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Carnap, Gödel, and the Analyticity of Arithmetic†

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Michael Friedman maintains that Carnap did not fully appreciate the impact of Gödel's first incompleteness theorem on the prospect for a purely syntactic definition of analyticity that would render arithmetic analytically true. This paper argues against this claim. It also challenges a common presumption on the part of defenders of Carnap, in their diagnosis of the force of Gödel's own critique of Carnap in his Gibbs Lecture.

We begin by reviewing, in §1, the fate of the analytic/synthetic distinction and how it was being applied in the period leading up to the publication, in 1934, of Carnap's *Logische Syntax der Sprache*. In §2 we shall examine Michael Friedman's recent reassessment of Carnap's account of that distinction, in light of Gödel's incompleteness theorems for arithmetic. We shall seek to defend Carnap's account by attending carefully to what he wrote in *Syntax*. In §3 we advance a new defence of a recognizably Carnapian position, by making points based on a logical analysis of the relevance of premises to conclusions of valid arguments. In §4 we conclude with some further reflections on Quine's celebrated attack on the analytic/synthetic distinction itself.

1. Some Historical Background

As Carnap notes in his intellectual biography [Carnap, 1963, p. 64], one of the 'major insights' of logical positivism was that logic *and mathematics*

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The literature on this topic since 1992 appears to leave some space for the ideas and arguments presented here. Although the paper has been updated in light of the more recent literature, its basic thesis, presented in 1992, remains the same. Only §3 is new, questioning a basic presumption made by more recent commentators in their presentation of Gödel's criticism of Carnap in his Gibbs Lecture. For helpful comments on the current version, the author is indebted to Robert Kraut, Stewart Shapiro, and Adam Podlaskowski.

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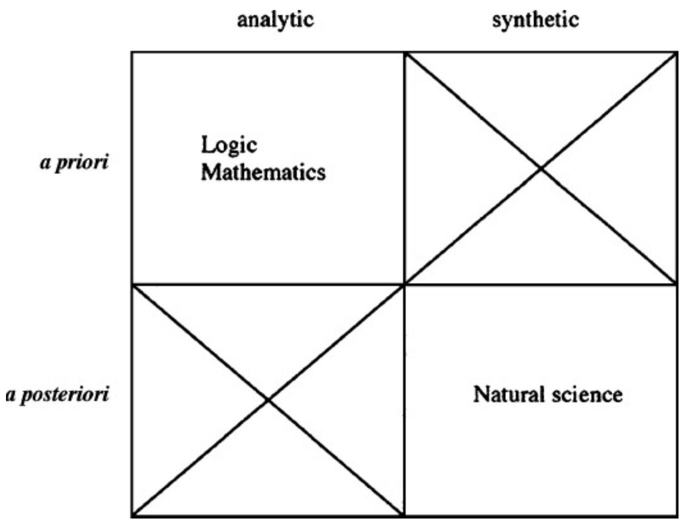
were analytic, while empirical science was synthetic. Kant's favorite species, the synthetic *a priori*, was made extinct by logical positivism. Arithmetic was thought to have been assimilated or reduced to logic by the Fregean or Russellian definitions of number in 'logical' terms, and the consequent derivation of basic arithmetical laws within class theory or type theory. Geometry was splintered: formal geometry was analytic, going the same way as number theory; while *physical* geometry, in the wake of the theories of special and general relativity, was to be regarded as both conventional and beholden to empirical evidence.[1] Other important Kantian synthetic *a priori* truths were discredited, and demoted to *a posteriori* status. The Kantian principle of substance (construed as conservation of mass[2]) was shaken by relativity theory, and the fundamental principle of cause (that every event has a cause) was shaken by quantum theory. The diagonalization of the analytic-synthetic/*a priori*-*a posteriori* diagram was complete:



It took time for the consequences of Gödel's discovery of the incompletability of formal arithmetic to sink in. There was no immediate rush to resurrect arithmetic truth as synthetic.

[1] Carnap, in his 1921 doctoral dissertation *Der Raum*, distinguished between formal and physical geometry. Carnap sought to recover a more modest spatial *a priori* in the *topological* core common to the competing physical geometries after Einstein's revolution. The *metric*, however, was man-made, conventional.

