Carnap and the Epistemology of Mathematics

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Abstract

Carnap held that all mathematical statements are either analytic or contradictory, whereas claims about the empirical world are synthetic. Why is the analyticity of mathematics important to him? A straightforward and historically influential answer goes as follows: Carnap is a linguistic conventionalist, and the notion of analyticity is supposed to explain how a priori knowledge of mathematical truths is possible by means of linguistic rules. In recent scholarship, however, this conventionalist reading has fallen out of fashion. Instead, Carnap is seen as rejecting traditional philosophy of mathematics altogether, including the very idea that mathematical knowledge is a priori. Against this trend, I draw on a previously unpublished exchange between Carnap and Irving M. Copi to defend a more traditional interpretation. I show that there is good reason to think that Carnap indeed wanted to account for the a priority of mathematics by drawing on the same linguistic rules he uses to characterise analyticity. I furthermore argue that this result casts some new light on one of the most contentious aspects of Carnap's position: his reliance on infinitary rules. The paper is followed by a full transcription of the Carnap-Copi exchange.

1 Introduction

Mathematical discourse is often regarded as distinct from discourse about the empirical world along a number of dimensions. One such dimension concerns the *nature of truth* in the respective domains. Some think that mathematical truths are *analytic*, i.e. true in virtue of meaning, whereas empirical truths are synthetic, i.e. true in virtue of worldly facts. Another dimension concerns the *source of justification*. Some hold that our knowledge of mathematics is *a priori*, i.e. independent of observation, whereas empirical knowledge is *a posteriori*, i.e. based on observations.[1] Interesting questions can be asked about how these cat-

[1] A third dimension which, apart from a brief episode around footnote 11, we will not consider in this paper, concerns the *modal status* of mathematics. Many hold that mathematical truths are *necessarily* true while empirical truths are merely *contingent*.

egories interact. Are the analytic and the a priori coextensive? Kant famously denied this, since he thought that mathematical truths are synthetic. Frege objected to Kant, and tried to show that at least *arithmetic* is analytic after all. Even supposing this dispute about the respective extensions to be settled, however, there is still a further question about the relationship between the two dimensions: Is mathematics knowable a priori *because* it is analytic? Or are these two phenomena independent of each other?

It is well known that Carnap followed Frege in endorsing the analyticity of mathematics, contra the iconoclast Quine, who thought the philosophical importance of the analytic/synthetic distinction overrated. But what is Carnap's view on the epistemology of mathematics? Compared to the notion of analyticity, this question has received relatively little discussion. And this is not too surprising, for in his mature work Carnap rarely talks about epistemological matters – in general, that is, and virtually never with regard to mathematics. We can gather that he is not opposed to a priori knowledge in principle (Carnap 1963: 978), that he came to reject the synthetic a priori (Carnap 1963: 956), and that he sees his approach as continuous with Frege's (Carnap 1963: 928). All this suggests an affirmative answer to the extensional question of whether the analytic coincides with the a priori. We are in the dark, however, about Carnap's attitude towards the explanatory question of whether a priori knowledge in mathematics is possible *because of* its analyticity.

In this paper I will give textual support in favour of ascribing an *explanatory thesis* to Carnap: for him, the analyticity of mathematics explains why and how a priori knowledge is possible. I will do so by drawing on a previously unpublished exchange between Carnap and Irving M. Copi. In 1949, the *Journal of Philosophy* published a note by Copi called "Modern Logic and the Synthetic A Priori" (Copi 1949), in which he argued that undecidable mathematical statements are a priori but not analytic. Even though Carnap is not mentioned by name in Copi's article – which I will call *MLSA* – his position on the analyticity of mathematics, put forward in works such as *The Logical Syntax of Language* (Carnap 1937), was clearly a target of Copi's attack. Carnap thus defended his own position in a letter to Copi not long after *MLSA* appeared. After some delay Copi sent a lengthy response, to which Carnap responded again.[2] The Carnap-Copi correspondence has been preserved in the Rudolf Carnap Archive at the University of Pittsburgh, but, to my knowledge, scholars have not made use of

[2] Rudolf Carnap Papers, Archives of Scientific Philosophy, Hillman Library, University of Pittsburgh (*RCP*): 088-18-08, -07, 027-03-10.

it so far. Carnap clearly regarded the exchange with Copi as a valuable source of information himself, for in 1961 – over ten years later – he sent a copy of the letters to Robert W. Beard from Florida State University, who had inquired about Carnap's attitude towards undecidable sentences (*RCP*: 027-02-34, -33). As I will show, the letters not only shed light on Carnap's views on the epistemology of mathematics, but also on another contested component of his position: the role of infinitary methods in defining analyticity for mathematical languages.

The paper proceeds as follows. In the next section, we will see that widely differing readings of Carnap's views on the connection between the notion of analyticity and the epistemology of mathematics have been proposed. Section 3 presents Copi's argument against the analyticity of mathematics from *MLSA*, which draws on Gödel's incompleteness results. The depth of Copi's challenge is illustrated by means of a public but forgotten exchange that took place in the *Journal of Philosophy* between Copi and Atwell R. Turquette, in which a number of different possible attitudes towards undecidable sentences are considered. Section 4 then discusses the previously unpublished private exchange between Carnap and Copi. Section 5 concludes by arguing that the letters support the view that Carnap held an explanatory thesis according to which the analyticity of mathematics accounts for its knowability a priori. Furthermore, I raise a challenge for those who reject the explanatory reading. A transcription of the letters follows.

2 Analyticity and Epistemology

2.1 Conventionalism and the A Priori

Giving a concise summary of Carnap's philosophy of mathematics is difficult since its interpretation has been so controversial. It is uncontentious that the notion of analyticity plays *some* important role in Carnap's account, for he goes to great length to construct definitions that make all purely mathematical statements either analytic (true) or contradictory (false). But *why* did Carnap see the need to draw this sharp distinction between empirical truth – which is synthetic – and truth in logic and mathematics?

According to a reading that was (and still is) widespread, Carnap is a *conventionalist* about mathematical truth (Putnam 1979, Potter 2000: chapter 11, Warren 2020: chapter 13). The reason he wants mathematics to come out as analytic, on this interpretation, is that he considers mathematical statements to be true *in* *virtue of* linguistic rules whose adoption is a purely conventional matter. The popularity of this interpretation is not too surprising, since it seems to capture the core idea of Carnap's influential "Empiricism, Semantics, and Ontology": that the acceptance of mathematical entities is not a problem for empiricists, because – thanks to their analytic character – all statements about mathematical objects are entailed by linguistic rules (Carnap 1956).

Beginning in the 1980s, however, a new wave of scholarship has challenged the traditional narrative.[3] Against the conventionalist reading, it has been stressed that Carnap's metaphilosophy leaves no room for a substantial distinction between truth in virtue of the way the world is and truth in virtue of linguistic conventions (Goldfarb and Ricketts 1992: 65). In a slogan: Carnap has no "thick notion of truth-in-virtue-of" (Ricketts 2007: 211). These anti-conventionalist arguments typically construe linguistic conventionalism as a primarily *metaphysical thesis* about the truthmakers of mathematical statements. But linguistic conventionalism can also be understood as an *epistemological* thesis according to which linguistic rules are what makes a priori knowledge of mathematics possible. For the purposes of this paper, I will focus on the question of whether Carnap is a conventionalist in the epistemological sense.

There is certainly a tradition of interpreting Carnap in this way. Quine, for instance, describes the linguistic doctrine of logical truth he ascribes to Carnap, as an "epistemological doctrine" (Quine 1960: 353). And Putnam endorses such a reading as well:

> Now, the thesis that every theorem of mathematics is either true by convention [...] or else a *consequence* of statements that are true by convention has often been advanced as an *epistemologically explanatory* thesis (e.g., by [...] Carnap in the *Foundations of Logic and Mathematics*) [...] (Putnam 1979: 424)

Somehow, so the idea, the analyticity of mathematics is supposed to give us a plausible epistemology for mathematics. In order to assess whether Carnap actually held this view, it will be helpful to distinguish between two different theses about the relationship between the analytic and the a priori:

> THE EXTENSIONAL THESIS
> A statement is knowable a priori *if and only if* it is analytic.

