24]

There is another, *ad hominem*, point to be made here. In *LP* IV.7ff I suggested that it may be acceptable to use quasi-valid inferences, such as material detachment, when not in the vicinity of paradoxical sentences. And in the present context the semantics show that the premise $T\ulcorner A\urcorner \wedge *T\ulcorner A\urcorner$ is false only, and hence not paradoxical. Thus we ought to be able to detach an arbitrary B. The point is well made. It shows that in no straightforward sense can a quasi-valid inference be used once the premise(s) have been established to be non-paradoxical. For example, the disjunctive syllogism cannot be made valid by adding premises stating that the other premises are not paradoxical, nor can the inference be said to be acceptable *if* the premises are not paradoxical. For whilst a casual perusal of the matrices of *LP* suggest that something like this ought to be the case, the problem here is that the fact that something is not paradoxical does not *rule out* its being paradoxical too.[25] Thus, in the present case, though $T\ulcorner A\urcorner \wedge *T\ulcorner A\urcorner$ is false and not paradoxical, it may be true too, which is exactly what it is. The argument for the claim that the premise is false and not paradoxical, we have already discussed. The argument for its truth (in fact its paradoxicality) is just the extended liar paradox itself. The sentence is, as one might expect, not only both true and false, but both paradoxical and non-paradoxical. Hence the enthymematic argument will fail for exactly the same reason that the disjunctive syllogism fails: The premises are true whilst the conclusion is arbitrary. The crucial point, of course, is that the "metatheory" may well be as inconsistent as the "object theory". Notwithstanding any of the above, it still seems to me to be right that in a certain sense, quasi-valid inferences are legitimately usable in consistent conditions. One would not, in virtue of the above, expect the story to be simpleminded. However, I have told it elsewhere,[26] so will not repeat it here.[27]

The preceding discussion may have been too much for some faint hearts. It has been the hope of some that we can treat inconsistency in a consistent way. We may have an object theory that is inconsistent, but our metatheory which describes it must be consistent.[28] However, this approach is too half-hearted to be workable. For it retains the object-language/metalanguage distinction. And if we are prepared to countenance this then we can avoid an inconsistent object language, as Tarski showed. In fact, the

Tarski construction is not at all satisfactory, as many writers have pointed out.[29] And the beauty of the paraconsistent approach to logical paradoxes is that it finally renders the object-language/meta-language distinction unnecessary in any shape or form. However, once the object/meta-language distinction has collapsed it follows that we have no reason to expect our own semantic discussions to be consistent. Indeed, we have every reason to expect them not to be. The diagonal construction at the heart of paradoxes is a construction which effectively tears any consistent semantic boundary, and thus destablises semantic categories.[30] But such is a fact of life.

In fact, once the object/meta-theory distinction has collapsed, it is no longer clear that one can maintain a distinction between ordinary and extended paradoxes. For the truth conditions of $*$ given at its introduction amount to this: $T\ulcorner *A\urcorner \leftrightarrow \neg T\ulcorner A\urcorner$ (and $T\ulcorner \neg *A\urcorner \leftrightarrow T\ulcorner A\urcorner$). But now if we state the truth condition of $\neg$ in the way that is very natural,[31] as $\neg T\ulcorner A\urcorner \leftrightarrow T\ulcorner \neg A\urcorner$, the distinction between $*$ and $\neg$ collapses, since $T\ulcorner *A\urcorner \leftrightarrow \neg T\ulcorner A\urcorner \leftrightarrow T\ulcorner \neg A\urcorner$. Thus $T\ulcorner *A \leftrightarrow \neg A\urcorner$, and hence $*A \leftrightarrow \neg A$. Extended paradoxes cease to be a problem even distinguishable from ordinary ones.[32]

Before finishing this part of the discussion, it may be worth saying a few words on the subject of accepting contradictions. In the preceding discussion I have countered objections by doing just that. The fact that this is an inherent possibility of the paraconsistent position has been felt by some people to be a major objection to it. The fear is that *any* objection may be met by accepting the contradiction involved. Thus, constraint on what is rationally acceptable vanishes, and rationality goes down the drain. I have discussed the issue at some length elsewhere[33] and will not repeat the discussion here. However, since the matter is important, I will say a few words. The fact that it may be possible to accept a contradiction *logically* (i.e., without trivialising one's position) does not mean that it is possible to accept it *rationally*. Even those who have taken consistency to be a constraint on rationality have held it to be a fairly minimal constraint, other factors[34] playing tougher roles, as they still can. This obviously raises the question of when it is rationally possible to accept a contradiction. No algorithmic answer to this question is to be expected. However, the acceptance of a contradiction in an area where a theory leads one precisely to expect that there should be contradictions is obviously not a rational black mark for the theory. By contrast, the acceptance of a contradiction where there is no such presupposition may be little more than an *ad hoc* evasion, with little to recommend it rationally. (Though perhaps only time and further investigations will tell.) It should be noted that where I have accepted a contradiction above, this is precisely where the paraconsistent theory of the semantic paradoxes leads one to expect that there will be contradictions lurking: where the discussion at issue is one concerning truth conditions, where the discussion applies itself, and where the diagonal construction is centrally employed. Thus, in the context, the move is, rationally, quite legitimate.

