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In 1995, with the publication of volume 3 of his collected papers, Gödel's sustained effort during the 1950's to refute the Vienna Circle's conception of mathematics (which he called "a combination of nominalism and conventionalism") was made widely available; already it has stimulated extensive philosophical commentary.[1] Attention has focussed, above all, on several drafts of what was to have been Gödel's contribution to the Carnap volume in the *Library of Living Philosophers* (Gödel *1953/9), a series to which Gödel had contributed twice already. In the end, Gödel decided against publication, and the volume appeared without his paper. Gödel himself, though convinced that "very weighty and striking arguments in favor of my views" could be alleged against the positions of Carnap he was attacking, seems to have had doubts about the effectiveness of his argument. (Goldfarb 1995, p. 324)

The recent commentators have tended, on the whole, to dismiss these doubts and support Gödel's view that Carnap's overall framework, based on the "principle of tolerance", is self-undermining.[2] Even those who share Gödel's doubts, like Goldfarb and Ricketts (1992), have held that Carnap's view can only be upheld in a weakened or diluted (and rather empty) form. But Carnap's own later views on these issues have hardly been given a hearing. If Gödel's confidence hadn't wavered, and something like one of his extant drafts had been published, Carnap would certainly not have replied in the style of Goldfarb and Ricketts. Our purpose in this paper is not so much to reconstruct what the historical Carnap might *actually* have said in such a response[3], though, as to suggest what he *could* have said within the framework of the overall position he had sketched out in his later

[1] Goldfarb 1995; Goldfarb & Ricketts 1992; Richardson 1994; Ricketts 1994, 1996; Friedman 1999; Parsons 1997; Potter 2000; Tait 2001.
[2] Friedman 1999, Richardson 1994, Potter 2000, Parsons 1997.
[3] An actual response of Carnap's to Gödel would have been colored by Carnap's enormous respect for Gödel and feeling of indebtedness to him (though there was also a debt in the other direction; see Awodey and Carus 2001). And Carnap was hardly ever an outspoken controversialist; one of the frustrations of his debate with Quine about analyticity, for example, is his reluctance to spell out his full position, at least in published form (Creath 1990); he tended in all of his replies (Carnap 1963) to construct his critics' points as narrowly as possible. In telling Carnap what he "could have said to Gödel", then, we make the unrealistic assumption that he would have felt free to spell out his fundamental differences with Gödel quite explicitly and place them in a wider context.

period.[4] This not only results in some interesting clarifications of his later position, casting doubt on the still widespread tendency to dismiss it unheard, but also leads us to the diagnosis, in Gödel's argument, of a rather subtle fallacy that has so far escaped the commentators.

I.

Gödel summarizes the syntactic program[5] in two assertions, that *mathematics can be interpreted to be syntax of language* and that *mathematical sentences have no content.* The investigations in Carnap's *Logical Syntax of Language*, he claimed, as well as those of the Hilbert school, had shown the following about these two points:

> (1) *Mathematics can be interpreted to be syntax of language* only if the terms "language" or "syntax" or "interpreting" are taken in a very generalized or attenuated sense, or if only a small part of what is commonly regarded as "mathematics" is acknowledged as such. . . . (2) *Mathematical sentences have no content* only if the term "content" is taken from the beginning in a sense acceptable only to empiricists and not well founded even from the empirical standpoint. Thereby these results become unfit to. . . support . . . the philosophical views in question (such as nominalism or conventionalism). (Gödel *1953/9-III, p. 337)

Regarding assertion (2), Gödel maintains that the examination of the syntactical viewpoint "leads to the conclusion that there *do* exist mathematical objects and facts which are exactly as objective (i.e., independent of our conventions or constructions) as physical or psychological objects and facts". (ibid.) Carnap, of course, would have regarded such questions (asked outside the context of a particular linguistic framework) about the existence or non-existence of any objects, whether physical or mathematical, as empty of cognitive significance. To that extent, he would have been happy to agree with Gödel that there is no difference between the two kinds of objects.

He would even have gone along with Gödel's insistence that they are equally "objective" in the sense that they are independent of our conventions or constructions. Carnap might have allowed

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[4] This framework was very much a sketch, and has only recently been the subject of tentative discussion and interpretation, above all by Stein (1992), Bird (1993), and Jeffrey (1994). The general understanding of Carnap's later position portrayed in this paper is greatly indebted to Howard Stein.

[5] The Carnap of the mid-1950's no longer regarded himself as taking a "syntactic" view of mathematics, of course, but for the purposes of Gödel's argument this makes so little difference that we ignore it here (except briefly in section III).

that there sometimes appears in this respect to be little distinction between the two kinds of objects, and that the distinction between *Formalwissenschaft* and *Realwissenschaft* can seem arbitrary. He might have given an example along the following lines: The computer-aided investigation of the distribution of large primes may appear not to differ much, in principle, from e.g. the empirical investigation of the distribution of matter in the solar system. In both cases we have a stream of results stretching from the current moment indefinitely into the future, to which we can assign a prior probability. Is it not artificial, then, to distinguish the results of these investigations into "analytic" on the one hand and "synthetic" on the other? But Gödel was less concerned to deny *this* distinction than to insist that there is no difference in the objectivity — independence of our conventions and constructions — of the two different kinds of "object" or universe of discourse.