[3] Some of the key works are Friedman 1999a, Creath 1990, Goldfarb and Ricketts 1992, Ricketts 1994, Friedman 1999b, Ebbs 1997, Awodey and Carus 2004, and Lavers 2008.

The Explanatory Thesis
A statement is knowable a priori *because* it is analytic.

The explanatory thesis is meant to be stronger than the extensional thesis. The former says that the analytic and the a priori coincide, but does not say that the analyticity of a statement is what explains why and how this a priori knowledge is possible. There might be a different explanation, or none at all. The explanatory thesis adds precisely this claim about the explanatory relations between the two notions.[4]

2.2 Two Theses, Two Takes

Did Carnap accept these two theses? It is difficult to tell, since he rarely ever addresses the notion of a priority in his publications on the philosophy of mathematics. And this silence is indicative of a sceptical turn in Carnap's attitude towards epistemological matters from the mid-1930s on. In the short paper "Von der Erkenntnistheorie zur Wissenschaftslogik" ("From Epistemology to the Logic of Science"), Carnap describes epistemology as an "unclear mixture of logical and psychological components" (Carnap 1936: 36, my translation), and recommends to abandon traditional epistemological questions in favour of studying the logic of science.[5]

The textual basis thus makes it difficult to reach firm verdicts about Carnap's position on epistemological matters. Nevertheless, attributing the extensional thesis to him should not prove too contentious. Carnap mentions the a priori a couple of times in his "Replies and Systematic Expositions" of the Schilpp-volume. Those remarks make clear that (1) he does in general believe that a priori knowledge is possible (Carnap 1963: 978), and (2) he does not believe in a priori knowledge that is synthetic (Carnap 1963: 956). Furthermore, all the examples of a priori knowledge Carnap mentions do concern analytic statements. It is thus reasonable to conjecture that he took the realm of a priori knowledge to coincide with what is analytic.

The explanatory thesis, on the other hand, is much more controversial. If Carnap had accepted it, one would expect there to be some explicitly epistemological remarks in contexts where he discusses the notion of analyticity. But

[4] Once can deny the extensional thesis and accept the explanatory thesis by holding that there are some a priori claims that are analytic, and which are knowable a priori *because* of their analyticity, but that there are also synthetic a priori claims. This may indeed be Kant's view, but I will ignore it here since Carnap does not accept the synthetic a priori.

[5] See Richardson 1996 and Uebel 2018 for a more general discussion of Carnap's attitude towards epistemology.

there are none to be found. Despite the absence of direct evidence for it, the explanatory thesis has been attributed to Carnap by Richard Creath:

> This discussion of pragmatic usefulness and explication must not obscure, however, the epistemic core of Carnap's doctrine. [... Carnap's] system provides an epistemology for mathematics which accords well with our ordinary convictions about how to justify mathematical claims: the justification of theorems involves deriving them from axioms; their justification is independent of experience and not subject to experimental disconfirmation; and the theorems are objectively true. (Creath 1992: 145, 149)

Creath admits himself, however, that the textual basis for this interpretation is not exactly clear-cut. He writes that in "scattered passages throughout his writing" Carnap "conceded [his work's] epistemic character" (Creath 1992: 163n2), but does not cite any specific such passage. Some support for Creath's reading may be drawn from Carnap's most detailed work on the philosophy of mathematics: *The Logical Syntax of Language*. One of the few occurrences of "a priori" in this book is the following:

> See Wittgenstein on this point ([*Tractatus*] p. 172): "All propositions such as the law of causation, the law of continuity in nature,. . . are *a priori* intuitions of the possible forms of the propositions of science." (Instead of "*a priori* intuitions of " we would prefer to say: "conventions concerning".) (Carnap 1937: 307)

Carnap also recommends to replace talk about a priori intuition by formulations concerning syntactical rules (Carnap 1937: 306). It is possible to interpret these passages in a way that supports Creath's epistemological interpretation. One can take Carnap's preference for talking about syntactical conventions over a prioriness to mean that syntactical rules are supposed to provide an *account* of what a priori knowledge really amounts to, thus providing an explanation of its possibility. But this reading, far from being forced upon us by the extant passages, requires some creative exegesis.

It is therefore not surprising that others reject the epistemological reading. Creath himself mentions Burton Dreben's dissent (Creath 1992: 164n3). Ricketts, a scholar in this tradition, thinks that ascribing the explanatory thesis to Carnap aligns him too closely to traditional philosophy of mathematics:

> Carnap thus does not present in *Logical Syntax* an account of the nature of mathematics, of our knowledge of mathematics, and of the applications of mathematics in empirical science comparable to the accounts developed by Kant, Mill, Frege, Wittgenstein, and Hilbert. Carnap rejects the questions these thinkers address. In a sense, he gives up philosophy of mathematics. (Ricketts 2007: 211f)

And, from a different angle, Michael Friedman also opposes a close connection between epistemology and Carnapan analyticity:

> [...] [The point of Carnap's conception of analyticity] was not to explain the special kind of epistemic security possessed by these disciplines, but rather to emphasize our complete *freedom of choice* concerning which rules of logic and mathematics to adopt. (Friedman 2018: 142)

Which side is right? Due to the sparseness of textual evidence, so far the only way to assess the plausibility of ascribing the explanatory epistemological thesis to Carnap is to rely on big picture considerations about whether it fits his general approach to philosophy. Naturally those are bound to be highly contentious in themselves, and so it is difficult to make progress. More explicit remarks by Carnap on the epistemology of mathematics would be very helpful to adjudicate this debate. Luckily such remarks can be found in the aforementioned letters to Copi, and so in the next section I will introduce the article that prompted Carnap to clarify his position.

3 Copi and Turquette on the Synthetic A Priori

3.1 The Original Article

Copi's *MLSA* deals with the philosophical significance of Gödel's incompleteness results. Copi's conclusion is that Gödel's limitative results make the extensional thesis – that sentences are analytic if and only if they are a priori – untenable. Copi characterises an analytic statement as one whose truth "follows from the syntactical or grammatical rules governing the language in which it is expressed" (Copi 1949: 243). His argument that, in mathematics, there must be a priori statements that are synthetic can be summed up as follows:

(1) It follows from Gödel's incompleteness theorems that, for every sufficiently strong mathematical theory, some purely mathematical statements are independent of that theory.

(2) Such undecidable sentences are therefore not analytic by the definition above.

(3) Nevertheless, undecidable sentences have truth values and can be known a priori.

(4) Undecidable mathematical sentences are therefore a priori and synthetic.

As Copi is well aware, there are several ways to reject this argument. He helpfully distinguishes between four strategies:

(a) Deny Gödel's result.

(b) Amend the definition of analyticity.

(c) Deny that undecidable statements are a priori.

(d) Hold that undecidable sentences are meaningless.

Copi takes strategy (a) to be a non-starter, a verdict with which we can agree. Option (b) is the one Carnap endorses in his letters. Before we look at those in more detail, however, it will be useful to also consider options (c) and (d). For one thing, neither of them can be dismissed as obviously wrong or misguided in the way (a) is. Secondly, in a published reply to Copi, Atwelle R. Turquette made the case for preferring (c) and, more emphatically, (d), over Copi's own endorsement of synthetic a priori truths. Thirdly, once we come back to discussing the nature of Carnap's own views on the epistemology of mathematics, it will be helpful to contrast different readings of his position with the alternatives represented by (c) and (d).

The public exchange between Copi and Turquette is not well known. While Copi himself is still remembered, for instance for his clear and well-written introduction to the theory of types, Turquette has vanished into obscurity. It is fair to say that his response to Copi is a mixed bag. Some of his objections are needlessly uncharitable to Copi's intentions, others are obscure, such as his complaint about the "wisdom of associating a highly refined logical language as is used in Gödel's theorems with a philosophical language which is as richly colored with historical meaning as the phrase 'synthetic *a priori*'" (Turquette 1950: 128).[6] He

[6] To this Copi responds, quite reasonably, that the question of whether mathematics is synthetic a priori has been on the philosophical agenda since, at least, Kant, maybe even Plato (Copi 1950: 633).

does, however, give some arguments in favour of considering undecidable sentences as either a posteriori justified or being meaningless that are worth taking seriously. The latter one is especially interesting since it relies on then-new and unpublished work by Leon Henkin on non-standard models, a theme that was taken up years letter in a critical article on Carnap's philosophy by E. W. Beth (Beth 1963). In what follows I will discuss (c) and (d) in turn.

