QUINE'S EVOLUTION FROM 'CARNAP'S DISCIPLE' TO THE AUTHOR OF "TWO DOGMAS"

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Recent scholarship indicates that Quine's "Truth by Convention" does not present the radical critiques of analytic truth found 15 years later in "Two Dogmas of Empiricism." This prompts a historical question: What caused Quine's radicalization? I argue that two crucial components of Quine's development can be traced to the academic year 1940–41, when he, Russell, Carnap, Tarski, Hempel, and Goodman were all at Harvard together. First, during those meetings, Quine recognizes that Carnap has abandoned the extensional, syntactic approach to philosophical analysis, an approach espoused in Carnap's 1934 *Logical Syntax of Language* and that Quine endorsed his entire career. Second, Tarski presents Quine with a philosophically well-motivated reason to think that an apparently analytic discipline, arithmetic, could be synthetic; this reflects one of the central assertions found in "Two Dogmas" but not in "Truth by Convention." I use this account of Quine's development to resolve a dispute between Creath and Mancosu concerning the timeline for Quine's evolving critiques of analyticity.

1. Introduction

How did Quine get from "Truth by Convention," published in 1936, to 1951's "Two Dogmas of Empiricism"? One might consider this question ill posed: both essays are commonly read as powerful attacks on the very idea of

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analytic truth, so how could there be any conceptual distance between the two? In section 2, I present evidence—some already in the literature, some previously unnoticed—that Quine had not embraced the radical rejection of analytic truth found in "Two Dogmas" by the time "Truth by Convention" was published.

If in fact Quine had not yet completely rejected the type of analyticity Carnap favored by 1936, the natural historical question to ask next is, What caused the more radical change manifested in "Two Dogmas"?[1] I will argue that two partial causes are found in the 1940–41 academic year, when Quine, Russell, Carnap, Tarski, Hempel, and Goodman were all at Harvard together and met regularly to discuss philosophical and logical topics of shared interest; Carnap took detailed notes of these conversations. First (see sec. 3), Tarski presented to the group a proposal for a species of mathematical nominalism, which was discussed at length in several sessions. In this nominalist system, certain statements of arithmetic—a discipline usually considered analytic since Frege—turn out to be synthetic. We know Quine took Tarski's proposal seriously because Quine tells us that these discussions with Tarski were the initial motivation for his own published version of nominalism (Quine and Goodman 1947, 112). But the notion that apparently analytic sentences could be synthetic foreshadows a central claim found in "Two Dogmas" but not "Truth by Convention": no assertion is forever immune from revision, even the intuitively analytic statements of logic and arithmetic. Second (see sec. 4), during these discussions, Quine saw clearly that Carnap had begun to conceive of analytic truth (and other logico-linguistic concepts) in intensional and semantic terms, instead of the extensional and syntactic framework endorsed in 1934's *Logical Syntax of Language*. Quine, however, strongly espoused the extensional approach to analysis his entire career and preferred syntactic analyses to semantic ones. To summarize, the importance of these 1940–41 conversations for Quine's development is, first, that Carnap adopted a modal and semantic approach to explicating analyticity, which Quine found unappealing, and,

1. After "Two Dogmas," Quine does not permanently consider every conception of analyticity unacceptable. After the extremely strong rejection of analyticity in "Two Dogmas," his views soften somewhat in later years. For example, he claims that certain sentences are 'stimulus analytic' in *Word and Object* (1960), and he offers another characterization of analyticity in *Roots of Reference* (a sentence is analytic if a speaker learns to assent to it by learning at least one of the words of the sentence; Quine 1974, 80). However, the Quinean types of analyticity cannot do the epistemological heavy lifting that Carnap and others use the notion of analytic truth to do. "I suggested in *The Roots of Reference* … an intelligible and reasonable notion of analyticity. However, I see little use for it in epistemology or methodology of science" (Quine 1986, 95). Peter Hylton has stressed this as well: "[Quine] rejects the idea that there is a defensible distinction *which will play the role that Carnap allotted it*" (2007, 53; cf. 2001).

second, that Quine sees that apparently analytic sentences could be considered synthetic. After detailing this picture of Quine's journey from "Truth by Convention" to "Two Dogmas," I present in section 5 a further virtue of this account: it can be used to resolve a dispute between two leading commentators concerning when, precisely, Quine reached the radical view of analyticity expressed in "Two Dogmas."