[2] But see von Weizsäcker's re-interpretation [von Weizsäcker, 1971] of the principle of substance as concerning mass-energy instead of just mass.

2. Gödel's Incompleteness Theorems and the *Logical Syntax of Language*

Michael Friedman has claimed that there is evidence in *Syntax* that Carnap had not fully absorbed the implications of Gödel's result for the claimed analyticity of arithmetic:[3]

> *Analyticity-in-L* fails to be captured in what Carnap calls the '*combinatorial analysis* ... of finite, discrete serial structures' (§2): *that is, primitive recursive arithmetic* [emphasis added]. Hence the very notion that supports, and is indeed essential to, Carnap's logicism simply does not occur in pure syntax as he understands it. . . .
>
> In the end, what is perhaps most striking about *Logical Syntax* is the way it combines a grasp of the technical situation that is truly remarkable in 1934 with a seemingly unaccountable blindness to the full implications of that situation. [1988, p. 93]

What are the alleged reasons for the failure to capture *analytic-in-L*? As Friedman explains, Carnap's classification of arithmetical sentences as analytic depended on logico-mathematical sentences being *determinate*, that is, provable or refutable. At §50 of *Syntax* Carnap writes '. . . we find the formally expressible distinguishing peculiarity of logical symbols and expressions to consist in the fact that each sentence constructed solely from them is determinate'. By 'logical' here, Carnap meant *logico-mathematical*; and in particular, *logico-arithmetical*. His Theorem 50.1, which was a trivial consequence of his very definition of what logical expressions were, was to the effect that every logical sentence is determinate.

Now one must not too hastily conclude that this contradicts Gödel's first incompleteness theorem for arithmetic. For, as Friedman is aware, Carnap was using here an *extended* notion of provability (and of refutability). Carnap was, in fact, helping himself to the infinitary ω-rule, which guarantees the completion of Dedekind-Peano arithmetic.[4] Carnap *knew* that Gödel's result forced such recourse if one wished to have every arithmetical sentence decided one way or the other in the formal system of transformation rules. One need only read §34a, on *Incomplete and Complete Criteria of Validity*, to be reassured on this score. After explaining first the absence of a decision method, and second the essential incompletability of recursively axiomatized theories of arithmetic (those based on what Carnap calls the *d-method*), Carnap writes:

[3] See also [Friedman, 1992, pp. 57–59], where his critique based on the purported requirement of metalogical neutrality is reprised.

[4] [Goldfarb, 1995, p. 329] and [Ricketts, 1996, p. 234] are also sensitive to this point.

> In order to attain completeness for our criterion we are thus forced to renounce definiteness, not only for the criterion itself *but also for the individual steps of the deduction*. . . . A method of deduction which depends upon indefinite individual steps, and in which the number of the premisses need not be finite, we call a *method of consequence* or a *c-method*. In the case of a method of this kind, we operate, not with sentences but with sentential classes, *which may also be infinite*. (Emphases added)

The exegetical question that therefore arises is whether Friedman is sufficiently charitable in his judgment that Carnap's attempted rehabilitation of logicism via logical syntax failed because of an inexplicable myopia (or worse: 'unaccountable *blindness*') concerning the full import of Gödel's result. This appears not to be the case. Friedman is not being altogether fair to Carnap in holding him to such a strong reading of Carnap's own standard of adequacy for his project in *Syntax*. The full context of the quotation from Carnap that Friedman makes above from §2 of *Syntax* is:

> In pure syntax only definitions are formulated and the consequences of such definitions developed. Pure syntax is thus wholly analytic, and is nothing more than *combinatorial analysis*, or, in other words, the *geometry* of finite, discrete, serial structures of a particular kind.
>
> . . .
>
> When we say that pure syntax *is concerned* with the forms of sentences, this 'concerned with' is intended in the figurative sense. . . . *The figurative 'concerned with' is intended here in the same sense in which arithmetic is said to be concerned with numbers, or pure geometry to be concerned with geometrical constructions*. (Final emphasis added)