4. THE PHILOSOPHICAL POINT OF SOME TECHNICAL CONSTRUCTIONS

I have argued that gluts and gaps are not logically interchangeable, and that no pure gap solution to the paradoxes will work: any acceptable account of the paradoxes must be inconsistent. The next obvious question is whether a pure glut solution will work, i.e., whether it is necessary to countenance truth value gaps as well as true contradictions. Now obviously there are no technical problems in creating formal semantics which allow for truth value gaps, and formal constructions, such as Woodruff's, may incline us toward positing gaps as well as gluts, just because one can omit them only by placing an apparently arbitrary restriction on the construction. However, one should not be carried away by numerological considerations. The question is whether there are good *philosophical* reasons for using the notion of truth value gaps in a semantical account of semantically closed languages. I must confess that I change my mind on this matter about twice a year. Suppose we have agreed that the logical paradoxes proper are to be treated as semantic gluts. The truth value of a paradoxical sentence is over-determined by the facts. That is, the (perhaps contingent) facts of reference, plus the meanings of semantic terms (which ensure, for example, the truth of the *T*-scheme) serve to over-determine the truth value of such a sentence, and make it both true and false. It is tempting to suppose that there must be a dual situation. Perhaps there are sentences which are under-determined by the facts. That is, the facts of meaning and reference determine neither the sentence nor its negation to be true. The "duals" of paradoxical sentences, such as 'This sentence is true' might well be such sentences. Should we not say that such sentences are neither true nor false? This is indeed a possible line. However, I doubt its philosophical correctness. It is plausible to argue[35] that truth is the point of assertion, and that given that, there is no room for a gap between truth and falsity. Something is true if it meets the point of making assertions. If something fails to meet that point it is *ipso facto*, false; there is no third possibility. I do not intend to do more than state the argument here; I shall not discuss it further. However if it is right – and I find it very plausible – then a sentence which is not determined to be true by the facts of the matter is false. Thus if for some sentence A, both A and $\neg A$ are determined to be true, we have a true contradiction. But conversely, if some sentence (such as the dual of a paradoxical sentence) is such that neither it nor its negation is determined to be true, then both it and its negation are false. However, this is not an end of the matter. For by the very meaning of negation, a sentence is false iff its negation is true. Since both A and $\neg A$ are false, it follows that $\neg A$ and $\neg\neg A$ (and hence presumably A) are true. Thus, far from being truth-valueless, A is both false and true. Hence it, like its dual, is paradoxical. The route is rather more circuitous, but it gets there in the end. I certainly do not claim that the above argument is indefeasible. One might reasonably object to it in a variety of places. However, it should make the point that the existence of gaps is in no way entailed by that of gluts, and that further arguments are required.

This brings me to the question of the philosophical importance of Woodruff's construction and similar jump-generated hierarchies. Undoubtedly this kind of construction is mathematically elegant and technically useful.[36] However, again, the question is what philosophical capital is to be made out of this. It seems to me that this is a point which has been lost sight of in the heady atmosphere produced by the mathematical beauty of the construction. In fact, I doubt that it has much to offer. Once we have constructed a suitable theory which can characterise its own notion of truth, i.e., which has a predicate satisfying Tarski's adequacy condition, the T-scheme, (such as $Th(J)$ where J is an interpretation whose truth predicate is perfect and complete), and is therefore a good model of this aspect of natural language, it seems to me that there is nothing much more formally to be said about truth. The rest of the construction is therefore *philosophically* otiose. The situation is, I think, something like this: the jump-generated hierarchy was proposed initially by people who accepted the consistency of truth. Because they believed in the consistency of truth, they were unable to endorse the T-scheme. Hence, they had to find some other way of accommodating our intuition concerning the connection between a sentence and its truth. The idea was that the jump operator should do exactly this. Rather than take the connection between a sentence and its truth to be one of static bi-entailment, they took it to be a dynamic one whereby an interpretation generates another, the extension of whose truth predicate is exactly those things which hold in the first.[37] I have argued elsewhere that as a solution to the paradoxes this will not work.[38] Not only would it produce an incorrect theory of meaning, but the dynamic idea is itself out of place. Moreover, being consistent, it is still destroyed by the extended liar paradox. Why, anyway, should we take seriously a truth predicate that reports what is true not in this world, but in some world a logical jump away?

Woodruff has now extended the jump construction to allow for inconsistency. If the aim of the construction is to tell us something about the meaning of truth, it seems to me that it suffers from exactly the same flaws as the consistent version (with the exception of the extended paradoxes – unless one tries to have a consistent meta-theory, in which case this will hold too). However, even worse, the original philosophical rationale of the construction has been undercut. For the perceived connection between a sentence and its truth is now accommodated by the *T*-scheme itself: we can have, after all, a theory in which it holds. There is therefore no need to try to accommodate it with a logical hop, skip and jump.