2. "Truth by Convention" Is Not a Radical Break with Carnap

Richard Creath has argued that Quine's 1936 "Truth by Convention" should not be read as a full-frontal assault on the intelligibility, applicability, or usefulness of the notion of analytic truth. He argues that such a reading is anachronistic and arises from the temptation to read the more radical criticisms of analyticity found in "Two Dogmas" into an article written 15 years earlier. On Creath's interpretation, "Truth by Convention" is better viewed as "more nearly a request for clarification than an attack" (1987, 487) on Carnap's notion of analytic truth.

What is the evidence that Quine was not convinced the concept of analyticity and its kin are fundamentally ill conceived until years after "Truth by Convention"? Creath points out, first, that "Truth by Convention" grew out of three lectures Quine gave to the Harvard Society of Fellows in 1934: certain portions of that article follow very closely the text of the lectures.[2] These lectures praise Carnapian views almost unequivocally.[3] Quine himself later describes these three lectures on Carnap as "abjectly sequacious" (1991, 266). So, Creath reasons, if the document that "Truth by Convention" grew out of is very sympathetic to Carnap's position, then Quine's position in "Truth by Convention" itself is probably not diametrically opposed to Carnap's views. But this is not conclusive, since Quine could have decisively rejected Carnapian analyticity after his 1934 lectures but before writing "Truth by Convention," despite surface similarities between the two documents. Thus, Creath offers a second piece of evidence: at the 1937 American Philosophical Association Meeting, Quine gave a lecture entitled "Is Logic a Matter of Words?" In it, Quine argues for (what he later calls) the 'linguistic doctrine of logical truth', which is associated with Carnap's position. So we have evidence of Quine defending Carnap's views both shortly before and immediately after he wrote "Truth by Convention."

2. For further analysis of these lectures, see Hylton (2001).

3. Yemima Ben-Menahem suggests that we can find certain seeds of Quine's later critique of Carnapian analyticity in the 1934 lectures (2006, 226–29).

Consider a third batch of textual evidence for Creath's view. During the greater part of the 1940s, in Quine's published (Quine 1943, 120) and unpublished writings (Creath 1990, 298 and 332), his attitude toward analyticity is one of growing skepticism, not the dismissal that we find in "Two Dogmas." For example, as late as 1947, Quine claims to give an "interpretation of prequantificational modal logic" in terms of analyticity: "The result of prefixing 'Necessarily' to any statement is true if and only if the statement is analytic" (1947, 45). Presumably, one would not offer an interpretation of modality in terms of analyticity if one considered analyticity to be thoroughly incomprehensible or otherwise unacceptable. And in the same article, Quine calls the suggestion (which he there attributes to Goodman) that the analytic-synthetic distinction is merely a matter of degree a "dismal possibility" (n. 4), so it seems that Quine had not yet abandoned all hope for an explication of analyticity that could do heavy lifting in epistemology, even if he felt it had not yet received a satisfactory explanation. This hypothesis finds further confirmation in Quine's December 1946 lecture: "The ideas which I have offered … this hour have been mainly negative: the obscurity of our conception of analytic [*sic*], and the difficulty of doing anything about it. … But I want to say in closing that my attitude is not one of defeatism, nor one of dismissing the problem as illusory. We have real problems here, meaningful problems worth working on. My feeling is … that we should recognize that we have not been doing very well, but not that we should give up trying" (Quine 2008, 35). Clearly, Quine had not yet admitted defeat in his attempts to draw the analytic/synthetic distinction at this point in time. And furthermore, he believed it a 'real' and 'meaningful' task worth undertaking. In short, throughout the 1940s, Quine showed a reluctance to accept and endorse any notion of analyticity found in Carnap's contemporaneous writings, but he had not yet reached the mature view we find in "Two Dogmas."

A final piece of historical evidence for viewing Quine's critique in "Truth by Convention" as less radical than that of "Two Dogmas" is quite telling, and it provides a clue as to when and where Quine broke with Carnap's notion of analytic truth. In "Homage to Carnap," his eulogy for Carnap at the 1970 Philosophy of Science Association meeting, Quine says he first contacted Carnap "in Prague 38 years ago," which would be fall 1932, and that he, Quine, "was very much Carnap's disciple for six years" (Creath 1990, 464). This entails that "Truth by Convention" was composed during the period of Quine's life in which he considered himself a disciple of Carnap, so "Truth by Convention" probably should not be viewed as fundamentally rejecting one of Carnap's most cherished ideas.

