

Review of *Carnap, Tarski, and Quine at Harvard*

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Greg Frost-Arnold, *Carnap, Tarski, and Quine at Harvard*. Chicago: Open Court (2013), 207 pp., \$49.95 (paper).

Greg Frost-Arnold's recent work *Carnap, Tarski, and Quine at Harvard* is a well-researched and informative analysis of an important event in the history of analytic philosophy. During 1940–41, three major figures in the field met to discuss the possibility of a finitist-nominalistic language of science. What emerged during this brief collaboration was a discussion that weaved together the unique philosophical approaches that each of Carnap, Tarski, and Quine took to logic, mathematics, science, epistemology, and metaphysics. Frost-Arnold's survey of these discussions not only highlights the project itself but also provides valuable insights into broader philosophical and historical issues surrounding these three men.

At root, this book is an analysis of the finitist-nominalist (FN) project that emerged during this time of collaboration. But this book is also so much more. Frost-Arnold expands on certain aspects of the FN project and connects them to historical issues such as the Carnap/Quine debate on analyticity, the project of the unity of science, the elimination of metaphysics, and the principle of tolerance, as well as contemporary issues in the philosophy of mathematics. As such, this book is of value to not only historians who wish to see more of the pieces put together but also philosophers of mathematics and logic who are currently grappling with epistemic and metaphysical issues similar to those faced by Carnap, Tarski, and Quine in 1940–41. Frost-Arnold has also included English and German versions of Carnap's

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notes taken at this time. Such a resource should prove invaluable to Carnap scholars.

The opening chapter discusses the influences that led Carnap, Tarski, and Quine to take an interest in the FN project. After surveying the writings of Chwistek, Kotarbinski, and Lesniewski, it becomes easy to see why Tarski would be interested in nominalism, as all three held that higher mathematics is equivalent to Platonism. This thought that higher mathematics is metaphysical is joined by Russell's labeling of numbers as "fictions of fictions" (22). Russell was at Harvard in 1940, and although he did not directly engage with Carnap, his influence was obviously present. All of the participants of the discussions were motivated by a desire to show that mathematics was not, in fact, meaningless metaphysics: "part of the motivation for undertaking the finitist-nominalist project is to demonstrate that (at least a substantive chunk of) mathematics is not metaphysics, but rather cognitively meaningful" (23). This motivation runs throughout the book, with the sixth and final chapter connecting the issues of understandability, language, the unity of science, and the elimination of metaphysics.

Another, somewhat unique, motivation for the FN project is discussed in chapter 2. "Tarski claims that a language must meet his finitist-nominalist restrictions in order to qualify as 'fully understandable' or intelligible" (27). The worry is that the language of higher mathematics, containing infinities and abstracta, is not understandable. All three figures had their own interpretation of 'understandable' and their own motivations for making mathematics understandable. Interestingly, if one assumes a notion of understandability that makes only the unified language of science understandable, then this goal becomes identical with the antimetaphysical motivation discussed above. This interconnection of philosophical ideals is typical of Frost-Arnold's narrative.

These motivations make sense for Tarski and Quine but raise an interesting challenge for Carnap. Given his principle of tolerance as a prohibition on restrictions of language form—"It is not our business to set up prohibitions, but to arrive at conventions" (1934/2002, 51)—why would Carnap be at all interested in the FN prohibitions that Tarski proposes? The answer to that depends on what you want a formal language to do, a point that I make in Hillier (2010, 28). Carnap is committed to the belief that "the proper task of philosophical work [is] the logical analysis of knowledge, i.e. of scientific sentences, theories, and methods, that is, *the logic of science*" (1935, 37). The FN prohibitions would be of interest to Carnap if they helped to clarify the logic of science, if they provided more accurate models of scientific language (Frost-Arnold recognizes Carnap's interest in modeling the language of science on 68).

We find another distinctly Carnapian motivation in chapter 3. Throughout his career, Carnap was interested in the relation between the observational

and the theoretical parts of the language of science. As a logical empiricist, Carnap identified the observational parts of a language as understandable, so he was immediately intrigued by Tarski's discussion of "fully understandable" languages. "Thus, when Tarski talks in 1941 about the only kind of languages he 'truly understands,' and tries to build a serviceable scientific language out of such languages, Carnap links this to his own previous work on the connections between theoretical language and observational language developed in 'Testability and Meaning'" (53). This makes Tarski's FN project appealing as a potential model for mathematics in a unified language of science, which explains Carnap's interest in it.