For these reasons, it seems to me that the jump construction and its corresponding hierarchy is (rather like the ultraproduct construction) an important model-theoretic construction of great simplicity and power whose importance lies in the models and theories it allows us to construct, but which has in itself no philosophical significance. However it may be that I am too pessimistic, and that the jump construction has philosophical importance I have missed. I look forward to part II of Woodruff's paper to see what he makes of these philosophical issues.

5. ON GÖDEL'S INCOMPLETENESS THEOREM

The above considerations are, it seems to me, major and sufficient grounds for the inconsistent approach to the paradoxes. However, let us turn to another argument I used in *LP*. This is the one concerning Gödel's incompleteness theorem. In *LP* I tried to show that the theorem gives grounds for treating paradoxical sentences as true contradictions. Since the formulation was not sufficiently clear for at least one reader, and to make this paper reasonably self-contained, I will explain the argument again.[39] First, however, a word on terminology: the argument concerns our naive proof procedures. These are those informal methods of proof which are used by working mathematicians (and logicians) to settle the truth of some matter.[40] Undoubtedly these change over time. The informal methods of the 17th century are not those of the late 19th century. Hence, to be precise, by "naive proof methods" I mean those in operation now. If something can be established by our naive methods of proof, I will call it a naive theorem, and the naive proofs and theorems, I will call the naive theory.

We are now in a position to spell out the argument. My claim is that treating the logical paradoxes as true contradictions resolves the problem posed by the following two claims:

Claim 1. Let T be any consistent theory which can represent all recursive functions, whose proof relation is recursive,[41] and whose axioms and rules are naively correct. Then T is incomplete in the sense that there is a naively provable sentence that is not provable in the theory.

Claim 2. The naive theory can represent all recursive functions, and its proof relation is recursive.

The problem *in nuce* is that the naive theory is, by definition, such that anything which is naively provable is provable in it. Assuming its consistency, it would, therefore, seem to be both complete and incomplete in the relevant sense.

In this and the next sections I will discuss the two claims above. In section seven I will discuss the proposed solution. Let us start with the less contentious claim, claim one.

The content of this claim is essentially Gödel's theorem, and the sentence in question is the Gödel "undecidable" sentence. Given that the proof relation of the theory in question is recursive, and that all recursive relations are representable in the theory, let $P(x\ y)$ be the formula of the language which represents the relation 'x is (the code of) a proof of the formula (with code) y'. Then the formula in question is just, of course, $\neg\exists xP(x\ g)$, whose own code number is g. This is shown to exist by the standard argument based on the fact that all recursive functions, including the diagonal function, are representable. By the usual argument it is easy enough to show that if this sentence is provable in the theory then it is inconsistent and, hence, if the theory is consistent, it is not provable. It is perhaps worth noting that, normally, proofs of the above assume that the logic of the theory is classical. However, this is unnecessary: a weak relevant logic would do just as well. So much is relatively uncontroversial. Any doubt about claim one is likely to attach to the part which says that this sentence is naively provable.

Again, what is not likely to be doubted is that there is a naive proof (or even a proof within T) of the conditional: if T is sound, then the sentence is true. In outline the proof is this. Let the sentence be A. Either A is provable in T, or it is not. If it is provable then, assuming soundness, it is true. Suppose it is not provable. Then for every integer n, $\neg P(n, \underline{g})$ is provable. Assuming that the system of proof is sound, it follows that every integer satisfies $\neg P(x, \underline{g})$, and hence that $\forall x \neg P(x, \underline{g})$ is true.[42] What is necessary to complete the informal proof of A is an informal proof of the soundness of T.[43] I did not discuss this in *LP* taking it to be fairly obvious. This was certainly an omission. However, I think the matter is a fairly trivial one. Contrary to what Chihara says,[44] soundness proofs are not difficult; they are very easy, almost trivial. *Finitary* proofs are difficult, but non-finitary proofs are simple[45] and can normally be given with the corresponding second order machinery – which falls comfortably within the bounds of our informal proof procedures.[46] For example, to prove the soundness of Peano Arithmetic we merely define the standard model of arithmetic and verify that the appropriate axioms are true in it. Of course, the verifications use the very principles (at the "meta-level") whose truth we are supposed to be verifying. The proof might not, therefore, convince a sceptic. It is, none the less, a quite legitimate proof. We can spell out the argument in a little more detail thus:

The bases of any proof in the theory are the axioms. For any axiom, A, we can infer that $\ulcorner A \urcorner$ is true from A, which is, *ex hypothesi*, part of our naive proof procedures. Now suppose that our proof proceeds by inferring A from $A_1 \ldots A_n$ by rule R. Given that $\ulcorner A_1 \urcorner \ldots \ulcorner A_n \urcorner$ are true, we can infer that $\ulcorner A \urcorner$ is true by, essentially, rule R, which is *ex hypothesi* part of our naive proof procedures. Hence by induction over the length of proofs, all theorems are true.[47]

Thus claim one is correct. Let us turn to the perhaps more problematic claim two.