It is now open to a defender of Carnap to point out to Friedman that the metaphorical reading intended by Carnap is that the finite, discrete, serial structures are to be likened to *geometrical points*; and, to continue the analogy, an *infinite sequence of such 'points'* (*e.g.*, the infinite sequence of premises involved in an application of the ω-rule) may be likened to a *geometrical line*. Simply being infinitary does not count against being *purely formal*. It is still without any reference to what symbols *mean* or *denote* that the application of the rule would be legitimate. Friedman is demanding too much in thinking that the resources of such combinatorial analysis as Carnap requires should not exceed those of primitive recursive arithmetic. That is an Hilbertian hangover from an *epistemological* concern in foundations, a concern with *reductive justification*. But Carnap's concern is altogether different in *Syntax*; it is a concern with *conceptual delimitation*, or *classification*, using purely formal syntactic considerations.[5] He was aiming to clarify a purely formal or syntactic sense in which logico-mathematical sentences were *analytic* or *contradictory*. That the *demonstration* of such status for any particular sentence could in general require more powerful resources than those of primitive recursive arithmetic he was only too well aware. In §34h, 'The principles of induction and selection are analytic', he is clear on this point, with regard to what his definition of 'analytic' is intended to effect: '... the characterization of a sentence as analytic if, *in material interpretation*, it is regarded as logically valid' (emphasis added). The material interpretation in question, of course, allows for truth-transmission by the ω-rule.

There is further evidence that Friedman is demanding too much on Carnap's behalf in the *logicist* component that is being reconciled with the formalist component to yield an adequate philosophy of mathematics. In §84 'The problem of the foundation of mathematics' in *Syntax*, Carnap points out that logicism can, while formalism cannot, make sense of the application of arithmetical concepts in empirical discourse.[6] Carnap then observes that his

> ... procedure is a purely formal one, and ... the meaning of the mathematical symbols is established and thereby the application of mathematics in actual science is made possible, namely, by *the inclusion of the mathematical calculus in the total language*. ... The *requirement of logicism* is then formulated in this way: *the task of the logical foundation of mathematics is not fulfilled by a metamathematics (that is, by a syntax of mathematics) alone, but only by a syntax of the total language, which contains both logico-mathematical and synthetic sentences.*
>
> Whether, in the construction of a system of the kind described, only logical symbols in the narrower sense are to be included amongst the primitive symbols (as by both Frege and Russell) or also mathematical symbols (as by Hilbert), and whether only logical primitive sentences in the narrower sense are to be taken as L-primitive sentences, or also mathematical sentences, *is not a question of philosophical significance, but only one of technical expedience*. (last emphasis added)

[5] Here the present author is in agreement with [Goldfarb and Ricketts, 1992, pp. 66–68], with [Goldfarb, 1995, at pp. 329–330], and with [Richardson, 1994, p. 76].

[6] Whether this view would survive Hartry Field's investigations [Field, 1981] of how mathematics conservatively extends synthetic theories, is a matter beyond the scope of the present discussion.

In the light of this emphatic qualification by Carnap of the demands of logicism, it is hard to accept Friedman's criticism of Carnap as having failed to accomplish a goal that he set himself. Friedman cannot be right in holding that it is only *primitive recursive arithmetic* that Carnap could have admitted as the language (or theory) of syntax. Carnap's own Theorem 60c.1 stated

> If S is consistent, or, at least, non-contradictory, then *'analytic (in S)' is indefinable in S.* . . .
>
> If a syntax of a language S_1 is to contain the term 'analytic (in S_1)' then it must, consequently, be formulated in a language S_2 which is richer in modes of expression than S_1.[7]

Carnap also noted quite candidly in §34h that

> [i]t is clear that the possibility of proving a certain syntactical sentence depends upon the richness of the syntax-language which is used, and especially upon what is regarded as valid in this language.

Friedman's rejoinder (personal communication) is to maintain that logical syntax loses its philosophical neutrality as between classical and intuitionistic mathematics, say, if it goes beyond the resources of primitive recursive arithmetic. And he asks what the *point* would be of retreating from a whole-hearted commitment to classical mathematics as *correct* to the standpoint of logical syntax where we simply describe the consequences of adopting classical mathematics.