In what follows, I proceed on the assumption that these four pieces of evidence above are conclusive: that is, I assume that "Truth by Convention"

presents, in some important sense, a less radical challenge to analytic truth than "Two Dogmas." That is, Quine has not completely rejected the notion of analyticity that Carnap hopes for in 1936. And this is reflected in the differing views expressed in "Truth by Convention" and "Two Dogmas": "Two Dogmas," unlike "Truth by Convention," makes the more radical claim that no intelligible explication of analytic truth can be found. Furthermore, "Two Dogmas," unlike "Truth by Convention," suggests the radical thesis that even mathematics and logic are not analytic.[4] If this picture of the difference between "Truth by Convention" and "Two Dogmas" is correct, then this immediately raises a historical question: What, if anything, prompted this radicalization of Quine's attack on analyticity? How did Quine get from the more moderate "Truth by Convention" to "Two Dogmas"?

One might think that Quine's transformation is just the result of time and reflection: all the conceptual ingredients for his rejection of Carnap's notion of analytic truth are present in "Truth by Convention." They simply needed time to 'ferment' or mature in Quine's mind to produce a final, decisive break 15 years later; Creath hints at such a picture (1990, 31), and Yemima Ben-Menahem expresses the thesis more explicitly (2006, 229). Although some crucial ingredients of the later break are undoubtedly present in "Truth by Convention,"[5] there is good reason to question this account. A revealing sign is found in the quotation from Quine's "Homage to Carnap" above: he says that he was Carnap's disciple for 6 years. This probably means the years 1933–38, inclusive, which obviously includes "Truth by Convention," published in 1936. Now the natural question to ask is, what happened in 1939 that could end a 6-year discipleship? A few paragraphs later in the "Homage," Quine tells us: "In 1939 Carnap came to Harvard as visiting professor. These were historic months: Russell, Carnap, and Tarski were here together. Then it

4. There are, of course, further differences between "Two Dogmas" and "Truth by Convention"; e.g., Quine is not yet concerned about synonymy in "Truth by Convention," whereas it is the target of much discussion in "Two Dogmas."

5. Two such ingredients deserve mention. First, Quine suggests that we should understand analyticity to be an empirical concept, specifically, a behavioristic one. This contrasts with Carnap, who held that analyticity (like other legitimate philosophical concepts) should be understood as a nonempirical concept (see Ricketts 1982 and Friedman 1999, chap. 9, for discussion). Interestingly, Quine held Carnap's view in the 1934 lectures, that ascriptions of analyticity are themselves analytic: Quine says that "it is … a matter of linguistic convention *which* sentences we are to make analytic and which not" (Creath 1990, 64; this marks an important difference between the 1934 lectures and "Truth by Convention" 2 years later). Second, Quine argues that, if we allow ourselves to declare some sentences true by linguistic fiat, then there is no principled cutoff point to prevent us from declaring any sentence true by convention. In "Two Dogmas," the combination of the first and second points becomes the claim that any sentence can be held true come what may.

was that Tarski and I argued long with Carnap against his idea of analyticity" (Creath 1990, 466).

Quine's memory is not entirely accurate here. The academic year in which Carnap, Tarski, and Russell visited Harvard was 1940–41, not 1939–40; Carnap and Tarski (along with Hempel) stayed the entire year, while Russell was present for the fall semester only. Despite this minor misremembering on Quine's part, it seems this 'historic' clash of philosophical titans could well mark the end of Quine's 6-year discipleship under Carnap, especially since Quine recalls, more than 3 decades later, arguing about analyticity with Carnap then. Quine makes similar remarks in his autobiography (although he recalls the dates correctly there).[6]

So perhaps an important part of Quine's transition from "Truth by Convention" to the more radical "Two Dogmas" can be traced to these conversations at Harvard during 1940–41. Fortunately for us, Carnap took detailed dictation notes of these conversations, now stored in the Rudolf Carnap Collection. However, scholars have only recently begun to study these notes (in, e.g., Mancosu 2005, 2008), and few people working in the history of analytic philosophy knew of their existence until a few years ago. I will use these notes to support the hypothesis that the 1940–41 conversations played an important role in radicalizing Quine's criticisms of analytic truth.