Carnap would have said that they are both equally dependent and equally independent of our conventions and constructions. In investigating the distribution of matter, we have to decide what experimental result will be regarded as evidence for "matter", i.e. we have to "decide what we mean" by the expression "matter" (we must decide on an *explication*, in Carnap's sense[6], for the pre-theoretical term "matter"). Once we have decided this, the occurrence of matter (in the sense agreed) is outside our control; it is determined by experience (though of course we are free to change our language again, if the scientific community finds it desirable). When we investigate the distribution of primes, we must also make our terms precise; in this case, the basic explications (of "prime number", "divisibility", etc.) seem more "obvious" since they date back to Euclid, though arithmetic was not axiomatized until the nineteenth century. Nonetheless, once we have accepted this framework, and have programmed it into our computer, we no longer have control of what the computer finds; it is new information just like that about interplanetary matter.

The difference between these cases lies not in their objectivity (their independence of the framework we choose), for Carnap, but in the resources required to find out whether a sentence bearing on them is true (in the usual framework-relative, internal sense). In the one case, facts

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Also, Gödel speaks not of the syntactic "view" or "program" but of the syntactic "interpretation" of mathematics; we discuss this term in section II below.

[6] As explained in Ch. 1 of *Logicial Foundations of Probability.* The differences between this concept of explication (and clarification) and the notion of "clarification" often used by ordinary-language philosophers are discussed by Carnap in his reply to Strawson (Carnap 1963, pp. 933-940); see also Stein 1992, especially pp. 280-282.

about the world are required, in the other case no fact about the world is relevant. This becomes clear, Carnap might have pointed out, if we consider how we would respond when the computer turns out an unexpected result: e.g. that the distribution of primes appears to increase over some range of numbers, rather than decreasing as expected. Our first response will be to ask the question, "Is this result a reflection of the real distribution, or is there a malfunction in the computer?" We find it *useful*, in other words, to maintain a distinction between empirical and purely logical (or mathematical) objects and statements.

This example shows how Carnap's and Gödel's analytic-synthetic distinctions differ fundamentally. For Gödel, this distinction is ultimate and metaphysical (and corresponds to an ultimate *epistemological* difference between the human faculties for "perceiving" the logical and empirical realms), while for Carnap the analytic-synthetic distinction is pragmatic. It is not anterior to his principle of tolerance, but a practical requirement or constraint on the choice of language.[7] It is wrong to say, then, as Michael Friedman does (1999, pp. 219-221), that the principle of tolerance depends on the presupposition — *prior* to any language choice — that analytic and synthetic sentences are distinguishable. For Carnap, Friedman says, ". . . what shows us that external questions are purely pragmatic is precisely the circumstance that they concern, in the end, only the question of which primitive *analytic* sentences to adopt. It is precisely for this reason, that is, that such questions involve us with no 'matters of fact'." (ibid., p. 220)

This is a misunderstanding. "Analytic" (like "existent" and other terms) can be understood as applying *internally* or *externally*. *All* rules defining a language, whether L-rules (logical axioms or rules of inference, in the *Syntax* terminology) or P-rules (basic principles of physics), are trivially provable from themselves, and so *externally* analytic. *Internally* the P-rules (if any) and their consequences are by definition *synthetic* (Carnap 1934, p. 138), though *externally* analytic.[8] The rules of a language, *all* analytic externally, determine which sentences in the language are internally synthetic and which are internally analytic. So the *distinction* between analytic and synthetic can only be internal; the external question whether "there is" an analytic-synthetic distinction

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[7] See Carnap 1966, pp. 257ff. as well as the discussions in Stein 1992 and Bird 1993.

schlechthin has exactly the status, for Carnap, as the external question whether numbers or other abstract entities really exist.

II.

But it is the *first* of the two assertions Gödel makes in the above quotation that has excited all the recent commentary: his assertion that the "syntactic interpretation of mathematics", as maintained by Carnap and other members of the Vienna Circle, can be *proved false*.[9] Carnap would have found Gödel's argument[10] very interesting, but would not have accepted it. To understand why, it will be best to treat this rather subtle point with Carnap-like, perhaps almost pedantic, thoroughness. Gödel's argument can be paraphrased in the following four steps:

> (i) For mathematics to be interpreted as syntax of language — and thus empty of empirical content — it must be proved that no syntactic (i.e. purely linguistic) stipulation can possibly have empirical consequences; otherwise mathematics is in danger of making claims about the empirical world on purely arbitrary, definitional (however convenient or practical) grounds.
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> (ii) But even the choice of a very weak language framework (as restricted as primitive recursive arithmetic) has the consequence, by Gödel's own second incompleteness theorem, that the consistency of our chosen language cannot be proven with its own resources.