3.2 The Empirical Strategy

Why think that undecidable mathematical statements are a priori rather than a posteriori? In *MLSA*, Copi briefly considers the latter option but quickly dismisses it. His argument depends on one particularly interesting class of undecidable statements: consistency sentences. Using the method of Gödelisation, claims about syntax, such as that no contradiction is derivable from a certain set of axioms and inference rules, can be expressed in an arithmetical language. Gödel's second incompleteness theorem shows that, for any sufficiently strong mathematical theory T, the consistency sentence Con_T is independent of T. Peano arithmetic (PA) is one example of an incomplete mathematical theory. If undecidable sentences were synthetic, the claim that PA is consistent – encoded by Con_{PA} – would thus be synthetic as well.

Copi thinks that this result is implausible. He writes that "the consistency of a set of postulates is clearly not something that could be established inductively or empirically" (Copi 1949: 245). But how clear is this, really? Take the case of PA. We think (and hope) that PA is indeed consistent, i.e. that it is impossible to derive a contradiction such as "1 = 2" from its axioms. What, if anything, justifies this belief? One reason that can be cited is that PA has been widely used and studied in the past decades, and that, as a matter of fact, no contradictions have been found. And this seems to be just the kind of inductive justification for claims about consistency that Copi deems to be impossible.

In his response, Turquette makes the same point by noting that the actual practice of mathematicians seems to fit the inductive model of justifying consistency claims (Turquette 1950: 127). He approvingly cites a paper by Nicolas Bourbaki, in which they write the following:

> Absence of contradiction, in mathematics as a whole or in any given branch of it, thus appears as an empirical fact, rather than as a metaphysical principle. The more a given branch has been developed, the less likely it becomes that contradictions may be met with in its

> further development. (Bourbaki 1950: 3)[7]

These considerations make Copi's hostility towards allowing inductive evidence in favour of consistency claims difficult to defend. The situation would be clearer if Copi had a positive account of *how* the a priori justification of undecidable sentences proceeds. But his paper is not that ambitious. While he takes himself to have refuted the analytic theory of a priori knowledge, he does not propose to put an alternative account in its place (yet):

> It is not my claim to have answered the question "How is *a priori* knowledge possible?" [. . .] the question of how *a priori* knowledge is possible remains one of the most urgent problems confronting philosophers today. (Copi 1949: 245)

Without a positive story in place, however, it is difficult to see how we can be so sure that the a priori justification of undecidable mathematical statements is possible at all.

In a rejoinder to Turquette's piece, Copi doubles down on his original position by giving an additional argument against the a posteriority of undecidable sentences. It is a slippery slope argument according to which Turquette's endorsement of option (c) commits him to the claim that there is *no* a priori knowledge of mathematical statements at all:

> [Turquette] might as well have quoted John Stuart Mill, who regarded *all* mathematical truths as empirical and inductive. For by the Gödel correspondence between systems of logic and the arithmetic of integers, Gödel's undecidable formula is a statement of pure mathematics, and no more nor any less empirical than the equation "7 + 5 = 12". To claim that the truths of logic and mathematics are empirical seems to be a stroke of desperation, for that thesis now long dead and buried, should be allowed to rest in peace. (Copi 1950: 634)

This objection, however, is unconvincing. Copi is of course right to say that consistency statements such as Con_{PA} are arithmetical statements. But his slippery slope argument presupposes that anyone who holds *some* arithmetical sentences

7 There is, to be a fair, a way to accommodate the point about a posteriori justification. While Copi's formulation suggests that he rejects *any* sense in which claims about consistency can be justified empirically, a more conciliatory position would maintain that they can be justified *both* a priori and a posteriori.

to be a posteriori – such as Con_{PA} – must classify *all* other arithmetical statements – such as "7 + 5 = 12" – as a posteriori too. But where's the argument for that? What excludes a view according to which there is both a priori and a posteriori justification in the realm of arithmetic? Copi does not tell us, and so this new argument is really just a re-assertion of his original view.

Ultimately, Copi's hope that empiricism about logic and mathematics should rest in peace did not come true. Quine rejected the analytic theory of the a priori in a more forceful way than Copi, and embraced empiricism in a more enthusiastic manner than Turquette, by denying that there is any philosophical need for the analytic/synthetic distinction at all.[8] We cannot go further into Quine's position here, but will briefly contrast it with Carnap's later on. For now let us move on to the second strategy Turquette considers: option (d), according to which undecidable sentences are meaningless.

3.3 The Meaninglessness Strategy

On first inspection, the strategy of characterising undecidable sentences as "grammatically correct formulas [. . .] which are nonsensical otherwise" is bound to look unappealing. Why does Turquette nevertheless consider this option to be "more reasonable [. . .] than to assign to them the kind of meaning which is usually associated with synthetic *a priori* truths" (Turquette 1950: 129)? In order to understand his thinking, we need to look into the most interesting part of his paper, in which he draws on results from Leon Henkin's then-recent dissertation on "The Completeness of Formal Systems".

Henkin had shown how to construct so-called *non-standard* models for formal mathematical theories. To stick with one concrete example, the natural numbers are the standard model for PA. Relative to this model the undecidable sentence Con_{PA} comes out as true. This is because Con_{PA} is a universal generalisation of the following form:

$$Con_{PA} =_{def} \forall x \neg Pr_{PA}(x, \ulcorner \bot \urcorner)$$

Furthermore, its instances $\neg Pr_{PA}(0, \ulcorner \bot \urcorner)$, $\neg Pr_{PA}(1, \ulcorner \bot \urcorner)$, $\neg Pr_{PA}(2, \ulcorner \bot \urcorner)$, ..., are provable in PA. Since the natural numbers are all the elements of the standard model, the generalisation must be true as well. Henkin showed that it is nevertheless possible to construct models which make the consistency sentence

[8] In a sense Quine does take mathematical truth to be empirical, but note that the position is very different from Mill's view according to which mathematical statements are akin to empirical generalisations (Burgess 2013: 284f).

Con_{PA} *false*. These models are non-standard in the sense that they contain more elements than just the natural numbers, even though they still make all the axioms of PA true.

In his paper Turquette draws on Henkin's observation in order to criticise Copi's view that undecidable statements are synthetic a priori. With some charitable exegesis two lines of argument can be distinguished: one of them epistemological, the other meaning-theoretic. The epistemological argument goes as follows. One presupposition Copi makes in *MLSA* is that undecidable statements have determinate truth values. In light of the results of Gödel and Henkin, however, this assumption is not innocent. Con_{PA}, for instance, is true in some models and false in others. Which, if any, is *the* truth value of this statement? Turquette points out that classifying such statements synthetic a priori in itself is of no help in answering this epistemological question:

> We then would ask just what is this truth? [...] The point is that we *do not know*, and what philosophical value is to be found in a synthetic *a priori* truth which we do not know and in fact from the results of Gödel never could know? This indeed would be a strange synthetic *a priori* truth. It would bee as useless as a Kantian *Ding-an-sich* and as indefinite as any contingency! (Turquette 1950: 128)

Turquette's second, meaning-theoretic argument then tries to turn the apparent *ignorance* of the truth values of undecidable statements into a *rejection* of determinate truth values for such statements. This is not spelled out in much detail, but the thought seems to be as follows: Since the truth values of undecidable statements are not determined syntactically, and since we also don't want to treat them like empirical truths, the natural conclusion is that they have *no* truth value at all. Turquette then implicitly identifies lacking a determinate truth value with lacking meaning. In this way he arrives at the conclusion that undecidable statements have the status of "grammatically correct formulas which are nonsensical otherwise" (Turquette 1950: 129).