3. In Tarski's Nominalist Language, Apparently Analytic Claims Become Synthetic

What do we find in these dictation notes Carnap took in 1940–41? The group's discussions range across several topics in logic and scientific philosophy, but the largest portion is dedicated to exploring (what we today would call) a nominalist language for mathematics and science. The issue of nominalism (which the discussants usually label 'finitism') appears in these conversations when Tarski proposes rather strict requirements a language must meet in order

6. Quine writes: "The fall term of 1940 is graven in my memory for more than just the writing of *Elementary Logic*. Russell, Carnap, and Tarski were all at hand. … My misgivings over meaning had by this time issued in explicit doubts about the notion, crucial to Carnap's philosophy, of an analytic sentence: a sentence true purely by virtue of the meanings of its words. I voiced these doubts, joined by Tarski, before Carnap had finished reading us his first page [of his manuscript for *Introduction to Semantics*]. The controversy continued through subsequent sessions and without progress in the reading of Carnap's manuscript" (1985, 149–50). Unfortunately, the final sentence is misleading: the group did advance past the first page of Carnap's manuscript. Carnap's notes record a discussion of the adequacy of a particular definition that appears in chapter 17 of the manuscript of *Introduction to Semantics*, which becomes definition 18-1 in the published version (Rudolf Carnap Collection, Archives for Scientific Philosophy, University of Pittsburgh, 090-16-03; hereafter RCC).

for it to qualify (in Tarski's eyes) as *verständlich*, that is, understandable or intelligible.

Tarski's proposal varies somewhat from meeting to meeting. Carnap records the first version of it as follows.

> January 10, 1941. … Tarski: I only understand at bottom a language that fulfills the following conditions:
>
> 1. Finite number of individuals.
> 2. Reistic (Kotarbinski): the individuals are physical things.[7]
> 3. Non-Platonic: Only variables for individuals (things) occur, not for universals (classes etc.). (RCC 090-16-28)

Three weeks later, Tarski offers a similar, although not identical, characterization of a language he considers completely understandable.

> Tarski: I really understand only a finite language S_1:
> only individual variables [cf. condition 3 above],
> whose values are things [2 above],
> whose number is not claimed to be infinite (but perhaps also not the opposite) [modified version of 1].
> Finitely many descriptive predicates [new requirement]. (RCC 090-16-25)

We can summarize these restrictions in current terminology as follows: an intelligible language must (i) be first order, (ii) have only "physical things" in its domain of discourse, (iii) make no assumption about the cardinality of its domain of discourse, and (iv) contain only finitely many descriptive predicates.[8] Only languages meeting these four restrictions, Tarski says, are fully understandable or intelligible.[9]

7. For further information on Kotarbinski's reism, see Kotarbinski (1935/1955), which Tarski helped translate into English.

8. There is not much discussion of what exactly counts as a 'physical thing': the discussants consider at least elementary physical particles, mereological sums of such particles, and spatiotemporal intervals (RCC 090-16-23).

9. These restrictions may strike current readers as rather severe. What would motivate Tarski, Quine, and Carnap to investigate such strictures on mathematical discourse? Part of the historical answer may include Russell's attitude toward numbers: he calls them "fictions of fictions" (1918/1956, 270) and holds that their introduction (so long as they are not constructed) is a piece of gratuitous metaphysics: "So long as the cardinal number is inferred from the collections, not constructed in terms of them, its existence must remain in doubt, unless in virtue of a metaphysical postulate ad hoc" (Russell 1918, 156). Given Russell's towering stature in the field at the time, his views likely played some role in determining which positions were worth taking seriously.

Any language meeting these four constraints will have trouble capturing even basic arithmetic. Why? Since numbers are abstract, one cannot introduce numbers qua individuals into the domain of discourse because of condition ii. The same condition prohibits introducing numbers as sets of sets, à la Russell, since sets are abstract objects too. Finally, one cannot introduce numbers as properties of properties, à la Frege, because the language must be first order. Carnap, Tarski, and Quine spend a good deal of time and energy in these notes discussing how best to capture as much arithmetical discourse as possible in such a language. The basic semantic idea is to assign an arbitrary physical object to each numeral as that numeral's denotation. Problems for reconstructing arithmetic arise when we 'run out' of physical objects; that is, how should we interpret extremely large numerals, if there are only a finite number of physical objects in the universe?

