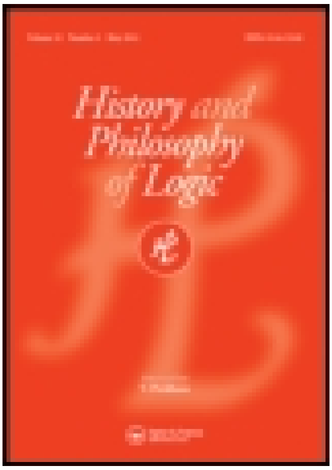


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# Carnap, Tarski, and Quine at Harvard: Conversations on Logic, Mathematics, and Science

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# Book Review

GREG FROST-ARNOLD, *Carnap, Tarski, and Quine at Harvard: Conversations on Logic, Mathematics, and Science*. Chicago, IL: Open Court, 2013. xv + 257 pp. $49.95. ISBN 978-0-8126-9830-5. (paperback)

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In the early 2000s, Greg Frost-Arnold and Paulo Mancosu each independently discovered Rudolf Carnap's shorthand notes of conversations that Carnap had with Alfred Tarski and W. V. Quine during the year 1940–1941, when these three important logicians (along with several others, including Nelson Goodman and Bertrand Russell) were together at Harvard University. *Mancosu* 2005 published a summary of the notes that made clear they contain surprising and illuminating news about the views of Carnap, Tarski, and Quine in 1940–1941. Like many others, I was excited by the news and looked forward to studying the notes myself. Until last year, however, it was not easy to get access to the complete set of the notes, and only a few of them were translated into English. Frost-Arnold's *Carnap, Tarski, and Quine at Harvard* removes these obstacles to studying the notes by making available for the first time in print both a complete German transcription of them and an English translation of the transcription. In addition, in the main body of *Carnap, Tarski, and Quine at Harvard*, Frost-Arnold both explains the historical background and shared assumptions that a reader needs to know in order to understand the conversations, and discusses in detail some of the central positions that are explored in those conversations. The volume as a whole is an extremely useful resource for anyone who is interested in central questions in the foundations of logic and mathematics and the history of analytic philosophy, especially the development of Carnap's, Tarski's, and Quine's philosophies of logic and mathematics.

## 1. Finitism

Carnap's notes touch on a wide range of topics, including transfinite rules of inference; non-standard models of Peano arithmetic; modality; the treatment of quotation marks in formalized languages; the possibility of a 'probabilistic' consequence relation; and the relationship between the notions *state of affairs* and *model* in semantics. One of the discussion topics – a special sort of *finitism* in logic and mathematics – stands out from the others, however, for several reasons, including its strangeness, the number of conversations devoted to it, and the spirit of collaborative inquiry displayed in the conversations about it. As Frost-Arnold explains, 'in these conversations, "finitism" refers to strict requirements Tarski proposes a language must meet to be *verständlich*, that is, understandable or intelligible' (p. 4). Here, for instance, is one key place in the notes where Tarski describes his finitism:

> *Tarski*: I truly understand only a *finite language* $S_1$; only individual variables, whose values are things; whose number is not claimed to be infinite (but perhaps also not the opposite). Finitely many descriptive primitive predicates. *Numbers*: They can be used in a finite realm, in that we can think of the ordered things, and we interpret the numerals as corresponding things. We can then use arithmetical concepts; but many arithmetical sentences cannot be proved here, since we do not know how many numbers there are. (31 January 1941, pp. 156–57)

To understand this remarkable passage and others like it in the notes, one needs to address several related questions, including 'How is Tarski's interest in finitism related to his view of Carnap's analytic–synthetic distinction?' 'What counts as a "thing", according to Tarski?' 'What is Tarski's attitude toward the infinitary arithmetic that is built into classical mathematics?' 'How is Tarski's view of understanding related to his method of defining truth for formalized languages?' and 'How much of infinitary arithmetic can we understand and accept if we restrict ourselves to the resources of Tarski's finitism?' To set the context for some points that I will make below, I will now briefly sketch answers to these questions, drawing on Carnap's notes and Frost-Arnold's discussion of them.