In his rejoinder to Turquette, Copi criticises this argument, and his response is much stronger than that to the empirical option (c). Copi's counter-argument is based on the principle of compositionally: "the meanings of combinations of symbols are determined by the meanings of their constituent symbols and their mode of combination" (Copi 1950: 635). We already saw that Con_{PA} is the generalisation $\forall x \neg Pr_{PA}(x, \ulcorner \bot \urcorner)$, instances of which such as $\neg Pr_{PA}(0, \ulcorner \bot \urcorner)$ are true and hence meaningful. For this reason Turquette would have to deny the following principle:

> If a sentence of the form $\phi(a)$ is meaningful, then so is the generalisation $\forall x \phi(x)$, where a and x occur in the same positions.

But, as Copi points out, this is difficult to pull off. Presumably Turquette doesn't want to deny that universal generalisations are meaningful *in general*, but only in cases where they are undecidable. In order to formally capture this idea, however, one would need an effective procedure to determine whether a formula is decidable or not. But there is no such procedure, and so Turquette's proposal has the consequence that we cannot decide whether a given formula is meaningful or not in an effective way. Arguably Copi is right in thinking that this is an untenable consequence.

Nevertheless, Copi's reply does not defuse all challenges to his position. While the conclusion Turquette drew from the phenomenon of non-standard models might have been implausible, the questions he raises are good ones. For undecidable sentences, what justifies the assumption that they have determinate truth values at all? What determines which truth value it is? And how are we able to find out about this truth value?[9] As we already saw, it was not Copi's aim to give a positive account that answers all of these questions. But a fully developed philosophy of mathematics will need one eventually. One important question is thus whether Carnap, to whose own views on the matter of undecidable sentences we will turn now, can be read as proposing a way to answer these questions based on the notion of analyticity.

4 The Correspondence with Carnap

4.1 Carnap's Letter

Carnap's first letter to Copi on the issue of analyticity is from August 1949, and thus predates the paper by Turquette. Contrary to the argument of *MLSA*, Carnap defends the view that mathematics, including undecidable sentences, is

[9] It is worth pointing out that, in a review of Carnap's *The Syntax of Language*, Stephen Kleene took Carnap to be committed a position similar to Turquette's option (d):

> If logic and mathematics are taken as wholly formal, one apparently must reject the conception, that a sentence S of classical mathematics is necessarily either true or false in such a sense that the problem, to determine which is the case, belongs to logic and mathematics when S (or "S is analytic") is irresoluble in terms of d-rules already stated. (Kleene 1939: 85)

By "d-rules" Kleene means the ordinary axioms and inference rules of a formal calculus, excluding Carnap's infinitary rules that we will encounter in the upcoming section.

analytic rather than synthetic. His approach is to endorse option (b): amending the definition of analyticity. In order to see how, we need to distinguish between two phases in Carnap's development: the syntactic and the semantic phase.

During the earlier syntactic phase, Carnap was opposed to semantic notions such as truth and reference, and thus aimed to define all logical concepts by means of syntactic rules. During this period he would thus have been willing to accept Copi's characterisation, according to which the truth of analytic statements "follows from the syntactical or grammatical rules governing the language in which it is expressed" (Copi 1949: 243). But, as he stresses in the letter, even in *Logical Syntax* he understood this idea in a wider sense than Copi does. According to Carnap, the latter relies on a too narrow notion of what it means for a statement to follow from syntactical rules. Gödel's incompleteness results show that no *recursively* axiomatisable theory with *decidable* inference rules is complete. But in *Logical Syntax* Carnap actually defines analyticity for the mathematical languages under discussion by means of *infinitary* rules. For the so-called Language I, which is a version of primitive recursive arithmetic, the relevant rule is the infinitary ω-rule (Carnap 1937: 38, 173):

ω-Rule

$$\frac{\phi(0), \phi(1), \phi(2), \dots}{\forall x \phi(x)}$$

Adding the ω-Rule to primitive recursive arithmetic results in a complete theory, for Gödel's incompleteness theorems have no application. From this perspective there is thus no need to deny that undecidable mathematical sentences are analytic as well.

The same conclusion also holds for Carnap's position during his later semantic phase. In the letter to Copi he writes that he would now define analyticity as a semantic rather than a syntactic concept, which means that undecidable sentences are "still based on linguistic rules, but not on syntactical ones". The idea behind this move is that, for Carnap, the specification of a *domain of quantification* for a formal language counts as a linguistic rule:

> [...] in a *rule of values* related to the rules of designation, it is stated for each kind of variable which entities are to be *values* of the variables of that kind. Their class is sometimes called the *range of values* of the variables in question. (Carnap 1942: 44, see also Frost-Arnold 2013: 76)

For mathematical languages, the intended interpretation specified by these linguistic rules might for instance be the natural numbers (Carnap 1937: 106). On this conception, analyticity for an arithmetical language can therefore be equated with the semantic notion of truth in the standard model (Coffa 1987, Koellner 2009). Carnap takes this to show that undecidable sentences, while not being derivable from the recursive rules of a theory, a nevertheless true in virtue of linguistic rules:

> The decisive point which you seem to overlook is this: it follows from Gödel's consideration that his undecidable sentence is true, and, moreover, L-true [= analytic]. In other words, Gödel has actually shown, on the basis of linguistic rules, (though not on the basis of the syntactical rules of the given calculus), that the sentence is true. Thus the sentence is known to be analytic.

While, on the surface, the semantic approach looks quite different from the syntactic method of using the infinitary ω-rule, the approaches share important similarities. In both cases the rules Carnap relies on to characterise analyticity can be described as non-recursive. In the syntactic case this is because the ω-rule has infinitely many premises. In the semantic case this is because the notion of truth in the standard model of arithmetic is not recursive: there is no way to effectively enumerate all the mathematical truths. Both early and late, Carnap's key move is thus to embrace non-recursive methods.

4.2 Copi's Response

After several months Copi sent Carnap a detailed response to his criticisms. He begins by listing the points of *agreement* between them. First, they both agree that the Gödel sentence is true.[10] Secondly, they both agree that it is a priori. Thirdly, they agree that it is not derivable using the (recursive) rules and axioms of the theory it occurs in. Copi thus conjectures that the remaining disagreement – whether to call the Gödel sentence a synthetic or an analytic a priori truth – is largely terminological. Nevertheless, he thinks that there are reasons to prefer the terminology he recommended in *MLSA*. Three kinds of considerations are distinguished: (I) classificatory reasons, (II) historical reasons, (III) philosophical reasons.

[10] While they do not say so explicitly, by *the* Gödel sentence they presumably mean the sentence that is usually glossed as "I am not provable".

As to (I), Copi maintains that, in order to clearly distinguish philosophical doctrines from linguistic proposals, *analytic* and *a priori* need to have different definitions. He illustrates this point by means of the "thesis of empiricism": that there are no synthetic a priori statements. If this is to be a thesis properly speaking – i.e. a non-trivial claim that could be either true or false – then analyticity and a priority need to be different concepts. This, in addition, is also how these notions have been traditionally understood. Copi reads Carnap as holding that these two concepts are identical, thus turning the thesis of empiricism into a linguistic proposal. While this is not impermissible as such, Copi thinks that it makes the theoretical situation less clear rather than more.

As to (II), Copi argues that traditional usage favours associating analyticity with what follows from certain rules of an effective and finitary nature, rather than rules in Carnap's wider infinitary sense. The strongest considerations marshalled in favour of this position rely on the idea that logical inferences are a matter of *logical form*. While the inference from "this book is blue" to "this book is coloured" seems valid in an intuitive sense, the validity is not due to the mere form of the sentences but rather due to the concepts involved. Logical inferences properly speaking, so Copi, are those whose validity is apparent due to the form of the statements involved alone, such as the inference form "A and B" to "A".

Copi argues that this traditional way of thinking about the nature of logic speaks against Carnap's liberal conception of analyticity which allows for infinitary rules. This is, roughly, because we only have a good grasp of whether a certain inference is valid in virtue of its form if it is an inference that can actually be drawn. It must, in other words, be possible to encounter the premises and the conclusion somehow, for instance as written down on paper. But, so Copi, "effectiveness or finiteness is of the essence here", since this requirement is obviously not met by infinitary rules like the ω-rule. Thus Carnap would have to deny that his notion of analyticity is continuous with the traditional conception of logical truth. And while, once again, this is not impermissible, Copi thinks that the advantages of this move are spurious compared to the potential confusions that it brings.