Again, Friedman appears to be demanding too much of the 'neutrality' to which Carnap aspired. This was not a neutrality that consisted in *poverty of principles*. He clearly did not feel obliged to surrender either existence principles or principles of logico-mathematical inference *in his chosen meta-system*. He did not feel obliged to eschew any such principle that might be a bone of contention between proponents of the object-systems that were to be subject to his syntactic investigations. Rather, Carnap's professed neutrality consisted in a refusal to *transmit down from the meta-level* to any object-system under study any of the commitments he was forced to make at the meta-level in order successfully to carry through the *purely syntactic* investigation of that object-system. His responses to Gödel's discoveries can be better described by saying that Carnap was only too acutely aware of how the meta-system *had* to be stronger *in order to establish his main thesis that the logic of science is the syntax of the language of science*.

[7] Interestingly, Carnap speaks here of modes of *expression*, not modes of *inference*.

In order to sustain this response to Friedman, one would need a more delicate investigation, for which there is not space here, of whether syntactical investigations at the meta-level carry one's commitments down to the object-level; and of the way in which those meta-commitments can take the form of either *existence principles* or *principles of inference*. One could, for example, have strong existence principles within an intuitionistic meta-system, in which one could prove the completeness of first-order (intuitionistic!) arithmetic equipped with the ω-rule.[8]

3. A Challengeable Presumption Underlying Gödel's Critique of Carnap

Gödel's most cogent criticism of Carnap, in the view of every commentator cited, is that Carnap incurs the burden of proving the consistency of mathematics—which burden, by Gödel's Second Incompleteness Theorem, cannot be discharged within the mathematics in question. Both [Ricketts, 1994] and [Goldfarb, 1995] hasten to argue that Gödel's critique misses its mark because Gödel fails to understand how Carnap's Principle of Tolerance had transformed the traditional debate between the realist and the conventionalist (or anti-realist, or positivist). They point out that for Carnap there is no framework-independent access to the notion of *empirical fact*, a notion that Gödel needs to employ in mounting his challenge.[9]

Neither of these commentators, however, takes issue with Gödel's presumption that a consistency proof is required in order to show that no empirical fact will follow from the mathematics adopted by the Carnapian.[10] They simply acquiesce with this Gödelian presumption, and seek to deflect Gödel's critique (in the way just indicated) rather than meet it head-on. Friedman, too, acquiesces with the Gödelian presumption. It is worth supplying detailed quotes to back up these observations.

Here are extended quotes from two different works by Ricketts.

> Kurt Gödel . . . advances a very general and persuasive objection to [Carnap's] viewpoint in an unpublished paper, 'Is mathematics syntax of language?' [sic] The application of arbitrarily stipulated syntactic rules will not generally take us

[8] That the ω-rule succeeds in completing *intuitionistic* first-order arithmetic is a point that Friedman appears to miss in his comments [Friedman, 1999, pp. 228–229].

[9] [Potter, 2000] at p. 270 agrees with this line of defence against Gödel's consistency critique, and canvasses no other.

[10] It is worth mentioning that the discussion by Michael Potter [2000, Ch. 11] acquiesces in the presumption in question. Only on this presumption can Potter claim (p. 273) that (on Carnap's criterion for descriptiveness) an inconsistent language 'has no descriptive vocabulary', hence 'cannot be interpreted as saying anything whatsoever (even anything trivial) about the world'.

> from empirical truths to empirical truths. We will not accordingly be justified in using a Carnapian language to reason about empirical matters, unless we have some reason to believe that the syntactic rules specifying the consequence relation 'do not themselves imply the truth or falsehood of any proposition expressing an empirical fact'. [fn] Gödel calls syntactic rules satisfying this condition *admissible*. Moreover, he thinks that if a set of syntactic rules is not admissible, then it is incorrect to call the sentences whose truth follows from the rules 'contentless' and to contrast them, in this respect, with real, empirical sentences. A proof of admissibility is then required to underwrite the foregoing contrast of analytic and synthetic sentences. *The admissibility of a set of syntactic rules implies the consistency of the language specified by those rules.* In particular, a proof of admissibility for the syntactic rules for one of Carnap's languages would also be a proof of the consistency of the mathematics formalized in the language. On the basis of the second incompleteness theorem . . . the mathematics required for the proof of admissibility cannot itself be taken to be true by syntactic convention without vicious circularity. [Ricketts, 1994, pp. 179–180] (emphasis added)
>
> Gödel maintains that in order to *justify* the claim that the sentences demarcated by some syntactic rules are true whatever the empirical facts may be, we have to be able to show, in advance of adopting the rules, that the rules are *admissible*, that they 'do not themselves imply the truth or falsehood of any proposition expressing an empirical fact' [Gödel, 1995b, p. 357]. That is, the mathematics and logic of a Carnapian language must be shown to be conservative over the synthetic sentences of the language, if we are to be justified in taking the mathematics and logic to be unconditionally true, that is, empirically contentless truths. Gödel observes that *a proof of admissibility is a proof of consistency* for the object-language set forth by the rules, and, by the second incompleteness theorem, this proof requires the use of mathematics more powerful than that formalized in the object-language. [Ricketts, 1996, p. 237] (second emphasis added)