Chapters 4 and 5 deal with that mainstay of Carnap/Quine scholarship: the analytic/synthetic distinction. There is a brief discussion about one of Carnap's worries about the FN project, namely, that arithmetic would become synthetic under Tarski's proposal. This turns out to not be as damaging as one might expect, since under Carnap's distinctive interpretation of analyticity in the 1940s a significant portion of arithmetic maintains this status under the FN project. Contrary to what one might first assume, FN does not make the entirety of the discipline synthetic. Furthermore, even if it did, this may not be worrying at all to Carnap. In "Formal and Factual Science," Carnap claims that "it is possible to reconstruct the language of science in such a manner that it contains only synthetic statements. This need not diminish the content of science" (1934/1953, 126). Such a reconstruction, however, would be a very poor model of the actual language of science, wherein mathematical claims do play the special role that analyticity is intended to capture.¹ This negative attitude toward the FN project does not violate the principle of tolerance, as "tolerance does not require every formal language to model every natural language equally well" (68).

Another interesting point in these chapters is Frost-Arnold's discussion of Quine's views of analyticity. He begins by supporting Creath's assertion that "Quine's position in 'Truth by Convention' itself is probably not diametrically opposed to Carnap's position" (82). Throughout the 1930s and 1940s, Quine still harbored hope that there could be a meaningful analytic/synthetic distinction, although he knew that it faced significant issues. In 1951's "Two Dogmas of Empiricism," these doubts had grown into full-on denial. What prompted the change? Frost-Arnold rejects the view held by some that the doubts of 1936's "Truth by Convention" themselves grew into the denial of 1951's "Two Dogmas." Instead, he convincingly argues that

1. Logic and mathematics are thought of as contentless, formal auxiliaries that serve only to facilitate inferences between synthetic sentences. It is this feature that Carnap is trying to capture with his formal definitions of analyticity (see Hillier 2009, 411–12).

the 1940–41 discussion of the FN project provided a major impetus for this shift with Quine’s realization that “apparently analytic sentences could be synthetic” (83–84, see also 86–87). This observation forced Quine to accept that there could be no meaningful distinction if even those paradigm cases of analytic sentences, mathematical sentences, could become synthetic. Quine’s break from Carnap is also influenced by the latter’s changing views on intensional languages, a point made in chapter 5.

The final chapter on the unity of science begins with the interesting claim that the goals of providing a unified language of science and of elimination of metaphysics “are two sides of the same coin” (117), that the “elimination of metaphysics is the negative or destructive part, while the production of a unified scientific language constitutes its positive or constructive aspect” (136). On the basis of the motivations for the FN project given in chapter 1, it is the role of the unification of the language of science in the elimination of metaphysics that is of most interest here.

The elimination of metaphysics is not as straightforward as applying the Verificationist Criterion of Meaning (as Ayer would suggest), although that does play a supporting role in the overall project. Throughout Carnap’s career, he (and other members of the Vienna Circle like Neurath) embraced something similar to this criterion of metaphysics:

(M) An apparently declarative sentence or apparently descriptive term is *metaphysical* if and only if that (apparent) sentence or term *cannot be incorporated into a total language of science.* (123)

Underlying this criterion is a form of “semantic foundationalism,” which holds that “there are sentences and/or terms that function as the ultimate source of meaning for all sentences” (127). Verificationism is the added statement that observation sentences are semantic foundations. With this antimetaphysical criterion in hand, we can now understand why the FN project would show that mathematics is not metaphysics. In order to show this, mathematics must be incorporated into a unified language of science, one with an understandable semantic foundation. That is precisely what the FN project hopes to provide, with FN criteria for understandability.

At the end of the day, this is a substantial contribution to Carnap scholarship. Frost-Arnold cleverly weaves together several strands of Carnap’s thought: the unity of science, the rejection of metaphysics, and the logical analysis of the language of science are all deeply connected to the 1940–41 discussions of the FN project. The extensive discussion of Quine’s break from Carnap regarding analyticity, and the role that the FN project played in it, should once again encourage us to revisit this seminal event in the history of analytic philosophy.

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