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[8] And the sentences of the observation language (sticking to a quasi-*Syntax* terminology) are still synthetic even if there are P-rules. Note that a language with P-rules would in principle have to be changed (would require new P-rules) every time an accepted observation conflicted with a P-rule.

[9] Following the quotation at the beginning of section I above, Gödel says, ". . . if the terms occurring are taken in their ordinary sense, then assertion 1 ["*mathemtics can be interpreted to be syntax of language*"] is disprovable." (Gödel *1953/9, p. 337)

[10] In draft III, published as the first part of Gödel *1953/9, he formulates the argument as follows: ". . . a rule about the truth of sentences can be called *syntactical* only if it is clear from its formulation, or if it somehow can be known beforehand, that it does not imply the truth or falsehood of any "factual" sentences (i.e. one whose truth, owing to the semantical rules of the language, depends on extra-linguistic facts). This requirement not only follows from the concept of a convention about the use of symbols, but also from the fact that it is the lack of content of mathematics upon which its apriori admissibility despite strict empiricism is to be based. The requirement uner discussion implies that the rules of syntax must be demonstrably consistent, since from an inconsistency *every* proposition follows, all factual propositions included." (p. 339) Similarly, in the same version: "*To eliminate intuition or empirical induction by positing the mathematical axioms to be true by convention is not possible* For, before any convention can be made, mathematical axioms of the same power or empirical findings with a similar content are necessary already in order to prove the consistency of the envisaged convention. A consistency proof, however, is indispensable because it belongs to the concept of a convention that one knows it does not imply any propositions which can be falsified by observation (which, in the case of mathematical "conventions", is equivalent with consistency . . .)." (ibid., p. 347)

(iii) Any proof that our chosen language is consistent, then, presupposes the consistency of the stronger metalanguage required for the proof, so the attempt to prove consistency — at any level — incurs an infinite regress, and we cannot completely exclude the possibility that the chosen language is inconsistent, and thus has empirical consequences (as it would imply not only every mathematical sentence, but every empirical sentence).

(iv) Conclusion: The requirement of step (i) cannot be met, so mathematics cannot be syntax of language.

Carnap would have pointed out that Gödel's assertion in step (i), though, that the consistency of any stipulated language must be *provable*, is not implied by what Gödel calls the "syntactic interpretation" of mathematics (SIM). He is right to point out that the SIM entails the consistency of any language stipulated, but this is not the same as *provable* consistency. Carnap could have agreed with Gödel on "if P then Q", in other words, but Gödel uses "if P then *provably* Q" as the basis for his argument. This stronger assertion rests on an apparent non-sequitur in step (i); let us examine the argument more carefully. Gödel begins with the correct statement that

(A) SIM implies that mathematics is empirically vacuous.

He also reminds us, correctly, that

(B) If a stipulated language for mathematics is inconsistent, then it may have empirical consequences.

From this, by contraposition, it follows that

(B′) If a stipulated language for mathematics is to be empirically vacuous, it must be consistent.

But from A and B′ it follows only that

(C) SIM requires the consistency of any stipulated language for mathematics.

It does not follow, as Gödel suggests in (i), that

(D) SIM requires the *provable* consistency of any stipulated language for mathematics.

In short, where Gödel says "the rules of syntax must be demonstrably consistent, since from an inconsistency *every* proposition follows" (ibid.), he should correctly say "the rules of syntax must be consistent, since from an inconsistency *every* proposition follows".

This small difference is an important one, as can perhaps be seen more clearly by considering Gödel's argument (shown on the left-hand side below) in conjunction with an analogous one — which might easily have occurred to Carnap if he had written his own reply to Gödel — shown on the right-hand side:

(i) For mathematics to be syntax of language, it must be proved that no stipulation can have empirical consequences.	(i′) For spacetime to be flat, it must be shown that such and such conditions (indicating curvature) do not obtain in any region of the universe.
(ii) But even a very weak language cannot be proved consistent without further assumptions. Any proof that our chosen language is consistent, then, presupposes the consistency of the stronger meta-language required for the proof.	(ii′) But the required observations may be affected by the presence of curvature; e.g. measurements may be distorted or (iv) (iv) instruments become unreliable. Moreover, the universe may be infinite, or there may be regions that are in principle inaccessible to us.
(iii) We can never be certain that the chosen language is consistent, and has no empirical consequences.	(iii′) We can therefore never be certain that our observations are conclusive, and that the required conditions obtain.
(iv) Conclusion: The requirement of step (i) cannot be met, so mathematics cannot be syntax of language.	(iv′) Conclusion: The requirement of step (i′) cannot be met, so spacetime cannot be flat.