At several places in the notes, Tarski expresses doubts about Carnap's analytic–synthetic distinction, especially (but not only) as it applies to mathematics. For instance, in a conversation that took place in March, 1940, Tarski says he does not have the intuition that there is a fundamental difference between logical and mathematical sentences, which Carnap calls analytic, and 'factual' ones, such as statements about temperature, which Carnap calls synthetic. Carnap objects that 'in the case of a closed sentence containing the physical temperature-functor, we cannot find the truth-value through mere calculation'. Tarski replies, 'That proves nothing, since often that is not possible for mathematical functions either, because there are undecidable sentences; [there is] no fundamental difference between mathematical but undecidable sentences, and factual sentences' (6 March 1940, p. 139). Among other things, this shows that Tarski thought he could not appeal to Carnap's account of analyticity, whether for logic narrowly construed (e.g. first-order quantificational logic with identity), or for logic more widely construed (so that it incorporates mathematics), to clarify infinite arithmetic. (I will return to this point below.) In short, according to Tarski, if arithmetic is infinite, this is a fact on a par with any other fact, including facts about temperature.

Tarski's finite language $S_1$ contains 'only individual variables, whose values are things'. But what does Tarski mean here by 'thing'? In one formulation, he restricts language $S_1$ to quantifying over 'physical things' (10 January 1941, p. 153). As Frost-Arnold points out, however,

> Tarski never articulates precisely what he thinks the 'physical things' are. Three options the group considers are: (i) elementary physical particles, such as electrons etc., (ii) mereological wholes composed of elementary particles (or quanta of energy) (Quine), so that e.g. the objects referred to by the names 'London' and 'Rudolf Carnap' will qualify as physical objects, and (iii) spatial and/or temporal intervals (Carnap). (pp. 5–6)

As Frost-Arnold notes (p. 9), it is clear from these examples that Carnap, Tarski, and Quine did not assume that all physical things over which the variables of $S_1$ range are observable. (I will return to this point below.)

One might expect that given his commitment to finitism, Tarski would simply dismiss infinite arithmetic. Instead, however, Tarski admits to feeling a 'psychological puzzle', which he describes as follows:

> The mathematician also appears to understand arithmetic in a definite sense. In the case of an undecidable sentence (e.g. that of Gödel), they are able, without looking back to the axioms, to say that they recognize this sentence as true. And I (Tarski) share this feeling to a certain degree. (31 January 1941, p. 157)

But how, given Tarski's finitism, are we to explain this feeling of understanding?

Note first that Tarski's finitism prevents him from supposing he can straightforwardly translate sentences of the language of classical infinitary arithmetic – $S_2$ for short – into a metalanguage in which one then can define truth for it. Tarski's method of defining truth for a formalized language is therefore of no help in gaining the sort of understanding of (at least parts of) $S_2$ that he seeks. For this reason, Tarski says he 'understands' $S_2$ only 'as a calculus', hence only in the sense that he knows what he has actually derived from what. As for other higher 'Platonic' statements, he interprets them 'as statements that a fixed sentence is derivable (or derived) from certain other sentences' (10 January 1941, p. 153). Tarski shares the feeling of most mathematicians that they understand $S_2$, then, *only insofar as he knows the formal rules (logical syntax) of* $S_2$; to 'truly' understand $S_2$, according to Tarski, a knowledge of its logical syntax is not enough.

Carnap and Quine both shared Tarski's misgivings about $S_2$, but to different degrees and for different reasons. Carnap writes, 'For me, subjectively: I believe I understand $S_2$ (not completely clearly like $S_1$, but still real understanding, not merely the operation of a calculus)' (31 January 1941, p. 158). As Frost-Arnold plausibly argues (pp. 51–3), however, Carnap had his own reasons for being interested in seeing how far one can take Tarski's finitistic project, and was therefore willing, at least provisionally, to assume that $S_1$ is the only language we completely understand, in the sense that we find it completely clear, and hence the language in terms of which we need to try to make sense of $S_2$.