As to (III), Copi holds that while some a priori claims of logic and mathematics are clearly a linguistic matter, others are not. As examples of a priori truths that are "obviously non-linguistic", Copi mentions the Gödel sentence and Russell's theory of types. It remains unclear, however, what exactly the basis of these judgements is. Mention of the theory of types suggests that one of the implicit criteria Copi uses to distinguish between linguistic and non-linguistic truths is ontological: since they entail the existence of objects, namely sets or classes, the axioms of the theory of types might be regarded as non-linguistic. While this has some plausibility, it does not cover Gödel sentences, about which Copi says the following:

> The situation in general seems to be this. Given any (decent) language, certain truths can be apprehended which for their demonstrations – or even their formulation – require a larger, richer language. But once this is achieved, still new truths can be apprehended which require a still larger and richer language, and so on. To me, this indicates that not all necessary truths are linguistic.[11]

It remains opaque what exactly this alleged process of apprehension is supposed to be, however, and how exactly it follows that the apprehended truths are true in a non-linguistic manner.

4.3 Carnap's Reaction

Within two months Carnap responded to Copi's letter. To an extent he agrees with Copi's assessment of their dispute, namely that much of it seems terminological. But he does express some reservations about thinking that there is no substantial disagreement at all. Let us thus see how Carnap reacts to the three considerations raised by Copi.

With respect to (I), Carnap is in near total agreement with Copi. Contrary to what the latter presumed, Carnap stresses that he had never wanted to maintain that analyticity and a priority are the same concept:

> I would define 'analytic' as a semantic concept ('L-true'). [...] The term 'apriori' on the other hand, is a term not of logic, but of epistemology. I have no exact explication for it; but it seems clear that it would involve such terms as "knowledge" or related ones. Therefore the thesis that all apriori statements are analytic is in no way trivial or a truism on the basis of my view, as you seem to believe.

Some disagreements between Copi and Carnap remains on the issue of whether the empiricist rejection of synthetic a priori knowledge is a thesis or a linguistic proposal. While Carnap grants that some empiricists agree with Copi that

[11] In this section only Copi talks about *necessary* truths instead of a priori truths, for reasons that are unclear.

it should be construed as a synthetic thesis, he himself prefers to conceive of empiricism as a linguistic proposal. Nevertheless, Carnap maintains that, even on this interpretation, it is not a *trivial* matter that there is no synthetic a priori knowledge.

How is that possible? For Carnap, the rejection of synthetic a priori truths amounts to the view that ″if we had adequate explications of all the terms involved, we should be able to show that the thesis holds″. The thesis is thus analytic – and so in a sense a linguistic proposal – since it is supposed to be true in virtue of the explications of the relevant concepts, such as ″knowing″, ″experience″, ″independent of experience″, and the like. But it is far from trivial that this is indeed the case, since actually developing these explications is a substantial task. Carnap's considered position on the relationship between analyticity and a priority is thus as follows:

> If my view is right, and if we had all the necessary explications then it seems to me, the result would be: the terms ′analytic′ and ′apriori′ would turn out to be L-equivalent but not synonymous.

L-equivalence means that, given the explications of these concepts, we could derive ″S is analytic″ from ″S is a priori″, and vice versa. The biconditional ″S is analytic if and only if S is a priori″ is thus L-true, i.e. analytic. The more demanding relationship of synonymy, denied by Carnap, puts additional constrains on how the respective claims are to be derived from each other (Carnap 1949).

Carnap does not comment at all on Copi's point (III). It is likely that this is because he did not share Copi's intuitions about which truths are linguistic and which are non-linguistic, and in any case thought that verdicts of this kind should carry no philosophical weight. This attitude comes out explicitly in Carnap's ″Empiricism, Semantics, and Ontology″, where he opposes the reliance on alleged ″ontological insights″ (Carnap 1956: 214). Furthermore, Carnap also rejected the widespread view that sentences with ontological commitments cannot be analytic. According to him, we can interpret Russell's controversial axiom of infinity, which has usually been classified as non-logical since it asserts that there are infinitely many objects, in such a way that it comes out as analytic (Carnap 1937: 141). It would have been interesting to read Carnap's thoughts on Copi's idea that we can somehow apprehend the truth of undecidable sentences, for it is less clear what he would have made of that notion. But as it stands there is little to go on.

Carnap's response to Copi's point (II), on the other hand, is especially illuminating, since here a certain amount of non-verbal disagreement appears to persist. Carnap can hardly deny that there is an entrenched tradition of equating analyticity with what can be derived using effective rules – as Copi points out, Gödel himself uses the term in this way. But Carnap presents two considerations that speak in favour of going against the traditional usage and in support of his liberal understanding of rules.

Carnap's first consideration is similar to the compositionality argument Copi gave against Turquette. Granting that all the instances of an undecidable Gödel sentence are analytic, Carnap thinks it would be odd to then insist on classifying the generalisation itself as synthetic:

> Gödel's sentence is a universal sentence of such a kind that every substitution instance of it (with the constant of a natural number substituted for the variable) is provable and therefore generally recognized as analytic. The universal sentence does not say anything more than the totality of the instances. I cannot imagine that any apriorist on the one hand, or any empiricist, would be willing to regard a sentence as synthetic, if all its instances are analytic.

Carnap's second consideration is a companion in guilt argument. He thinks that Copi's concerns about extending the notion of analyticity to non-effective rules are misplaced, since "many definitions in mathematics are of this nature". This point is very important for understanding Carnap's position on infinitary rules. But unfortunately he does not give any examples of these allegedly unproblematic non-effective definitions in mathematics. I suspect, however, that he is thinking of cases like the following from computability theory: Some Turing machines halt on every input, others do not have this property. There is no effective procedure that decides whether an arbitrary Turing machine has the halting property or not. Nevertheless, there is a set of all Turing machines that halt on every input. Since Turing machines can be represented as natural numbers, this set is just a particular subset of the natural numbers. We thus have an example of a respectable set whose membership is not effectively decidable.[12]

If Carnap had examples like this in mind then his claim about mathematical practice is correct. But there remains the question of whether this companion

[12] Before *Logical Syntax* Carnap still harboured philosophical misgivings about such undecidable sets (Carnap 1983: 50), but he gave them up in a notable exchange with Gödel (Gödel 2003: 351, 355, Carnap 1937: 113f).

in guilt argument suffices to justify his own use of infinitary rule in the definition of analyticity. As we will see now, all depends on whether we accept the epistemological interpretation of analyticity or not.

5 Analyticity and Epistemology Again

5.1 Examining the Evidence

Having surveyed the exchanges between Copi, Turquette, and Carnap in some detail, let us come back to the systematic question we started with: What, for Carnap, is the relationship between the notions of analyticity and a priority? What, asking more broadly, is Carnap's position on the epistemology of mathematics? I introduced a distinction between two theses of different strength:

> THE EXTENSIONAL THESIS
> A statement is knowable a priori *if and only if* it is analytic.

> THE EXPLANATORY THESIS
> A statement is knowable a priori *because* it is analytic.

The letters remove any remaining doubt about the weaker, extensional thesis. Admittedly, Carnap does not full-on *assert* that a sentence is a priori if and only if it is analytic, because he has not given an explication of a priority yet. But he nevertheless expects and wants a future explication to deliver the result that the extensional thesis comes out true.

How about the stronger, and much more contentious, explanatory thesis? At first sight, the letters may suggest either a negative or an agnostic answer. After all, Carnap emphasises that explicating a priority is a distinct and independent project from explicating analyticity. It is thus tempting to conclude that Carnap either didn't want to rely on the notion of analyticity to explain the possibility of a priori knowledge, or at least had no settled view on whether and how this could be done. The anti-epistemological interpretation of Carnap endorsed by Dreben, Ricketts, and Friedman thus seems to have the upper hand.