The argument on the right differs from that on the left in that it concerns an empirical question. But it has the same logical form as Gödel's argument (on the left), and it is no less sound. In both cases, the conclusion is unwarranted.

The erroneous claim in the right-hand argument is "it must be *shown* that . . ." in (i′). It would be correct to say "it must be *the case* that . . .", for spacetime to be flat, such and such conditions do not obtain. It would also be correct to say "for spacetime to be *shown* to be flat, it must be *shown* that such and such conditions do not obtain". In Gödel's argument, likewise, though "if P then Q" is correct, and "if provably P then provably Q" is correct, neither of these is equivalent to Gödel's "if P then provably Q".

Gödel's argument, therefore, does not refute the possibility that mathematics can be interpreted as syntax of language, as he claims. However, a slight modification of this argument does show something else of equal interest. Though we saw that (D) above is unwarranted, it would be correct to say

(E) A *proof* of SIM requires the *provable* consistency of any stipulated language for mathematics.

As Gödel correctly argues, the consistency of any stipulated language cannot be proved. If Q is false then "if P then Q" is true only if P is false. So by (E) there can be no proof of SIM. *We cannot prove that mathematics is syntax of language by mathematical reasoning.* But this result, far from underminung SIM, is in complete harmony with it. For the syntactic view implies the vacuity of mathematics, which would surely be violated if that viewpoint could itself be *proved* mathematically, as such a result would itself be a non-trivial mathematical proposition.

III.

Gödel might nonetheless have wanted to respond, as Michael Potter has argued in a recent book (Potter 2000), that mere consistency is not enough — that Carnap's position requires *provable* consistency, and is therefore impossible. Potter's argument invokes the constraints on the principle of tolerance resulting from the addition of semantics to the study of scientific language. As Carnap himself put it, the syntactical rules

> . . . can be chosen arbitrarily and hence are conventional if they are taken as the basis of the construction of the language system and if the interpretation of the system is later superimposed. On the other hand, a system of logic not a matter of choice, but either right or

> wrong, if an interpretation of the logical signs is given in advance. (Carnap 1939, p. 48, quoted by Potter 2000, p. 272)

"In other words," Potter says, "the introduction of a theory of semantics is to be seen as a constraint on the principle of tolerance." This is not quite right. The introduction of a *theory of* semantics is not yet a constraint; a particular interpretation must be given first. And even then, this interpretation is not a constraint on the principle of tolerance as such, but on the freedom to choose a syntax for that particular interpretation. These distinctions might seem trivial, but weighty consequences follow. For Potter takes Carnap, by the introduction of this "constraint of interpretability" (Potter 2000, p. 272), to be committed to a *particular* fixed semantics in which at least some descriptive sentences mirror the world "indefeasibly" (ibid., p. 277).

Potter sees Carnap's position as resting on a kind of transcendental argument by which consistency is a necessary condition for the possibility of the descriptiveness of language. On Carnap's account, Potter argues, we can never be sure that the language we use actually describes anything unless we prove that it is consistent; for the discovery of an inconsistency would deprive the entire language of its descriptive capability ("since every sentence is derivable, every sentence is determinate, and the language has no descriptive vocabulary at all"[11]; ibid., p. 271). So to refute this account "there need be only one sentence in my language which I am indefeasibly sure I can use to express an empirical claim", Potter says (ibid., p. 277), since such a sentence would imply certain knowledge of consistency, according to Carnap's account. Moreover, says Potter, the "constraint of interpretability", which concedes the existence of such "indefeasible" sentences (ibid.), thereby commits Carnap to the claim that we know mathematics to be consistent. But we know by Gödel's second incompleteness theorem that we can*not* know this, so Carnap contradicts himself; in fact, says Potter, this is "as close to a straightforward contradiction as one is likely to encounter in philosophy". (ibid.)

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[11] Note that while Gödel speaks of the "empirical consequences" derivable from an inconsistency, Potter deprives the language of *any* possibility of descriptive (or empirical) force in that case. This is only a terminological difference; Gödel uses "empirical" to mean "non-logical" (as we saw in section I, he accepts an analytic-synthetic distinction prior to any language, reflected in different human *faculties* for "perceiving" facts in the logical and empirical realms), while Potter uses "descriptive" (in Carnap's sense, putatively) to mean "contingent" or "not derivable".

Carnap could have made two points in response to Potter's brief on behalf of Gödel. First, the imputed "transcendental argument" misrepresents his view. The descriptive capacity of a language is not destroyed by the discovery of an inconsistency since we can change the rules or postulates of a language without changing its vocabulary, thus preserving all the purely descriptive sentences; there is no need to start from scratch, as Potter claims.[12] Second and more importantly, Potter is quite mistaken in attributing to Carnap a *fixed* "constraint of interpretation", or the acceptance of even a single "indefeasibly" descriptive sentence. It is fundamental to the later Carnap's view, as we saw above (section I), that there is *no* fixed partition, antecedent to any language, of sentences into analytic ones and synthetic ones. *Any* sentence whatever, including Potter's favorite "This table is black", could, if it were for some reason convenient, be declared (externally) analytic (could become a "P-rule", in the *Syntax* terminology; Carnap 1934, p. 133), and be thereby deprived of its descriptive capacity. No sentence, regarded in isolation, is *inherently* descriptive. So both of the premises Potter attributes to Carnap rest on misinterpretations, and the contradiction vanishes.