6. THE RECURSIVENESS OF PROOF

This step concerns the properties of our naive notion of proof. Part of the claim is that the naive theory can represent all recursive functions, and this is relatively unproblematic. For our naive canons of proof contain those of ordinary arithmetic, in which all recursive functions are specifiable in the usual way.

The contentious part of the claim is that the naive notion of proof is a recursive one. When I wrote *LP* I did not give this as much argument as it deserves. Let me redress this now. In fact it is part of the very notion of proof that a proof should be effectively recognisable as such. For the very point of a proof is that it gives us a way of settling whether something is true or not. It is, therefore, a proof only when it is recognised as such. The point is a common enough one. As Church puts it:[48]

> . . . consider the situation which arises if the notion of proof is non-effective. There is then no certain means by which, when a sequence of formulas has been put forward as a proof, the auditor may determine whether it is in fact a proof. Therefore he may fairly demand a proof, in any given case, that the sequence of formulas put forward is a proof; and until this supplementary proof is provided, he may refuse to be convinced that the alleged theorem is proved. This supplementary proof ought to be regarded, it seems, as part of the whole proof of the theorem . . . .

If the proof relation is effectively recognisable, then by Church's thesis (which seems entirely reasonable in the context), it is recursive.

This appeal to essence may be bolstered by the following considerations. The naive notion of proof is a social one. In particular it is one which is taught and, correspondingly, learnt. Yet the collection of proofs is (potentially) infinite. Hence the notion cannot be taught by giving a simple finite list. If proof is not a recursive notion, the process whereby it is learnt becomes unintelligible. Consider the following analogy. People are able to produce (potentially) infinitely many numerals. Moreover everyone can agree that what is produced *is* a numeral. This is perfectly understandable in virtue of the fact that numerals can be produced by applications of effective rules from a finite vocabulary. (They are a recursive class.) If this were *not* the case, then that agreement is achieved would be a mysterious and even mystical process. So it is with proof. Hence there are good reasons for supposing the notion of naive proof to be recursive. What reasons are there against it?

Several grounds might be adduced, most of suspect cogency.[49] Intuitionists have sometimes been unwilling to tie themselves down to formal systems. However, they do not deny the effective recognisability of proof. As Dummett puts it:[50]

> . . . intuitionists incline to write as though, while we cannot delimit in advance the realm of all possible intuitionistically valid proofs, still we can be certain for particular proofs given, and particular principles of proof enunciated, that they are intuitionistically correct.

Nor can they deny this. For, as Dummett has argued, the only cogent argument for intuitionism turns on the meaning and use of mathematical language and, in particular, on the fact that in mathematics truth is not, in general, effectively recognisable whilst proof is.[51] Hence there are no *specifically intuitionist* grounds for doubting this claim.

Another reason sometimes adduced against the recursiveness of proof is Gödel's theorem itself as used, for example, by the anti-mechanists such as Lucas. However, as might be expected in the present context, this just begs the question. The situation is this. Both the anti-mechanist and I agree that the recursiveness and the consistency of proof are not compatible. To infer the non-recursiveness of proof, therefore, invokes consistency and hence begs the question. To the extent that the anti-mechanists *argue* for consistency (which is not much) the arguments tend to involve *ex falso quodlibet*,[52] and so beg the question again.

It is important to appreciate the strength of this rejoinder. The argument we are analysing was given precisely to show that the law of non-contradiction is wrong (in the appropriate sense). One cannot, therefore, appeal to consistency, or the law of non-contradiction, to show that the argument fails (or one might just as well invoke the law at the beginning and sink the whole enterprise). In an argument against the law one cannot dogmatically assume it. It is necessary to produce independent arguments for the law. In this context it is worth noting that Chihara (p. 121) suggests that if it ever came to a showdown between accepting inconsistency and something else, there would be no contest in favour of the something else. Notice, however, that like most philosophers, he produces no *arguments* for consistency. (Who then is being *reason*able?)[53] And as I have already noted, such arguments are difficult, if not impossible, to come by.

To return to the main point: the anti-mechanist's argument. We may even, in fact, turn this on its head. If we assume a materialist theory of mind (which is, I think, correct, though I shall not argue it here) then presumably mechanism follows. For the neural circuitary of the brain can presumably be reproduced (in theory) by vastly more clumsy electro-mechanical devices. But then (at least according to the anti-mechanist) naive proof must be recursive, and naive theorems recursively enumerable.[54] (Notice also that there are – as far as I know – no arguments from a non-materialist theory of mind against the recursiveness of proof).

There is a final argument against the recursiveness of proof, perhaps the major one, that needs to be considered. It is sometimes suggested that proof may not be recursive because we may, from time to time, add to our axioms or rules of proof, new ones in a non-rule governed way (or at least in a way not governed by the current rules of proof).[55] To the extent that this is an argument against the view being defended here, it is an *ignoratio*, since I have said that we are considering the proof procedures as they are at any one time. However, it is sometimes suggested that the mere formulation and proof of the Gödel sentence for a theory launches us into a revision of those very proof procedures. Hence, the notion of proof we need to be concerned with is the whole diachronic one, of which each synchronic slice may be recursive, though the diachronic whole may not be.[56] Any plausibility that this line has will depend on how the idea that the novel proof procedures are generated, is substantiated. So how exactly is this supposed to be achieved?