Quine's attitude toward $S_2$ is more difficult to discern from the notes. In the weeks before Tarski introduces his finitism, Quine sketched an early version of his later, mature view of the differences between logic, mathematics, and physics. In these parts of the notes, he says he conceives of mathematics, like physics, as involving 'hypostases' that are underdetermined by experience but less closely linked than are most laws of physics to any particular observations. When it comes to constructing versions of infinitary mathematics, such as $S_2$, within general set theory, Quine claims, the paradoxes of set theory show that 'familiar common-sense results for finite classes' do not univocally determine a general theory of classes; to develop a general theory of classes 'one must [therefore] consciously search for a myth' about classes. Quine says he searched for a myth after reading about the paradoxes, and he speaks of 'Russell's myth, Zermelo's' (20 December 1940, p. 150). In conversations that occur soon after Tarski announces his finitism, however, Quine seems to share Tarski's misgivings about infinitary mathematics – perhaps because Quine sees it as part of a 'myth'? – and participates in the group's investigations of its consequences. Further evidence that Quine took this finitistic nominalist project seriously is provided by his 1946 paper, 'Steps toward a Constructive Nominalism', which he co-authored with Nelson Goodman, who also participated, though less frequently than Carnap, Tarski, and Quine, in the group's conversations in 1940–1941. In addition, in a letter that Quine wrote to Carnap in 1943, Quine discusses 'the program of finitistic constitution system on which the four of us [i.e. Carnap, Tarski, Quine, and Goodman] talked at intervals in 1941'. He writes

> Tarski and I questioned the precise nature of your distinction between the analytic and the synthetic … I argued, supported by Tarski, that there remains a kernel of

> technical meaning in the old controversy about reality or irreality of universals, and that in this respect we find ourselves on the side of the Platonists insofar as we hold to the full non-finitistic logic. Such an orientation seems unsatisfactory as an end-point in philosophical analysis, given the hard-headed, anti-mystical philosophical temper which all of us share; and, if this were not enough, evidence against the common-sense admission of universals can be adduced also from the logical paradoxes. So here again we found ourselves envisaging a finitistic constitution system. (from Letter 97, 1943-1-3, in *Creath 1990*, p. 295)

In the same letter, Quine also suggests that the finitistic project can be divorced from 'questions of epistemology' if we 'accept, provisionally, whatever rudimentary Platonism may be embodied in our regular logic and classical mathematics' and seek to make 'separate progress' on 'an epistemologically motivated finitistic substructure'. If such progress were made, he reasons, 'it would be a case of resolving the Platonic kink without much altering the existing logical, mathematical, and semantical superstructure, perhaps, just as Weierstraß eliminated the nonsense about infinitesimals without wrecking the differential calculus'.

This characterization of a finitistic project displays an attitude toward 'our regular logic and classical mathematics' that Tarski, Carnap, and Quine also shared in their conversations in 1941. They did not simply dismiss $S_2$ or set theory, but tried to see how much of it could be captured in a finitistic language. In other words, despite their overlapping doubts about how well they understand $S_2$ – doubts that Quine likens in the above passage to doubts about the use of infinitesimals in the differential calculus – Tarski, Carnap, and Quine recognized that, like differential calculus, which presupposes it, $S_2$ is integral to science, and cannot be simply scrapped. They wanted to construct a language $S$ that is both understandable and sufficient for all of science, and hence they could not simply dismiss $S_2$. Such a language would have to be 'rich enough to treat the syntax and semantics of the complete language of science' (10 January 1941, p. 158). Where $M$ is a metalanguage in which the syntax and semantics of 'the complete language of science' is formulated, the requirement is that $S$ be rich enough to express all the statements of $M$ that are needed to formulate the syntax and semantics of 'the complete language of science'. Thus Carnap, Tarski, and Quine converged on the following central problem:

*We together*: So now *a problem*: What sort of part $S$ of $M$ we can take as a kind of nucleus, such that

(1) $S$ is understood in a definite sense by us, and
(2) $S$ suffices for the formulation of the syntax of all of $M$, as far as is necessary for science, in order to treat the syntax and semantics of the complete language of science. (10 January 1941, p. 158)

Their project was to investigate how much of the language of science the 'poor nucleus', namely Tarski's $S_1$, can express. And they asked, 'If $S_1$ does not suffice to reach classical mathematics, couldn't one perhaps nevertheless take $S_1$ and *perhaps* show that *classical mathematics is not really necessary for the application of science in life*?' (10 January 1941, p. 158) In subsequent discussions recorded in the notes they explored this question, but did not arrive at a stable answer to it. Most of their discussions of the question focused on technical points about how to simulate parts of elementary arithmetic in a finite domain of individuals. I will not summarize these points, but will focus instead in the remainder of this review on three points on which I do not completely agree with Frost-Arnold's otherwise excellent commentary on the notes.

**Partial interpretations of a calculus**

Carnap held that in addition to the L-true (analytic) sentences of a calculus (or language system) L suitable for science, there may also be both protocol sentences – i.e. sentences that we may use to report observations – and what I will call theoretical sentences – i.e. sentences that are neither protocol sentences nor analytic in L, but that stand in various indirect relations to protocol sentence, relations mediated by the logic of L. In *Foundations of Logic and Mathematics* (*Carnap 1939*; henceforth, *Foundations*), Carnap proposed that the theoretical sentences of a physical calculus L may be *interpreted* by (first) correlating the terms that occur in them with things (such as physical objects, lengths, and times) and (second) deducing predictions from the theoretical sentences, together with premises that state the results of observations (*Foundations*, §§23–25). The correlations by themselves yield direct, or *complete*, empirical interpretations of the protocol sentences of L. Together with the deductive relations between theoretical sentences and protocol sentences of L, the correlations also yield empirical interpretations of the theoretical sentences. These latter interpretations are only indirect, or partial, however, since theory testing is holistic in the sense that theoretical sentences have empirical consequences only when conjoined with other non-analytic sentences of L (*Foundations*, §24, penultimate paragraph. Carnap highlights the holism of theory testing, citing Duhem, already in §82 of his 1937 *Logical Syntax of Language*; henceforth *LSL*).

It is not easy to see how Carnap's partial interpretation view is related to Tarski's and Quine's concerns about the meaningfulness of infinitary mathematics. Frost-Arnold suggests that 'Quine and Tarski must not have accepted Carnap's partial interpretation view, for if they did, they would not dispute the meaningfulness of higher mathematics' (p. 57).

One immediate problem for this suggestion is that Carnap's standard examples of partial interpretations of the theoretical sentences of a physical calculus presuppose that we understand and can accept infinitary mathematics, whose theorems are, according to Carnap, analytic – i.e. they follow from the semantical rules of a suitably constructed language (such as Language I or II of *LSL*). Carnap stresses that

> The truth of a mathematical theorem, even if it contains descriptive signs, is not dependent upon any facts concerning the designations of these signs. We can determine its truth even if we know only the semantical rules; hence it is L-true [i.e. analytic] … A physical theorem, in contradistinction to a mathematical theorem, has factual content. (*Foundations*, §23)

For Carnap, while we may need a partial interpretation of descriptive signs that occur in a mathematical theorem in order to apply it to physical things, we do not need such an interpretation to assess the truth of the theorem, which follows solely from the semantical rules for the language in which it is expressed. As we saw above, however, Tarski and Quine did not want their understanding of infinitary mathematics to rest on Carnap's analytic–synthetic distinction, which they found obscure. Carnap-style partial interpretations of physical calculi that classify mathematical sentences as analytic therefore do not address Tarski's and Quine's concerns about the meaningfulness of infinitary mathematics.