IV.

Gödel speaks of the "syntactic interpretation" of mathematics; this vague expression itself requires some interpretation, and there are signs in the evolution of Gödel's drafts that he was concerned to clarify what he meant by it. But however Gödel had decided to clarify this concept, Carnap would have wanted in his reply to make clear that the *syntactic* "interpretation" is of a different kind, or at a different *level*, from the "interpretations of mathematics" with which it might appear to be in competition, like the classical trio of logicism, intuitionism, and formalism, or Gödel's own position of Fregean or (in some sense) Platonic realism.

What all such "interpretations" have in common is that they can be regarded as informal clarifications of the role of mathematics in knowledge. As such they are rather vague, however;

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[12] Maintaining a vocabulary across changes in rules or postulates is not tantamount to the assertion of a language-independent empirical world; we remain free to change the basic vocabulary as well (e.g. to make fields basic rather than particles) if it turns out to be convenient. Also, quite apart from this, the *actual* Carnap would have pointed out that only the theoretical sentences of a language would even require such a translation in the event of inconsistency, since he put observation sentences into a separate and much simpler observation sublanguage (e.g. Carnap 1956). This creates problems of its own, of course, but not one about recovering the observation sentences after a discovery of inconsistency in the theoretical language.

they can be (and have been) given *explications* as different formal systems for the expression and proof of mathematical statements, like set theory, type theory, or first-order arithmetic. What, then, explicates the syntactic interpretation? Like the other "interpretations", it is an informal clarification of the truth and applicability of mathematical statements, and can also be explicated (this was the subject of *Logical Syntax*). But the syntactic interpretation is not a *decision* for any particular formal system. It is, rather, a statement of *neutrality*, both among the other various interpretations (or clarifications), and among the formal systems suggested as their explications. It is the assertion that none of these interpretations can claim to be *true* or *correct* in the absolute or language-transcendent way in which each of them often claims to be; it regards mathematical truth and existence as language-relative. Each of the different formal systems advanced has its own advantages and disadvantages as a proposal for constructing the language of science, however, and it is only these *practical* advantages and disadvantages, the syntactic interpretation proposes, that remain (once absolute truth or correctness are eliminated) as possible criteria for choosing one such system over others.

What Gödel calls an interpretation of mathematics should therefore, according to Carnap, be taken not as an *assertion* about some state of affairs, but as a *proposal* for clarifying the nature and uses of mathematics, a proposal which can then, in most cases, be explicated by a particular formal system. The neutrality of the syntactic interpretation among the others puts it in a peculiar position in this respect, however. It is not explicated by any particular formal system; it proposes, rather, a criterion of *choice* among the various systems (i.e. the criterion of usefulness, which of course has no content until a purpose is specified). It might thus be seen as located, so to speak, at a meta-level with respect to the other interpretations. It holds that the nature of mathematics (especially the old puzzle how mathematics can be both necessary *and* applicable to empirical facts) can only be clarified and explicated by abandoning the claims to framework-transcendent truth advanced by the traditional interpretations of mathematics (like intuitionism or formalism), and focussing instead on the formal systems themselves as candidates for some particular *practical* role, e.g. to be used as the language of natural science.

The informal concepts clarified by the syntactic interpretation, then, are framework-relative or proposal-relative logical truth and analyticity. And the syntactic interpretation is itself the proposal that truth and analyticity be regarded as proposal-relative. It is therefore, unlike other "interpretations", self-applicable: it belongs to the class of statements (those informally called "proposals for language frameworks") to which it applies. This circularity is not vicious; the proposal conforms to the rule it proposes, but not by stipulative fiat of that rule itself. This is perhaps easier to see if we contrast it with other explications. The concept of differentiable function, for instance, left intuitive by Leibniz, Newton, and Euler, was explicated in the well-known way by Cauchy and Weierstrass. In this case, the explicandum is not a property of statements but a property of functions on $\mathbf{R}^n$, which some functions possess and others do not. It does not make sense to ask whether the Weierstrass definition of differentiable function is differentiable. There is no problem of self-applicability.