A simple suggestion is that the proof relation is simply changed by the addition of, say, the Gödel sentence as an extra axiom. This suggestion, however, does not do justice to the facts.[57] For it is clear that in the new proof procedures, the Gödel sentence should not be axiomatic, but non-trivially provable. (The proof is essentially that given in Section 5.) Moreover there is a clear sense in which whatever the changes are that are made to allow this proof, they are not arbitrary but a natural projection of the prior proof procedures. The only plausible account of this I know, is that according to which the specification of the proof relation necessary in formulating the Gödel sentence introduces new concepts or vocabulary which can be slotted into pre-existing proof procedures which are, in some sense, schematic, to increase their strength. This has been suggested by Dummett.[58]

> . . . Once a system has been formulated, we can, by reference to it, define new properties not expressible in it, such as the property of being a true statement in the system; hence, by applying induction to such new properties, we can arrive at conclusions not provable in it.

The idea is clear enough. But it will not work. The crucial question is why the property specifiable in terms of the systematisation should be logically novel, i.e., not already in the range of the schematic (or second order) variables of principles such as that of induction. Dummett gives no arguments *why* this is so. And, in fact, it is not. For the proof relation of the old system is (as is being conceded) recursive and hence specifiable in arithmetic vocabulary already to hand. (Though the combination of symbols involved in the specification may be temporally novel.) Moreover the predicate "true statement of the system" is equivalent to "true and a formula of the system". The second conjunct is, as with the proof relation, arithmetic, and the first conjunct can be novel only once, and not indefinitely, as required.[59]

However, even if this revision of vocabulary could be substantiated, there are still reasons to suppose that this line will not work. For even granted that it is the diachronic proof relation that is relevant in this context, there are good reasons for supposing that that is recursive too. For as we have noted, the manoeuvre which is used in transcending the old system of proof is not a random or arbitrary one, but a quite determinate and rule-governed one. Thus, on this conception, we have not only rules of proof for generating theorems, but rules to generate rules of proof, to generate theorems. But Theorems in the diachronic sense are still generated by effective rules and, therefore, proof should still be recursive by Craig's theorem. Indeed, given that this process is just as teachable and learnable as the synchronic one, then similar considerations will push us to the conclusion that it is recursive.[60]

Thus claim two stands up to inspection.

7. THE DÉNOUEMENT

So much for the stage-setting. The problem is now posed. Anyone who insists upon the consistency of proof would seem, in virtue of claims one and two, to have to admit that some sentences are both naively provable and not provable. The solution I propose will, of course, be obvious. Proof (naive proof) is not consistent.[61] Indeed, given that all the other steps are right, this is the only solution.[62] Putting it in this way invites the response that we can and should change our standards of proof to consistentise them. But there are problems with this suggestion. The conditions we have assumed naive standards of proof to satisfy are quite minimal. We have assumed, essentially, only two things: (i) that they meet the conditions of claim two and, in particular, have certain arithmetical expressive power, and (ii) that given any consistent theory, they can reason in a fairly basic way about its semantical notions and, in particular, can carry through the soundness proof of Section 5. Thus, it would seem, any change can be made only by weakening our expressive capability in one of these two fundamental areas of thought. Surely changes which limit our expressive ability are not desirable.[63] And, given only that our proof procedures meet these minimal constraints, no change in them will have any effect on the argument. We might say, therefore, that our proof procedures are *essentially* inconsistent.

At any rate, our current standards of proof are inconsistent. The next move is but a short one. Some of the contradictions are true. One way of arguing this point is to note that our naive proof procedures, the means by which we establish things as true, issue in contradictions, which must therefore be true. The point is, I think, right enough. However, against this it may be suggested[64] that the conclusion does not follow, since our proof procedures may not be sound. Against this one might endorse the self-certifying nature of mathematical intuition. This line is perhaps less easy to refute than it might appear.[65] But I am not inclined to take it.[66] One might point out instead that we can give a soundness proof for these proof procedures along the lines indicated in section five. The proof, it is true, is in the very system whose soundness is being queried. This is inescapable. For as many have observed,[67] there is a sense in which any justification of our norms of inference must ultimately use those norms. The argument will not, therefore, convince a total sceptic, but may satisfy a lesser doubter. However, maybe it is the sceptic we have to deal with. After all, the objection is of the form: maybe there is *something* wrong with a proof used, and if this be an acceptable objection we lapse into scepticism, since it can be raised against any argument whatever. If my argument falls only to general scepticism I am quite content. If the objection is to have more bite than this, it must cast doubt not in general, but on some particular principle of proof assumed to hold in naive proof theory. Given the weak nature of the assumptions made, I see little scope for this and so will leave the matter to any genuine objector.[68]