The problem just sketched is a result of Carnap's decision to adopt what he calls (in *Foundations*, §19) 'the customary interpretation' of mathematics, according to which mathematical sentence are analytic. Strictly speaking, however, Carnap's general method of constructing calculi for science does not *require* that we interpret mathematical sentences as analytic. His method permits us to build physical laws into a calculus by stipulating P-rules, or physical rules, for it (*Carnap 1937*, §51), and one could decide to adopt axioms of infinitary mathematics as P-rules of one's physical calculus, thereby classifying them as among the non-analytic sentences of the calculus. One could then apply Carnap's method of partial interpretation to explain the meanings of the sentences of such a calculus, including the theoretical sentences of infinitary mathematics that follow deductively from the axioms for mathematics that one adopted as P-rules of the calculus. It is therefore tempting to think that Tarski and Quine could have appealed to Carnap's account of partial interpretation to address their concerns about the meaningfulness of infinitary mathematics.

To evaluate this tempting thought, one would need to answer at least two difficult questions that I do not have the space to address here. First, given the restrictions they placed on their project in 1940–1941, could Tarski and Quine have made sense of the modified Carnapian proposal that we simply 'adopt' axioms of infinitary mathematics, with their infinitary ontological consequences, as P-rules of a physical calculus? Second, could Tarski and Quine have accepted the basic aims and structure of Carnap's account of partial interpretation while rejecting his analytic–synthetic distinction even for logic narrowly construed (i.e. first-order logic with identity)? Until these and related questions are answered, we will not be in a position to evaluate Frost-Arnold's interesting suggestion that Tarski and Quine would not have disputed the meaningfulness of higher mathematics if they had accepted (something like) Carnap's account of partial empirical interpretation.



## Quine's interest in finitism

Frost-Arnold sees Quine as motivated to pursue the finitistic program by a kind of 'epistemological foundationalism' that he later recanted in his paper 'Epistemology Naturalized' (pp. 35–6). Frost-Arnold bases this interpretation mainly on the letter to Carnap from which I quoted above. In this letter, Quine says he is more hopeful than Carnap of 'the eventual possibility of …reducing to the form of clear questions the type of inarticulate intellectual dissatisfaction that once drove [Carnap] to work out the theory of the *Aufbau*' (from Letter 97, 1943-1-3, in *Creath 1990*, pp. 294–5), and that he sees Tarski's and his interest in finitism as partly motivated by a concern for 'epistemological immediacy'. It is not clear from the letter, however, which aspects of epistemology interested Quine, and whether his interest was in any important way motivated by an attraction to epistemological foundationalism, as Frost-Arnold believes.

There are at least two good reasons to doubt that Quine's interest in finitism was so motivated. First, as we saw above, and Frost-Arnold points out, the finitism in which Tarski and Quine were interested is not restricted to observable things, but includes 'elementary physical particles, such as electrons etc.', and 'mereological wholes composed of elementary particles (or quanta of energy)'. The predicates that provide us with our understanding of these types of physical objects are far removed from any direct confrontation with experience, and are empirically interpreted only partially, in the way sketched above, by tracing the deductive relations between theoretical sentences in which they occur, on the one hand, and protocol sentences, on the other. For reasons Carnap explained already in *LSL*, §82, the resulting sense of empirical content is not foundational, but holistic.

Second, in his 'Intellectual Autobiography' in 1986, long after he published 'Epistemology Naturalized', Quine writes that the finitistic nominalism of 'Steps Toward a Constructive Nominalism' – a nominalism that 'get[s] mathematics into an ontology strictly of physical objects' – would be his 'actual position' if he 'could make a go of it', or, in other words, if he could show that such a nominalism is able to express all the mathematics we need for science. This remark from 1986, 45 years after the discussions at Harvard about finitism, suggests (though does not definitively establish) that Quine had a stable, long-term preference for nominalism, conditional on its being able to express the mathematics we need for science. The remark therefore suggests that contrary to Frost-Arnold's interpretation, Quine's motivations for pursuing the finitism project, both early and late, were independent of any commitment to foundationalism in epistemology.