I think, however, that this verdict is premature. Some revealing passages in Carnap's letters strongly suggest that a full explication of a priority would not make the notion independent of analyticity after all. Instead, Carnap seems to have thought that the linguistic rules he uses to characterise analyticity are also what enables us to gain a priori knowledge of mathematical truths. This, if correct, would in turn lend support to Creath's epistemological reading and the explanatory thesis.[13]

In a passage from the first letter to Copi I already quoted, Carnap writes that, "on the basis of linguistic rules", the Gödel sentence is "*known* to be analytic" (my emphasis). This naturally suggests a reading according to which linguistic rules are what gives us epistemic access to mathematical truths. It is also in accord with "Empiricism, Semantics, and Ontology", according to which mathematical questions are answered through "logical analysis based on the rules for the new expressions" (Carnap 1956: 209). Now, it is relatively easy to understand how this works for mathematical statements that are *not* undecidable: we try to construct a proof of the statement using the axioms and inference rules provided. But how exactly could linguistic rules help us to determine whether the Gödel sentence is true, as Carnap claims that they do?

In the first letter to Copi, all we learn that the linguistic rules needed here are not the ones of "the given calculus". What Carnap seems to have in mind is that we establish the truth of undecidable sentences in a *metalanguage* in which we talk about some incomplete theory like PA. In his *Introduction to Semantics*, for instance, Carnap writes that sentences independent from the Language I of *Logical Syntax* can be proved in an extension of it:

> Today I should not call the [non-recursive] rules in §14 rules of [Language] I but rather rules of a different though related system, say I_t, containing transfinite rules, instead of 'analytic in I', I should say 'provable in I_t'. (Carnap 1942: 247)

In order to understand how this is supposed to work, it would be helpful to have a concrete example at hand. Carnap's second letter to Copi contains an important clue of how one can establish the truth of an undecidable sentence. In another passage I already quoted, Carnap argues for the analyticity of Con_{PA} by saying that a "universal sentence *does not say anything more* than the totality of the instances" (my emphasis). This suggests that Carnap's strategy goes as follows, taking the case of PA and Con_{PA} as our example:

[13] One might complain that the proposal under discussion does not fit the letter of the explanatory thesis, since what explains the possibility of a priori knowledge is not quite the analyticity of a sentence as such. Rather, there is a *common factor* that explains both why a sentence is analytic and why it is knowable a priori: namely the fact that the sentence follows from certain linguistic rules. Fair enough, but nevertheless the spirit of the explanatory thesis is preserved: Carnap is read as putting forward an explanatory theory about the epistemology of mathematics, contrary to what anti-epistemological readers like Ricketts have him do.

(1) We establish that $\neg Pr_{PA}(0, \ulcorner \bot \urcorner)$, $\neg Pr_{PA}(1, \ulcorner \bot \urcorner)$, $\neg Pr_{PA}(2, \ulcorner \bot \urcorner)$, ..., are provable in PA.

(2) In the metalanguage we then use the ω-rule to infer the universal generalisation $\forall x \neg Pr_{PA}(x, \ulcorner \bot \urcorner)$.

(3) Thus Con_{PA} is analytic, since it can be established purely based on the rules of the object- and the metalanguage.

If this interpretation is correct, then Carnap's definitions of analyticity do not leave open the question of how we know *which* sentences are analytic. Instead, they are meant to provide an answer to the question Turquette posed to Copi: how, if we are so sure that undecidable sentences have determinate truth values, can we tell which ones they have? Carnap's answer, so the suggestion, is that by relying on the ω-rule we can establish that, for instance, Con_{PA} is true rather than false. On this conception analyticity therefore plays a crucial explanatory role in accounting for a priori knowledge in mathematics.

An important clarification is required here to fully appreciate the force of the proposal. We already saw that, in the letters, Carnap and Copi talk about *the* Gödel sentence, presumably meaning the sentence usually glossed as asserting its own unprovability. In the exchange with Turquette, furthermore, undecidable sentences asserting the consistency of a system played a major role. These are two especially interesting examples of undecidable sentences. But for every incomplete mathematical theory there are many more undecidable sentences – infinitely many, in fact. We thus need to ask: When arguing for the analyticity of undecidable sentences, does Carnap only care about the *specific* examples discussed? Or does he defend the more general claim that *every* undecidable sentence is either analytic or contradictory?

In a way the answer is obvious: Carnap endorses the second, general thesis. Otherwise he would have to admit purely mathematical sentences that are synthetic after all, and so his position would largely coincide with that of Copi. This point, however, highlights that the epistemological reading of Carnap commits him to a strong claim: namely that infinitary rules allow us to find out about the truth value of undecidable sentences *in general* – not only for the particular examples discussed, which may appear much more defensible.[14] Is it nevertheless

[14] As has been widely discussed, the Gödel sentence for PA can for instance be proved in PA extended by certain axioms about *truth* (see Waxman 2017 for an overview). One might take this to be an illustration of what Carnap means when saying that the Gödel sentence is shown to be true "on the basis of linguistic rules, though not on the basis of the syntactical rules of the given calculus". But the truth-based strategy does not generalise to all other undecidable

plausible to read Carnap in this way? I will close the discussion by comparing the costs and benefits of the epistemological interpretation.

5.2 Two Paths Diverge

In *MLSA*, Copi mentioned and then dismissed the following alternatives to his own view that undecidable mathematical sentences are synthetic a priori:

(c) Deny that undecidable statements are a priori.

(d) Hold that undecidable sentences are meaningless.

As, noted, however, Copi's confident rejection of these option seemed hard to defend, since he did not offer any positive account of how the a priori justification of mathematical statements proceeds. Without at least the beginning of such an account, how can he be so sure that it is possible at all? In his reply Turquette raised precisely this challenge, and Copi's rejoinder did little to address the underlying problem.

One big advantage of the epistemological reading of Carnap is that it takes him to give the positive account Copi lacked, in the form of an analytic theory of the a priori: Just like regular mathematical statements are justified by deriving them using the recursive axioms and inference rules of a mathematical theory, undecidable mathematical sentences are justified by deriving them using infinitary rules. Contra (c) there is thus a unified a priori epistemology for both kinds of sentences, and contra (d) there is no reason to suppose that undecidable sentences lack truth values.

On the other hand, the explanatory advantage of the epistemological reading comes at a price: Carnap's reliance on infinitary rules becomes much more problematic than he seems to have realised. We earlier saw that Carnap justified his use of infinitary methods by pointing out that "many definitions in mathematics are of this nature". As an example of what Carnap could have meant, I introduced the example of the set of Turing machines that halt on every input. We can grant that definitions of this kind are unproblematic. But this does not suffice to support the epistemological interpretation of analyticity. For if it is correct, then Carnap needs to use infinitary means in a rather different way. In order to gain knowledge about undecidable sentences in the way described, we do not merely need to be able to *talk about* objects that cannot be fully characterised using an effective procedure, such as undecidable sets. Rather, we need

sentences, and so showing that no undecidable sentence should be classified as synthetic requires the more controversial route via infinitary rules.

to actually *use* infinitary rules like the the ω-rule as an inference rule to derive conclusions, just as we do with regular inference rules like modus ponens. In other words, Carnap needs *infinitary rule-following*.

But is that really something we can do? In *Logical Syntax* Carnap dismisses the concern, raised by Tarski, that infinitary rules are fundamentally different from effective ones:

> Tarski discusses [... the ω-rule] and rightly attributes to it an "infinitist character". In his opinion: "it cannot easily be harmonized with the interpretation of the deductive method that has been accepted up to the present"; and this is so far as this rule differs fundamentally from the [... finitary rules] which have hitherto been exclusively used. In my opinion however, there is nothing to prevent the practical application of such a rule. (Carnap 1937: 38, 173)

Most philosophers since, however, have sided with Tarski against Carnap and hold that following the ω-rule is impossible (McGee 1991, Field 1994, Raatikainen 2005, Button and Walsh 2018: chapter 7). If, as the epistemological reading suggests, Carnap wants to deny this, then his philosophy of mathematics crucially relies on a contentious thesis about rule-following and human abilities. An assessment of Carnap's position thus requires further work on the idea that we can follow infinitary rules.[15]

Relatedly, it may seem that the epistemological reading can at most be applied to Carnap's position during his syntactic phase, but not to the later semantic approach. For while one can at least imagine what it means for someone to establish an undecidable sentence based on the ω-rule, it is unclear what the analogous procedure for the semantic definition of analyticity would be.[16] Using contemporary terminology, Carnap defines analyticity as truth in the standard model of arithmetic. How could this type of definition serve as an epistemic guide for establishing whether a particular undecidable sentence is true or false? I agree that this is a real challenge. Personally I think that it can be answered, but for reasons of space I will not be able to elaborate here.[17] Let us instead consider the merits of rejecting the epistemological interpretation of analyticity.