And here are extended quotes from two different works by Goldfarb, to much the same effect.

> Gödel states, '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 of any "factual" sentence.' (III, §11; in a similar remark

> in V, page 3, he replaces 'factual sentence' by 'proposition expressing an empirical fact'.) *This requirement will be met only if the rule of syntax is consistent, since otherwise the rule will imply all sentences*, including factual ones. Hence, by the Second Theorem, mathematics not captured by the rule in question must be invoked in order to legitimize the rule, and so the claim that mathematics is solely a result of rules of syntax is contradicted. [Goldfarb, 1995, p. 327] (emphasis added)

> Gödel's central argument is based on his second incompleteness theorem. As he puts it, '[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" sentence' [Gödel, 1995b, §11, p. 339]. Evidently, *a rule will fulfill this requirement only if it is consistent, since otherwise the rule will imply all sentences*, factual and logical alike. The second incompleteness theorem states that mathematics not captured by the rule in question must be used in order to prove the rule consistent. Thus, additional mathematics must be invoked in order to legitimize the rule, and the claim that mathematics is solely a result of rules of syntax is refuted.
>
> This is a powerful argument . . . [Goldfarb, 1996, p. 226] (emphasis added)

Finally, here is a quote from Friedman, setting out Gödel's objection to Carnap. Friedman, too, does not demur from Gödel's presumption that a *mathematical* inconsistency would imply at least one (indeed, any) *empirical* claim:

> Gödel argues that, if the choice of logico-mathematical rules is really to be viewed as conventional, then we must have independent assurance that these rules do not have unintended empirical or factual consequences. We must know, that is, that the rules in question are conservative over the purely conventional realm. *We therefore need to show that the rules are consistent*, and this, by Gödel's second theorem, cannot be done without using a meta-language whose logico-mathematical rules are themselves even stronger than those whose conservativeness is in question. Hence, we can have no justification for considering mathematics to be purely conventional, for an unintended incursion into the empirical or factual realm cannot be excluded without vicious circularity. [Friedman, 1999, pp. 201–202] (emphasis added)

The standard systems of intutionistic and classical logic allow any sentence whatsoever to follow from a contradiction. This is what disqualifies these systems from counting as paraconsistent, hence also from counting as relevant. Now, the common presumption underlying the foregoing quotes from Ricketts, Goldfarb, and Friedman is that the logic of the language in which empirical statements may or may not follow from the mathematics formulated within it *has to follow the example of the standard systems of intutionistic and classical logic in this regard*. The presumption is therefore to the effect that the logic contains the rule of *ex falso quodlibet*, the so-called 'absurdity rule'. Notoriously, this rule underlies the first paradox of Lewis:

$$\frac{A \qquad \neg A}{B}$$

and its presence is the reason why neither intuitionistic nor classical logic qualifies as a *paraconsistent* logic, or as a *relevant* logic.

It is open to a defender of Carnap to point, on Carnap's behalf, to the existence of (paraconsistent, because) relevant logics *not* containing this rule. Fully developed relevant logics that are demonstrably adequate for the demands of both mathematics and empirical theory-testing became available only after Carnap's and Gödel's writings on these issues.

The system *IR* of intuitionistic relevant logic, and the system *CR* of classical relevant logic, are examples of such systems.[11] If Q is deducible from $P_1, \ldots, P_n$ in intuitionistic [resp., classical] logic, then either Q or $\perp$ is deducible from (some of) $P_1, \ldots, P_n$ in *IR* [resp., *CR*]. This metatheorem ensures that intuitionistic [resp., classical] mathematics does not need *ex falso quodlibet*. If the Carnapian would but fit out his chosen language(s) with just such a relevantized, hence 'paraconsistent', consequence (or deducibility) relation, then Gödel's criticism on this score would be circumvented.