## Quine's criticisms of Carnap's analytic–synthetic distinction

Some of Quine's criticisms of Carnap's analytic–synthetic distinction focus on the question of whether the distinction can be expressed in extensional terms. Was Quine solely responsible for raising the question in this way or did Carnap himself also regard it as an important question? As Frost-Arnold points out, in *LSL* Carnap was committed to a *thesis of extensionality*, according to which 'for every given intensional language $S_1$, an extensional language $S_2$ may be constructed such that $S_1$ can be translated into $S_2$' (Carnap, *LSL*, §67, p. 245). In *Introduction to Semantics*, which Tarski and Quine read in manuscript during the year 1940–1941, Carnap offers a definition of 'value range', and thereby, also, of 'L-true' (his explication of 'analytic in L'), in an extensional metalanguage. These points shed light on Quine's 'Two Dogmas of Empiricism', especially Section 4, in which Quine argues, in effect, that Carnap's definitions of 'L-true' in terms of semantic rules, while presented in what appear to be purely extensional terms, fail to draw a boundary between analytic and synthetic. For these reasons, I agree with Frost-Arnold that to understand Quine's criticism of Carnap's analytic–synthetic distinction, it helps to ask why Quine proceeds as if the distinction, if it exists, can be drawn in extensional terms.

Frost-Arnold goes beyond this first, helpful observation, however, to conjecture that 'the radical critique of analyticity that Quine advocates by 1950 is perhaps as much a product of Carnap changing his views (toward fundamentally intensional, specifically modal approaches, away from exclusively extensional ones) as Quine changing his' (p. 111). This conjecture is doubtful, I think. Quine first clearly formulated his concerns about how to draw Carnap's analytic–synthetic distinction in 1943, in the same letter to Carnap that I have quoted from above. Quine explains in that letter that 'the root of the difficulty' with Carnap's analytic–synthetic distinction is 'the assumption of a thoroughgoing constitution system, with fixed primitives and fixed definitions of all other expressions, despite the fact that no such constitution system exists' (p. 296). Quine does not complain that the terms in which Carnap's explications of 'analytic' are given are not extensional, but that the explications depend on 'the assumption of a thoroughgoing constitution system'. He later developed this objection in Section 4 of 'Two Dogmas of Empiricism', where the problem becomes that of defining 'analytic in L' for *variable* L. For each L, one can lay down an extensional definition of 'analytic in L', but only by arbitrarily stipulating semantical rules for L; since the choice of the rules for any given L is arbitrary, the resulting explications of 'analytic in L' do not give us any insight into the what 'analytic' means for variable L, beyond that the analytic sentences are true. This criticism of Carnap's explications of 'analytic' does not focus on Carnap's increasing tolerance for 'fundamentally intensional, specifically modal approaches'. Quine's criticism in Section 4 of 'Two Dogmas of Empiricism' applies to Carnap's self-consciously *extensional* explications of 'analytic' in such works as *Carnap 1939* and *Carnap 1942*. We now also know that this issue came up explicitly in one of the recorded conversations from 1941: Carnap states that 'L-concepts', which, of course, include 'L-true', are 'easy to define' using extensional resources that Quine accepts, since we can simply 'give the *logical constants* through *enumeration*' (31 January 1941, p. 156). It is reasonable to suppose that remarks like this sparked Quine's efforts to see if the notion of 'L-true' can be explained in extensional terms, and that he gradually came to the conclusion that 'L-true' cannot be so explained.

Despite these quibbles, on the whole I found Frost-Arnold's commentary on the notes extremely helpful and illuminating. He has done a great service, also, by preparing and publishing in the same volume a complete German transcription of the notes and a complete English translation of the transcription. In short, Frost-Arnold's *Carnap, Tarski, and Quine at Harvard* makes a major contribution to our understanding of Carnap's, Tarski's, and Quine's philosophies of logic and mathematics.

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