In fact, given that our proof procedures are essentially inconsistent in the sense explained, the possibility that our current procedures are unsound is pretty irrelevant. For we can run the argument with respect to whatever proof procedures are sound, provided that these meet the minimal conditions we have noted, and hence the conclusion follows. Once one has conceded that our canons of proof are essentially, and not just accidentally, inconsistent, the battle is all over bar the shouting. Refusing to call contradictions true can be but an empty gesture. The situation is, in fact, one familiar from the history of philosophy. It would seem that our concepts of proof force upon us certain contradictions, not accidentally, but as part of their essence. This is, of course, precisely the position that Kant thought we were in with respect to the antinomies of pure reason.[69] Kant, it is true, did refuse the label 'true' to the antinomies. However, this is because at the last moment he turned from the brink, thinking that he could fault the arguments by applying the positivist principle that a category can sensibly be applied only to sensations.[70] Subsequent thinkers, and especially Hegel, criticised this reason and drew the appropriate conclusions: contradictions can be true.[71] This is not the place to engage in historical exegesis, so I will say no more here. However, the point is made: if we are stuck with accepting contradictions, then logic is paraconsistent. In this case, refusing the label 'true' to some contradictions can be at best a Pyrrhic victory.

But what has all this to do with the logical paradoxes? Those who have conceded my argument thus far are unlikely to begrudge me the final step. Given that there are true contradictions, why should we not suppose that the logical paradoxes are amongst them? Everything speaks for it, and nothing against it. However, the argument is tighter yet. Given that our naive system of proof is inconsistent, we no longer have reason to suppose that its Gödel sentence is unprovable. In fact, the sentence is naively provable. What is the proof? Essentially that given in section five. In this way we can prove both the Gödel sentence *and* its negation.[72] Of course these proofs are now conceived of as within the system itself. But since the system is inconsistent, this is quite acceptable. However, notice that now the system must be able to represent its own semantic notions. In particular, a perusal of the soundness proof of section five will show that the T-scheme is an essential part of the proof. But given the T-scheme which applies to all sentences, including those containing the truth predicate itself, and given the diagonalising potential of arithmetic, we know that the liar paradox and its ilk will be forthcoming. In other words, we can say what some of the contradictions in the system (some of the true contradictions) are: they are precisely the logical paradoxes. QED.

Our naive theory is an inconsistent and semantically closed theory. Thanks to the work of Dowden, Woodruff *et al.* there is no problem about seeing what such a theory is like formally.[73] By contrast, any consistent theory cannot be semantically closed. Hence semantic reasoning about the system cannot be represented in the system. But it is essentially semantic reasoning which allows us to prove the Gödel sentence to be true. Hence the disjuncture between what can be naively established and what is provable in the theory. This is the nub of the issue.

8. CONCLUSION

If, to return to the image with which I started this paper, we consider the path of papers written on the logical paradoxes, then there is much to be learnt from the more recent additions, those by Chihara, Dowden and Woodruff included. However, the case for the paraconsistent approach to the paradoxes has not been weakened. In fact, it seems to me to have been strengthened. If we consider the path of papers, not as a single line, but branching according to the approach to the paradoxes advocated, then the "Logic of Paradox" would, I still submit, be on the right track.[74]

NOTES

[1] (1979) Hereafter *LP*.

[2] And not as Chihara (p. 117) says, sentences that are true and not true, though, as we shall see, this may sometimes be the case too.

[3] Chihara (1984), Dowden (1984), Woodruff (1984). Page references to these authors are to their papers.

[4] See Priest and Routley (1983), Chs 1 and 2.

[5] On the first, see Priest (1981). On the other two see Priest and Routley (1983), Ch. 5.

[6] I shall not pursue the general issue here. The problems are well enough known. See Priest and Routley (1983), Ch. 5, Priest (1983).

[7] For more on this, see Priest (1984a).

[8] Chihara included (see below, Section 6). For the dogmatism of classical logic, see Priest (1984a).

[9] Łukaciewicz (1971). For a discussion of Łukaciewicz, see Priest and Routley (1983), Ch. 1. For more modern attempts, see Priest and Routley (1983), Ch. 5.

[10] Chihara, p. 124.