In contrast to this case, consider Tarski's explication of the concept of truth. Here we can, without contradiction, ask "Is Tarski's truth-definition true?" This question is answerable, however, only if it is asked with specific reference to a particular semantic system containing a truth-definition. Without reference to a particular language system, it no more makes sense to ask this question than it does to ask whether numbers (or uncountable sets, or electromagnetic fields) exist. If truth is defined for a language in Tarski's way, then the truth of this definition, expressed in an appropriate meta-meta-language, follows trivially from the fact that it is the definition (just as the statement "a real-valued function $y = f(x)$ for which the limit of $\{f(x) - f(x + d)\}/d$ as d approaches 0 exists is differentiable at x" is true in the framework of classical analysis). The truth definition applies to itself, but trivially. (And it is important to note that its truth says nothing whatever about its quality or usefulness as an explication of truth compared to other possible explications.)

Interpretations of mathematics, in Gödel's sense, are generally not self-applicable even in the trivial sense of Tarski's explication of the concept of truth. They stand outside the discourse they purport to be about; they are not themselves intended as mathematical statements. But what sort of statements *are* they? *Not*, in any case, ones that have a clear significance within the discourse they themselves constitute. The syntactic interpretation has the advantage that it *is* consistent with itself in this respect. The stipulation that analyticity is a matter of constitutive stipulation makes *itself* analytic within the framework it constitutes.[13] And although this self-applicability does nothing to *justify* the syntactic interpretation, the fact that it *does* apply consistently to itself would have seemed to Carnap a clear advantage over other "interpretations" of mathematics — which, *lacking* significance within the framework they constitute, are forced back on a claim to some other, transcendental or absolute, status.

V.

The neutrality suggested by the syntactic interpretation does not presuppose any assumptions about the nature of language or of reality, e.g. the assumption that there "are" abstract objects or that there "is" or "must be" an analytic-synthetic distinction. The latter is rather a condition anyone concerned with the usefulness of a language in science would be inclined to impose on the choice of an interpretation. Without imposing this condition, Carnap thought, we will hardly be able to account for the central role of mathematics in empirical science, while also doing justice to the importance (stressed, for instance, by Einstein[14]) of separating statements internal to the mathematical framework from those that involve a physical interpretation of this framework.

But the imposition of any *particular* such constraint or condition is not required by the syntactic view. While the ability of the constraint in question (availability of an analytic-synthetic distinction) to account for the dual role of mathematics (as analytic and purely logical on the one hand, central to empirical science on the other) lends it a certain credence, Carnap never claimed this constraint, or even the syntactic approach as a whole, to be true, or correct. In fact he went to some lengths to

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[13] There is a kind of analogy in this respect between the *Syntax* approach to proving consistency and the self-applicability of the syntactic interpretation. As Goldfarb and Ricketts put it, "Is Carnap's position infected with a vicious circularity here? We think not. To be sure, there is a regress, but it is not obviously circular or vicious unless one thinks that some foundational work must be done by the syntactical description of a language. If no such task is at issue, then the upshot is simply that we can never make the conventional nature of mathematics fully explicit in any framework. The structure of Carnap's view is then coherent. Given the distinction between issues within a linguistic framework and issues between linguistic frameworks — a distinction that is always central to Carnap's thought — then the position is not circular so much as self-supporting at each level. If the mathematical part of a framework is analytic, then it's analytic; and so invoking mathematical truths at the level of the metalanguage is perfectly acceptable, since they flow from the adoption of the metalanguage." (Goldfarb and Ricketts 1992, p. 71)

[14] In this connection Carnap often referred to Einstein's paper "Geometrie und Erfahrung" which contains the famous passage: "Insofar as the sentences of mathematics refer to reality, they are not certain, and insofar as they are certain, they do not refer to reality . . . I place such a high value on this conception of geometry because without it, the discovery of the theory of relativity would have been impossible for me." (Einstein 1921, pp. 3-6)

deny any such claim. He argued that *all* interpretations of mathematics, in fact, should be regarded rather as *proposals* for a practical purpose: the construction of our scientific language.

This idea would have seemed unsatisfactory to Gödel, as it makes the selection among "interpretations" at least partly a practical matter; the interpretation of mathematics is no longer itself determined in the pure mountain air of mathematics, it cannot itself be a mathematical problem. But this is actually the lesson of Gödel's own incompleteness theorems, as the modification of his argument discussed at the end of section II above makes clear. For consistency is required not only by the syntactic interpretation, but by *any interpretation* of mathematics (or at least any interpretation for which an explication is proposed, since if the formal system proposed as its explication were inconsistent, that interpretation would admit any statement whatever as well as its negation). The modification of Gödel's argument discussed in section I above, therefore, shows that *no* interpretation (if it requires consistency), can be *proven* true.

Gödel might still have objected, as Beth (1963) did, that the syntactic interpretation is less "neutral" than it seems. The various proposed "interpretations" of mathematics can't after all, he might have said, be treated on an equal basis (as the principle of tolerance demands), since the standpoint adopted in the investigation of some languages presupposes linguistic resources that those who favor simpler languages (or other interpretations) rule out. But to rule out linguistic resources because they violate the truth of some interpretation of mathematics is exactly what Gödel's own argument, in the above modification, shows us to be pointless, since there can be no such truth. The response of the syntactic view is not to find some other ground for the truth of any particular interpretation, but to concede that there is no point in applying the predicates "true" and "false" to an "interpretation" or a "conception" of mathematics; an interpretation can only be more or less useful for some stated human purpose.