Those who deny the connection between analyticity and a priori knowledge have one clear advantage: namely that they have no need for the mysterious-seeming infinitary rule-following just described (Ricketts 2003: 262). But they

[15] See Warren 2020 and Warren 2021 for a recent defence of infinitary rule-following.

[16] Koellner ms: 34 raises an analogous challenge for the idea that such semantic rules "determine" the truth values of mathematical statements.

[17] I do so in [redacted].

face other challenges: If not for epistemological reasons, why was the analyticity of mathematics so important to Carnap? Why not just call undecidable mathematical sentences synthetic, as Copi suggests? What speaks against Turquette's options (c) and (d)? And how can Carnap's remarks about gaining mathematical knowledge from the letters be understood?

To see the force of these questions, it is useful to consider Quine's reaction to Carnap's philosophy of mathematics. He took the notion of analyticity to play an epistemological role, and rejected this approach in favour of his own alternative:

> Once we appreciate holism [...] the notion of analyticity ceases to be vital to epistemology. (Quine 2008: 26f)

Like Quine, those who reject the epistemological reading do not think that the notion of analyticity is vital to the epistemology of mathematics. But, unlike Quine, they still want to defend Carnap's insistence on a sharp analytic/synthetic distinction, with undecidable mathematical sentences falling on the analytic side. But this is hard to pull off, since the issue threatens to turn into a purely terminological quibble. Consider again Carnap's remark that he "cannot imagine that [anyone] would be willing to regard a sentence as synthetic, if all its instances are analytic". What exactly would be so bad about the classification described here? Once we give up the connection between the analyticity of a sentence and its epistemological status, it is hard to think of good answers. I thus contend that the enemies of the epistemological reading face a challenge as well: they need to explain why Carnap cared about the analytic status of undecidable mathematical sentences.

6 Conclusion

Carnap's exchange with Copi has not enabled us to conclusively answers all questions about Carnap's epistemology of mathematics. But important progress has been made. First, we have some concrete textual evidence that can be taken to support an explanatory reading of Carnapian analyticity. Secondly, we have a clearer conception of the advantages and disadvantages of such a reading. Thirdly, the role of infinitary methods in Carnap's philosophy of mathematics has been illuminated. And, fourthly, we have seen that opposing the explanatory thesis raises difficult exegetical questions as well. No doubt others who study the exchange will be able gather additional insights into Carnap's thinking from them.

7 The Letters

7.1 August 24, 1949: Carnap to Copi

Dear Dr. Copilowish,

Thank you for your letter of April and for the return of the Rüstow. I am very glad to hear that you were appointed assistant professor at Michigan, and that you are living and working there happily and successfully.

I am also glad to learn that you are writing an introductory logic text. I have used in recent years Cooley's book. It may be dry and dull as you say, but it gives clear explanations, and examples, and exercises. In my view, it is the best among the books so far available. But certainly, there is room for improvement. I heard that Cooley himself is working on an improved and entirely changed edition. Quine is also writing one – it will remain to be seen what level it will be on. Your book on the paradoxes will be still more valuable. I hope, it will not take you too much time to work it out. At present there is actually no satisfactory representation of this topic at all. Especially the distinction between the logical and the semantical antinomies, although noticed already by Ramsey, has not been satisfactory explained by anybody from a contemporary point of view. From what I have seen and heard from your thesis, you seem to be just the right man for doing it.

I read with interest your paper ″Modern Logic and the Synthetic A Priori″. But I must tell you frankly that I cannot agree at all. Your reasoning seems to be based either on a wrong conception of our concept of ″analytic″, or on a misconception of Gödel's result, or both. It is true that in the period before semantics I regarded the concept ″analytic″ as syntactical. But even then this concept, e.g., as defined in my ″Logical Syntax″, is by no means identical with ″provable″, because it is based on transfinite syntactical rules. To a calculus containing such rules, Gödel's result does not apply; it holds only for calculi consisting of finite rules. More importantly, we would define ″analytic″ today as a semantical, not a syntactical concept (L-true). Thus, it is still based on linguistic rules, but not on syntactical ones. Now Gödel's result may be formulated in my terminology as follows. For certain semantical systems of arithmetic, there is a sentence which is L-true but not provable (and not decidable). The decisive point which you seem to overlook is this: it follows from Gödel's consideration that his undecidable sentence is true, and, moreover, L-true. In other words, Gödel has actually shown, on the basis of linguistic rules, (though not on the basis of the syntactical rules of the given calculus), that the sentence is true. Thus the sentence is known to be analytic. It can by no means be regarded as synthetic, in any case not on the basis of those definitions for "analytic" and "synthetic" we use, and which are presupposed in our formulation of the thesis of empiricism, which says that there are no synthetic statements a priori.

We are again on our hill in Santa Fe. Most of the time I am working on my probability book, that is to say, on the second volume; the first volume is in print just now – it will appear in the spring of 1950. Recently Feigl was here for a week, and soon Quine will come through. Thus there are also nice opportunities for seeing friends, talking about philosophy and the world.

With best regards from both of us to both of you,

Yours,

7.2 June 15, 1950: Copi to Carnap

Dear Professor Carnap:

I must apologise for not having answered your letter sooner. The delay was not caused entirely by neglect, for I have thought often of your remarks on the a priori, and have even begun answers several times. But none were satisfactory, up to now.

It is very interesting to me that your objections were so diametrically opposed to those of Turquette. You insist that Gödel's sentence is analytic, while he wavers between calling it either false or nonsensical. The entire matter is evidently in need of clarification, since in general you and Turquette share the same basic philosophical position. I enclose a copy of a rejoinder to Turquette which I have submitted to the Journal of Philosophy. (I have not yet been notified of their decision as to whether or not they will print it.) As is clear in the paper, I disagree vigorously with Turquette.

But in spite of your stating that you "cannot agree at all" with me, I nevertheless want to agree with you. That is, I feel that our disagreement is merely verbal. I believe that we agree on the logical facts of the matter. (1) Gödel's sentence is true, and it is – in some sense or other – logically true. (2) And it is, as we know, not provable or derivable within the system in which it occurs and of which it speaks (assuming consistency, of course). Because of (1), you want to call it "analytic." Because of (2),[18] I want to call it "synthetic." We both agree that it is a priori true.

Since we agree on the facts, there should be no great disagreement over the words. I can only indicate why my usage seem to me preferable. Please understand that I do not hold them to be conclusive reasons, and that I admit that your interests can legitimately lead you to prefer a somewhat different terminology. I am fully prepared to be tolerant of different choices of terminology. Here, however, are my reasons for preferring my own.

I. In the interest of greater intelligibility, I feel it important that philosophical doctrines should be clearly distinguished from linguistic proposals. If to say that "there are no synthetic statements a priori" is to formulate the "thesis of empiricism", then "analytic" and "a priori" must have different definitions. Traditionally and historically, they do have different definitions. A priori true statements are those which are necessarily true and not empirical or inductive, that is, not a posteriori. And analytic statements are those which are necessarily true in that to deny them violates the law of non-contradiction, (which I take it intends that they can be finitely proved from the logical principles included within the language in which they are formulated.) Thus, historically, the a priori is – at least intensionally – a broader concept than the analytic. Now if you propose to give the same definition for both "analytic" and "a priori", this violates historical usage. That is no objection, of course; if we explain our usages we are free to adopt whatever definitions we choose. But if we do propose this new, unconventional usage, then the "thesis of empiricism", as formulated above, is no longer a thesis. It is tantamount to a linguistic proposal. Again, let me say that there is nothing wrong with linguistic proposals. But they are not theses or doctrines, and to regard them as such "... has the disadvantage of leading easily to self-deception ..." (to borrow a phrase from your Logical Syntax, p. 312.) To summarise this first point: if your sense of "analytic" is adopted, then the "thesis of empiricism" is no thesis, but merely a disguised linguistic proposal; and since I want to regard empiricism as a significant doctrine, I do not want to use "analytic" as synonymous with "a priori".