[11] One might also ponder the possibility of definition in the other direction.
[12] See Routley (1977) and Brady (1984).
[13] Note that the truth conditions of *LP* are just those of Woodruff restricted to exclude the gap case, as the appendix of Priest (1980) makes clear.
[14] In Priest and Crosthwaite (198+).
[15] In *LP* §IV I pointed out the general problem of the failure of extensional "*modus ponens*". The failure seems to become intolerable in the particular case where the logic is used as the underlying logic of set theory and semantics.
[16] As in Priest (1980).
[17] I.e., those of Routley and Meyer. See Routley *et al.* (1984).
[18] Woodruff can conclude (p. 228) that we cannot "tell the difference" between gaps and gluts only because it is impossible to tell the difference without taking designation into account. In a similar way the duality of truth and falsity in classical logic implies that we cannot "tell the difference" between truth and falsity without taking designation into account. (The point of calling something true is not captured merely by giving truth conditions.)
[19] Hence Woodruff is just wrong (p. 213) in claiming that it is no better to have only gluts than only gaps.
[20] Exclusion negation is commonly contrasted with choice negation, which is a functor † such that $\ulcorner \dagger A \urcorner$ is true just if $\ulcorner A \urcorner$ is false (and false just if $\ulcorner A \urcorner$ is true). Exclusion and choice negation correspond to the operators *c* and *d* of Woodruff, p. 227.
[21] A slightly different version, suitable for people with intuitionist leanings, is obtained by simply applying the reductio scheme $(A \rightarrow *A) \rightarrow *A$ to the instance of the *T*-scheme for (α): $T\alpha \leftrightarrow *T\alpha$.
[22] As I pointed out in *LP* §V.3, where a slightly different version of the extended liar is given.
[23] In *LP* §III.7, I defined validity in terms of material truth preservation. I now think this is wrong, just because such validity does not support detachment, and that the second approach is correct. See Priest and Routley (1982). Of course, the adoption of the second possibility is not open to someone who uses classical logic in their "meta-theory". So much the worse for that policy.
[24] These remarks are pertinent to whether the addition of "Boolean negation" to a language ruins it for paraconsistent purposes. On which see, e.g., Belnap and Dunn (1983). For * is really just Boolean negation. If Boolean negation is characterised proof theoretically, including the rule *ex falso quodlibet*, then clearly it does. However, if it is characterised semantically, it does not. These two characterisations are equivalent only given the account of validity in terms of material truth preservation, which is a mistake.
[25] A warning to this effect was sounded in *LP* §V.2.
[26] I have discussed the general legitimacy of the use of quasi-valid references in Priest (1984b).
[27] We might pursue "meta-language" contradiction further. A final argument against the approach I have adopted here might go as follows. Given that we have a theory of truth, one of whose consequences is a sentence of the form $T\ulcorner A \urcorner \wedge *T\ulcorner A \urcorner$, and given that such a sentence cannot be true, it follows that the theory must be unsound. But a sound theory is obviously desirable. Hence this approach to the paradox is not workable. This argument too will not work. Even assuming that it follows that the theory is unsound (and there are a number of further steps and assumptions which

need to be spelt out and which might well be questioned here), given the context we are working in, this does not prevent the theory being sound too. Hence, this "one-sided" criticism misses its mark. The "double-sided" situation is precisely to be expected.

[28] See, e.g., Rescher and Brandom (1979), Ch. 26.

[29] See, e.g., Kripke (1975), Gupta (1982), Priest (1983).

[30] The point is made at greater length in Priest (198+a).

[31] This approach is taken in Priest and Crosthwaite (198+).

[32] The final worry that Dowden raises concerning my approach is to the effect that the proof theory for *LP* will be "byzantine". This can be dealt with quickly. A suitable proof theory can in fact be found by simply dropping the axioms and rules for tense operators in the axiomatisation given in Priest (1981). Whether this is byzantine I am content for the reader to judge. If the problem is not with *LP* itself, but with the proposal concerning quasi-valid inferences, this was suggested as an informal procedure. The idea can, however, be made quite precise. See Batens (1984).

[33] In Priest (1984b).

[34] For example, being progressive, in the sense of Lakatos (1970).

[35] As in Dummett (1958–9).

[36] On a very small point (Woodruff, p. 216), the weak truth value gap matrices can be generalised in a natural way to allow for gluts. See Priest (198+b).

[37] This kind of motivation is pretty explicit in, for example, Gupta (1982).

[38] Priest (198+a).

[39] I have not attempted a point-by-point answer to Chihara. This would be tedious, and in the end only bog down the discussion. However, I take up all Chihara's major points that do not arise from a misunderstanding, and in other cases it will be easy enough for the reader to work out what I would say.

[40] I take it that these are what Chihara (p. 123) calls apparent proof procedures. What he calls naive proof procedures are rather different. In particular, therefore, his argument against the determinateness of (what he calls) naive proof procedures is an *ignoratio*.

[41] To say this is, of course, to presuppose that the proofs of the theory can be coded arithmetically. The naive theory is a theory in a natural language, or a part of it, the mathematical vernacular. Given perhaps a little tidying up of the grammar, there is no reason why this should not be coded in the usual way.

[42] A slightly different argument is given in *LP*, pp. 222–3.

[43] It is not necessary to prove the consistency of the system as Chihara (p. 119) suggests. Classically, of course, soundness implies consistency. However, naive proof theory, as I envisage it, is sound but inconsistent.

[44] Chihara, p. 119. Actually, what Chihara says is that consistency proofs are difficult, but since classically soundness implies consistency and Chihara is thinking classically, the attribution is right enough.

[45] Actually, finitary consistency proofs themselves are not so difficult once we use a relevant logic. See Meyer (1976).