Gödel and Beth are right, as Carnap acknowledged (1963, pp. 872-873) to point out that there is a certain sense in which advocates of simpler languages fail to understand richer languages. They can manipulate the purely formal calculus of a richer language, of course, but are unable initially to give it a meaning. At this initial stage of a pragmatic comparison of languages, the principle of tolerance is admittedly not perfectly neutral. This is the case in any conversation where one side initially does not know the other's language. But anyone who wants to join in the pragmatic discussion about the relative merits of different languages must *learn* the other's language. This is always the case where two different cultures or intellectual frameworks confront each other; knowing the languages of both sides is the price of entering the discussion. It seems that the *refusal* to learn another's language can only be motivated by a prejudice, most likely a conviction that one's own favored proposal (e.g. for the interpretation of mathematics) is *true* or *correct*. What the above modification of Gödel's argument shows is that, since we cannot prove that an interpretation of mathematics is true, there can be no rational grounds for such a refusal to learn the other's language.

But, Gödel might finally have objected, what rational grounds are we left with for accepting our mathematics even provisionally if its consistency can't be *proved*? Isn't Carnap himself falling back, in the end, on our *experience* that classical mathematics has not *so far* shown itself inconsistent — and is he not thereby insidiously adopting an *empiricist* interpretation of mathematics? Whatever the objections to Plato, Gödel might have said, isn't Mill worse?[15]

But Carnap could have responded to this last objection — as well as to the previous arguments of Gödel and Beth — with a more fundamental point. The *specification* of a language, he could have said, is a *practical* task — which is to say an *external* one. To say that the specification or construction of a language *presupposes* a richer meta-language is to apply internal standards to an external question. The kind of presupposition invoked by Gödel, Beth, and others is *internal*. It is a *consequence* of the rules of a particular range of languages (whichever languages Gödel's second incompleteness theorem can be proved in). It is misapplied, therefore, to the *act of proposing* the very rules it results from. An act or process of proposing language rules can no more be said to have "presuppositions" than any other tool-making acts or processes. The act or process of making a law, for instance, may have *empirical* preconditions, among which may be the *normative* statements or standards that motivated the legislators to propose the law; but it is obviously a category mistake to say that the law *presupposes* these empirical conditions or normative standards in the way that one law may presuppose another *within* the overall system of legal reasoning. The

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[15] This objection to the above Carnapian line of argument was brought to our attention by Tom Ricketts.

proposal of neutrality among "interpretations", then, is itself *constitutive*; it has no internal presuppositions and can therefore not be "self-undermining".

And that, finally, is why the use of past experience to justify a provisional acceptance or use of classical mathematics is not *empiricist*: the decision to accept or use a language is a *practical*, i.e. an external, decision. External questions, unlike "cognitive" (mathematical or empirical) ones, cannot be answered with proofs or experimental evidence. But reason and experience can be *brought to bear* on them; Carnap certainly championed the use of reason and science in the practical sphere, and himself proposed a framework for normative reasoning (Carnap 1963, §32). When making the practical decision whether to use a certain mathematical language, our past experience with that language is clearly a relevant consideration. Though he never spelled it out in these words, Carnap conceived of the relation between practice and theory as *dialectical* (Stein 1992): the search for the first principles of knowledge is to be conducted *jointly* with promoting the good of human kind. The two projects are not, in the end, separable.

VI.

Gödel might still have objected that, despite such apparent "neutrality", his own position was not accommodated; the so-called principle of "tolerance" turns out not really to be very tolerant after all. Carnap would have conceded, as we have just seen, that it is not *completely* neutral. And moreover, there is an important sense in which such issues, for Carnap, cannot be argued about. In his terms, these are not *cognitive* issues; they are not *internal* questions. They are *external* questions, and thus necessarily pragmatic, not cognitive. They amount, in the end, perhaps, to questions of values.

This is not to say that questions like this (regarding the "neutrality" of the principle of tolerance) cannot be discussed at all, but Carnap was reluctant to do so because he thought our language for dealing with these questions extremely vague and unsatisfactory. Still, he was not averse to attempting improvements (see, again, Carnap 1963, §32); in the spirit of clarifying the explicandum, he might if prodded have said something like this: The sense in which his view is genuinely more neutral than others (despite the fact that it disqualifies certain other views) is its adoption of neutrality *itself* as the governing value to be optimized. Of course some people may reject that value, and to them, obviously, the principle of neutrality is *not* neutral. But this is a common problem, in many practical and scientific situations. Democratic and open societies, for instance, cannot be *completely* democratic and open; they have to limit the rights of those who reject democracy and openness itself. Science itself cannot be perfectly neutral or accommodating, as it must exclude or ignore those who refuse to recognize the evidence accepted by the scientific community.[16] In practical terms, then, *absolute* or *complete* neutrality is impossible. But the proposed view that makes it an explicit goal to *maximize* neutrality may have a claim to be more neutral than views maximizing other values.