The interesting question would then arise of just how much mathematics would be needed for a proof of admissibility, within a metalogical context where admissibility does not entail consistency. (Note that these considerations hold even in the presence of infinitary rules such as the ω-rule.)

This proposed head-on response to Gödel's criticism does, however, appear to clip the Carnapian's wings. For it commends to the Carnapian (and to his reader) only the more limited range of suitably *relevantized* logics. What, then of the Principle of Tolerance? In logic, there are supposed to be no morals. Yet here the relevantist appears to be imposing

[11] See [Tennant, 1987; 1994; 1992; 1997; 2002].

a moral: *adopt and investigate only those logics whose consequence (or deducibility) relations are, in a suitable sense, relevant.*

The Carnapian may, however, be able to avoid this appearance of preaching (via the original Tolerance Principle) what he is no longer prepared to practise (because of the newly-recommended insistence on relevance). All that is required is that, for any logic in the original range countenanced by the Carnapian, it should be possible (by means of a uniform method) to extract from its deducibility relation what might be called its 'relevantized core'. This indeed is what already happens in the case of the systems of intuitionistic and classical logic. Their respective relevantized core systems *IR* and *CR* are obtained from them by applying a uniform method. The method involves turning proofs into normal form, and then removing from them all applications of the absurdity rule, upon suitable re-fashioning of the remaining natural-deduction rules. If such a uniform method were to have a suitably wide range of application, then the Carnapian would be well placed to reconcile the Principle of Tolerance with the new suggested means of disposing of Gödel's consistency-based critique.

4. Concluding Reflections

Carnap would have been in exonerating company even if Friedman's diagnosis and criticism were correct. For in 'Truth by convention' [1936] one finds Quine acquiescing with the main claim that *Principia Mathematica* had successfully reduced arithmetic to logic.

Tardy though he might have been to seize on Gödel's result to call into question the analyticity of arithmetic, Quine perhaps had a more important reason not to worry himself unduly about the classification or re-classification of arithmetic truth within the famous four-celled box. For he was about to obliterate the internal partitions of that box anyway.[12]

Syntax was about to be published when Quine visited Carnap in Prague in 1933. The Hillman Special Collection at the University of Pittsburgh has some remarkable notes made by Carnap about their exchange on that occasion.[13] Bear in mind that 1951 was the date of publication of Quine's 'Two dogmas of empiricism'. Almost two decades earlier Carnap made this record:[14]

Quine, 31.3.33

[12] As Putnam [1983] has pointed out, Quine's attack on the analytic/synthetic distinction is just as much an attack on the *a priori*/*a posteriori* distinction.

[13] For Quine's reaction to the author's discovery of these notes, see [Quine, 1991].

[14] RC 102-60-12 in the Carnap Archive; author's translation.

> He said after reading my MS 'Syntax': 1. Is there a principled distinction between logical laws and empirical statements? He thinks not. Perhaps though it is only expedient, I seek a distinction, but it appears he is right: gradual difference: they are the statements that we want to hold fast.

With the writing of 'Truth by convention' in 1936, the worry was touched on as follows:

> . . . there is the apparent contrast between logico-mathematical truths and others that the former are a priori, the latter a posteriori; the former have 'the character of an inward necessity' in Kant's phrase, the latter do not. Viewed behavioristically and without reference to a metaphysical system, this contrast retains reality as a contrast between more or less firmly accepted statements; and it obtains antecedently to any *post facto* fashioning of conventions. There are statements which we choose to surrender last, if at all, in the course of revamping our sciences in the face of new discoveries; and among these there are some which we will not surrender at all, so basic are they to our whole conceptual scheme. Among the latter are to be counted the so-called truths of logic and mathematics, regardless of what further we may have to say about their status in the course of a subsequent philosophy.

Those who are unconvinced by Quine's attack on the analytic/synthetic distinction face an interesting challenge: can we sustain an account of arithmetic as analytically true? And if not, why not? Carnap's accommodation or otherwise of Gödel's incompleteness theorems should not end up as a lost starting point for these reflections.

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