[46] Non-finitary principles, such as the induction principle, may be just as self-evident (in the sense that once understood they need no further proof to settle their truth) as finitary ones. There may be occasional disputes over the points on which proof may legitimately rest. However these must always remain peripheral or the very notion of proof (as a particular "language game") would break down.

[47] Much fuller details of this kind of construction are given in Wang (1962), Ch. 18.
[48] Church (1956), p. 53.
[49] I am not impressed by Chihara's appeal to authority (p. 121).
[50] Dummett (1959), p. 184 of reprint.
[51] See Dummett (1973).
[52] See, e.g., Lucas (1961), p. 53 of reprint.
[53] I am surprised that Chihara, of all people, should take this dogmatic attitude. For in at least some of his papers (e.g., (1979), Section 7) he suggests that our naive notion of truth is inconsistent, but not incoherent. I would have thought that this would make him a little less ready to adopt such a dogmatic attitude.
[54] Gödel seems to agree that recursive enumerability follows from materialism. See Wang (1974), p. 326. Strictly speaking, the argument shows only that the set of theorems establishable by one person is r.e. However the notion of what is naively provable is a social, and not a purely subjective, one. The connection between these two things would need, therefore, to be spelt out.
[55] See, e.g., Wang (1974), p. 325; *LP*, p.222.
[56] A formal model of this process might be something like the construction given in Feferman (1962) where we have a hierarchy of theories each of which is recursively enumerable and each of which can prove the Gödel formula for lower members of the hierarchy.
[57] Though this is how the Feferman construction in effect proceeds.
[58] Dummett (1963), p. 195 of reprint. Notice that the revision of vocabulary need not require a revision of coding since the language of the whole hierarchy generated may be coded at stage zero.
[59] It might be claimed that at each stage a novel truth predicate is added, and hence the progression takes us up the hierarchy of Tarski metalanguages. This suggestion is to be rejected on the ground that this conception of truth cannot be right, as I have already discussed. (see fn. 29 and the text thereto.)
[60] One should note that the cumulative proof relations of both the Tarski and the Feferman constructions become non-recursive only when they cross the final frontier and go where no man has gone before: the transfinite.
[61] Isn't this what the logical paradoxes showed us all along? The fact that such an argument is required to prove the obvious is a sure sign of the grip of a philosophical theory on philosophical minds.
[62] A wry dialectician might ask (*ad hominem*) what is wrong with simply supposing the sentence to be both provable and not provable? In reply I would say that anyone who really accepts this has gone all the way to meeting me anyway: some contradictions are accepted as true. In any case this can be avoided by putting the argument in its contraposed form: if the sentence is provable, the theory is inconsistent, but the sentence is provable. Therefore . . .
[63] It is not even clear that they are *possible*, at least at will. As Wittgenstein made plain, we have little control over what arguments we find logically compelling.
[64] As Chihara does suggest.
[65] The fact that standards of proof change – which is beyond dispute – hardly settles the matter. For along with the change in standards of proof goes in general a change in the meaning of the sentences involved in proof. See Lakatos (1976), Ch. 8. From the fact that a sentence can now be shown to be false which was provable before, one cannot therefore infer that the old standards of proof were unsound.

[66] The difficulties of dogmatism are well known. See Lakatos (1962).
[67] See, e.g., Dummett (1975).
[68] Perhaps the only natural places for suspicion are some of the semantic principles involved, particularly the *T*-scheme. However the *T*-scheme falls under suspicion only when it applies to sentences of its own language, and there is nothing in the argument which requires this. Indeed, given that claim one specifies consistent theories, the soundness proof of section five must be considered as being carried out in a metatheory. There are therefore no problems with the *T*-scheme.
[69] See Kant's *Critique of Pure Reason* A297, B354f; A339, B397.
[70] See Priest and Routley (1983), Ch. 2.
[71] See Section 48 of Hegel's lesser *Logic* (Part 1 of the *Encyclopedia of the Philosophical Sciences*).
[72] Thus I agree with Woodruff (p. 221) that contradictions should not spread to areas unconnected with paradox. However I disagree that sentences without the truth predicate (and *a fortiori* grounded sentences) are necessarily such. In fact the Gödel sentence is one of the family of paradoxical sentences of the form $\neg\theta\ulcorner A\urcorner$ where θ is any functor satisfying the rules $\vdash B \Rightarrow \vdash \theta\ulcorner B\urcorner$ and $\vdash \theta\ulcorner B\urcorner \rightarrow B$, such as necessity, provability, etc.
[73] However we should note that an adequate formalisation of our naive proof procedures should be not only axiomatic, but have inconsistent (absolutely) grounded theorems. Dowden's construction does not satisfy this condition, though Woodruff's construction is more generous in the possibilities it allows.
[74] This paper, written fairly speedily, has not benefited from a reasonable maturing process. Ideally I would have liked more time to think about some of the issues involved, and receive comments and criticism. I am, however, exceptionally grateful to the Editor for comments on the first draft of the paper submitted, which helped a great deal.

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