Of course this informal discussion leaves open the question how to explicate the vague term "neutrality". Carnap might (or could, at least) have explicated "neutrality" in terms of formal (or semantic) "explicitness", or some similar concept, as follows: neutrality would be regarded as maximized in a framework of discussion that allowed various views (proposed languages) to state their assumptions and their terms (including categorial terms) as clearly and unambiguously as possible, in a way that both (a) their own users regarded as adequate, and (b) they are directly comparable[17]; but excluding all normative or optative components of those views. (Not that these normative considerations should not be discussed; on the contrary, Carnap regarded them as very important. But he did not think they could be adequately addressed until the contending views (languages) themselves are clearly and completely (i.e. explicitly) identified so that their consequences can be worked out and assessed.)

VII.

Returning to the above pseudo-argument about spacetime, assume the truth of (ii′), i.e. suppose we cannot determine whether curvature exists. Then we have two choices. Either "spacetime is flat"

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[16] Carnap had made this point as early as 1932 (Carnap 1932, pp. 179-181); see also Reisch 1991.

[17] We assume, though neither Carnap nor anyone else, as far as we know, attempted to show this formally, that (a) and (b) are to some degree incompatible, and must be traded off against each other in any given framework; more of one means less of the other. For this reason philosophers sometimes claim (e.g. McKeon 19XX) that philosophical views are, and must always remain, fundamentally irreconcileable with each other. But this is simply to insist on a framework that optimizes (a) alone, while omitting (b); there is no a priori reason for choosing such a framework over one that sacrifices some (a) for some gain in (b).

becomes cognitively meaningless (as no facts can be brought to bear on it) or it becomes analytic (i.e. a definition). In the latter case, we *propose to call* spacetime "flat", as a matter of convention or stipulation, just as one might define the notion of being "at rest" relative to the position of the earth for the purpose of describing the motions of the heavenly bodies. In the syntactic view, such a stipulation amounts to identifying mathematical truth with truth in a particular, specified language.

A realist like Gödel would have rejected the idea that the flatness of spacetime is a pseudo-question. For him, there is a true curvature of spacetime, however unknowable it may be now or in principle. In mathematics, too, the realist would hold the notion of truth to be absolute, and not dependent on a particular choice of linguistic framework. Realism no more permits mathematical truth to be dependent on our linguistic conventions than it permits spacetime curvature to depend on our physical conventions.

But Gödel did not *refute* the syntactic program, as he claimed. What Carnap would have emphasized in his reply is that the syntactic view was never *asserted* to be *true*; it was a *proposal* for how to see the relation between mathematics and its applications. The merits of such a proposal can, of course, be discussed. But the question whether we should adopt this proposal is a *practical* question; the proposal is not the kind of sentence to which the concepts "proof" or "disproof" apply.[18] This is not to say that no questions *relevant* to the merits of such a proposal can be proved; Gödel's incompleteness results are an obvious example. No interpretation of mathematics that fails to take these results into account deserves to be taken seriously. And as pointed out in section V, our historical experience with mathematics is also relevant to the question whether to adopt a proposal like the syntactic program. But such empirical and rational considerations — even Gödel's theorems, for all their importance — do not *by themselves* uniquely determine an interpretation of mathematics.

Carnap would also have wanted to point out that there can be proposals at different levels. The "syntactic interpretation of mathematics" was a very general proposal, for instance; supporting it, in *Logical Syntax*, were many specific proposals for explicating the concepts used to specify and

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[18] This is a form of the traditional distinction between "is" and "ought", for which Carnap argued in section 32 of his replies (Carnap 1963, esp. pp. 999-1005).

constrain the broader proposal (e.g. the concept "analytic"). And the rejection of any particular proposal for explicating a subordinate concept does not (in any ordinary logical sense) *entail* the rejection of the broader proposal supported by that concept.[19]

This is the case also for Carnap's pragmatic proposal that in building frameworks for the overall study and assessment of theories and languages, we maximize neutrality and separate values from logic. Obviously these concepts are vague, and require explication. But the unsuitability of any particular explication (of "neutrality", for instance, or of "logic", or of the distinction between values and logic) does not by itself undermine the proposal that we be as neutral and inclusive as possible in our assessment of languages and forms of expression. This proposal cannot be disproved, it can only be found attractive or unattractive, useful or useless, elegant or clumsy, by those who elect to use it.

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[19] Carnap seems to have thought that this may perhaps be possible someday, when a more precise, formal language has been developed and accepted for the discussion of such pragmatic questions. He conceded, though, that such a development could only be viewed as a distant utopia.

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