II. There are historical reasons for using " analytic" as non-synonymous with "a priori". Here I do not refer simply to the past usage outside of logic (of the sort mentioned in the preceding paragraph), but to the history and development of logic itself. One need not have studied logic to be able to distinguish valid from invalid arguments. A sufficiently acute person can judge correctly ques-

[18] Handwritten insertion: "considering the adequacy of the system in which it occurs".

tions of validity simply by letting his "logical intuition" consider the meanings of the assertions involved. On the other hand, a person who has studied logic, and has come to accept certain argument forms (such as modus ponens) and the hypothetical syllogism) as valid argument forms, has a different method of evaluating presented arguments. If the presented argument is of one of these forms, then it is immediately accepted as valid. This can be done without any use of "logical intuition" in the particular case, the evaluation being performed by simply seeing the form and recognizing it as one already accepted as valid. Thus the study of logic permits the evaluation of presented arguments simply by comparing their shapes with antecedently given patterns, substituting a mechanical technique for ratiocination. Of course, ratiocination is involved in preparing the set of patterns. But even the working out of a full panoply of theorems in a deductive system can be done in a purely formal fashion, with no need to consider meanings. Formal logic, as we know, is concerned with forms rather than with meanings. Effectiveness or finiteness is of the essence here, since only in this fashion can universal agreement be reached about what follows from what. The valid forms of argument that can be deduced within a good logical system are all analytic. In a rather generous interpretation of this phrase, they can all be derived from the principle of non-contradiction, which is a purely formal principle. This is the sense of "analytic", as used both traditionally and by Gödel in his article in the Russell-Schilpp volume. This makes the notion of analyticity clearly a linguistic-deductive one. And of course all analytic statements are a priori. But to enlarge or expand the notion of "analytic" so as to make it intensionally coextensive with the a priori, is to destroy the linguistic character of that notion, and to simply beg an important philosophical question. To summarize this second point: if the constructive or finite sense of "analytic" is abandoned (either by generalizing it so as to include what follows by transfinite rules, or by shunting it out of the formal realm of formal logic into semantics) then the whole historical tradition which identifies it with deducibility is disregarded, and apparently just for the sake of reinterpreting a genuine question so that it will permit a tautologous answer.

III. Finally, there are philosophical reasons for preferring to keep separate the senses of 'analytic' and 'a priori'. Some truths are pretty obviously linguistic. But other, equally necessary truths seem to be obviously non-linguistic. The Gödel sentence seems to be one example of the latter sort. Russell's simple theory of types seems to be another. (Of course, mathematically considered, type theory seems to be just one of many alternative devices to accomplish a certain result. But philosophically considered, it resolves too many philosophical puzzles to be regarded as simply as an ad hoc mechanical device.) The situation in general seems to be this. Given any (decent) language, certain truths can be apprehended which for their demonstrations – or even their formulation – require a larger, richer language. But once this is achieved, still new truths can be apprehended which require a still larger and richer language, and so on. To me, this indicates that not all necessary truths are linguistic. Since there is this distinction, it would seem valuable to have two distinct terms, one to denote the linguistic truths, the other to denote the whole class of necessary truths, linguistic and non-linguistic both. We have the terms 'analytic' and 'a priori' which suffice.

Finally, I am intrigued by Saunders MacLane's suggestion in his March 1938 review of your Logical Syntax. There he raised the question of what would happen if analyticity were to be regarded as a linguistic property and one could, by the Gödel technique, construct a sentence asserting its own non-analyticity. (This would seem all the more plausible in light of subsequent generalizations of the incompleteness results to include non-finitary systems of logic.) You, in your Syntax and Gödel in his Princeton notes, observe that 'true' and 'false' are not linguistic notions. It would seem to be the case that 'analytic' and 'non-analytic' aren't either.

I am still working on my text-book, which is a little over half finished. Teaching takes a great deal of my time and energy, so that progress in writing is disappointingly slow. However, I hope to be finished some time early next year. Your kind words of encouragement on my projected work on the paradoxes are heartening and much appreciated.

My wife joins me in greeting Mrs. Carnap and you.

Yours,
Irving Copi

P.S. Last month a third son arrived: We have named him William Arthur Copi.

7.3 August 16, 1950: Carnap to Copi

Dear Mr. Copi,

Thank you very much for your letter of June 15 with a detailed discussion of the problem of synthetic and analytic and for your manuscript with the reply to Turquette.

After reading your explanations, I am inclined to agree with you that the difference between us may be mainly of a terminological nature, though perhaps not entirely.

I still think that to call Gödel's sentence synthetic would deviate too much from the traditional use of the term. Gödel's sentence is a universal sentence of such a kind that every substitution instance of it (with the constant of a natural number substituted for the variable is provable and therefore generally recognized as analytic. The universal sentence does not say anything more than the totality of the instances. I cannot imagine that any apriorist on the one hand, or any empiricist, would be willing to regard a sentence as synthetic, if all its instances are analytic.

You seem to think that I propose to give the same definition for both 'analytic' and 'a priori'. This, however, is not at all the case. I would define 'analytic' as a semantic concept ('L-true'). The fact that the definition is not effective (definite), cannot be an objection against it, because many definitions in mathematics are of this nature. The term 'apriori' on the other hand, is a term not of logic, but of epistemology. I have no exact explication for it; but it seems clear that it would involve such terms as "knowledge" or related ones. Therefore the thesis that all apriori statements are analytic is in no way trivial or a truism on the basis of my view, as you seem to believe. I think, the thesis, if true, is analytic, not synthetic; but that, of course, does not make it in any way trivial. I believe, for example, that the problem of the alleged synthetic apriori nature of the statement "orange is more similar to red than to green" constitutes a very serious problem; I would not claim that claim that I could solve or brush aside this problem with a move of the hand. The thesis of analyticity seems to me to be the result of explications, as many other philosophical theses, e.g. Frege's thesis concerning the logical nature of mathematics, and many others. The great difficulty in solving the problem, which makes it non-trivial, is just the great difficulty of finding adequate explications for such concepts as "knowing", "experience", "independent of experience", etc. When I assert the thesis of analyticity, I mean by that to assert that <u>if</u> we had adequate explications of all the terms involved, we should be able to show that the thesis holds (and moreover holds logically, and hence is analytic). But I may remark, by the way, that the latter view is not shared by all my empiricist friends. Some of them regard the regard the thesis of empiricism as synthetic rather than analytic. This, of course, depends on upon the way of explicating the terms in question. If my view is right, and if we had all the necessary explications, then it seems to me, the result would be: the terms 'analytic' and 'apriori' would turn out to be L-equivalent but not synonymous. (On the difference between the two latter concepts see my "Reply to Linski".)

To MacLane's remark. The answer is clear. The decisive logical fact is the following: 'Provable in S' can, under certain conditions, be defined within the system S itself; but 'Analytic in S' cannot; this can be proved in analogy to Tarski's proof concerning the non-definability of 'true in S'. Thus it is impossible to construct a sentence asserting its own non-analyticity.

Well, a letter is insufficient to discuss all these problems. I hope, we shall have an opportunity to talk about them in detail.

Our best congratulations to the arrival of your third son. In view of the critical times, your courage is admirable. If only our statesmen were wiser so that we could have better hope for avoiding the catastrophe!

I agree with your rejection of Nagel's proposal of a language with only empirical sentences. But I should prefer to say not that is is impossible but that it is inconvenient because the rules of formation would become indefinite (see my "Testability and Meaning" p. 424).

With best regards from both of us to both of you,

Yours,

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