Much more could be said about this nominalist project.[10] But for present purposes, the only question we need to ask is, what does Tarski's nominalism have to do with Quine's critique of analytic truth? Carnap unhappily notes that in a Tarskian language "arithmetic becomes dependent on contingent facts"—that is, parts of arithmetic become synthetic, instead of analytic (RCC 090-16-23). Precisely which sentences become synthetic depends on the details of the semantic rules dealing with numerals that outstrip the number of physical objects in the world, but 'There are infinitely many numbers' will be a synthetic claim in any Tarskian language. Why? If there are an infinite number of physical things in the universe, then the sentence will be true, but if there are finitely many physical objects in our world, then the sentence will be false, and by iii above, we can make no assumptions about the cardinality of the domain of discourse in a Tarskian language.

How does the fact that portions of arithmetic become synthetic under Tarski's nominalist proposal relate to the radicalization of Quine's critique of analyticity? To answer that question, we should examine Quine's arguments in "Truth by Convention" more closely. There, one way in which Quine places pressure on the analytic/synthetic distinction is as follows. Suppose one claims to establish the truths of logic by simply declaring (every instance of) certain sentence forms involving 'and', 'not', and 'all' to be true by "linguistic fiat" (e.g., every instantiation of the sentence form ⌜Not(p and not-p)⌝). Quine then asks: if we are allowed to declare these particular sentences true simply

10. For the conceptual or philosophical explanation for Tarski's strictures, see Mancosu (2008), and see Frost-Arnold (2008) for more on Tarski's nominalism in particular. Wolenski (1993) provides an overview of Tarski's wider philosophical commitments; on Polish nominalism more generally, see Simons (1993).

by linguistic fiat, why could we not continue expanding this list of conventional truths and include, for example, Einstein's field equations in our list of sentences that are true by convention? And there is no reason to stop with fundamental physical laws: as long as we have the power of linguistic fiat to make any sentence we like true, we could include 'Earth is the third planet from the Sun' or 'Oliver Cromwell died in 1658'. "If in describing logic and mathematics as true by convention what is meant is that the primitives can be circumscribed in such a fashion as to generate all and only the truths of logic and mathematics, the characterization is empty; … the same might be said of any other body of doctrine as well" (Quine 1976, 102).

In short, Quine questions the existence of a sharp, reasonable, and motivated cutoff point for statements considered true by convention that would prevent an indefinite expansion of such truths beyond the realm of logic and perhaps mathematics.[11] He cannot see any special quality that the terms 'or' and 'not' possess that (for example) 'mass-energy density' lacks, such that instances of sentence forms involving only the former but not the latter as constants can legitimately be simply stipulated true. We can read Quine as making a slippery slope argument: once we permit one sentence to be true by linguistic fiat, there is no principled ground for stopping unlimited inflation of such truths. This line of attack on analyticity takes a more exact form in "Elimination of Extra-logical Postulates," penned jointly with Goodman in 1940. This article provides a formal procedure for converting any system of postulates framed in a formal language into a postulate-free language that has, in an important sense, the same content as the original postulate system. Quine later improved this formal recipe (1964).[12] So Quine held fast to the idea that if one sentence could be made true by linguistic convention, then there is no principled way to prevent an indefinite expansion of such truths.

11. A few years later, Quine will not even allow that the truths of mathematics can be so circumscribed. He takes Gödel's incompleteness results to show that we "can't even formulate adequate, usable conv'ns afterward" since no single logical system captures all the logico-mathematical truths (W. V. Quine Papers, Houghton Library, Harvard University, MS storage 299, box 12, folder: Phil. 20m-1940; hereafter Quine Papers). It is perhaps worth noting that one of the primary goals of *Logical Syntax* was to develop a philosophical framework for analyticity in the face of the apparent problems created by Gödel's incompleteness theorems. In particular, are the true but unprovable sentences of Peano arithmetic being analytic? This is a prima facie problem for Carnap, because previously he and other defenders of analyticity could maintain that 'analytic' was coextensive with the mathematically respectable term 'provable'.

12. Quine writes: "Briefly, the point is that there is a mechanical routine whereby, given an assortment of interpreted undefined predicates 'F_1' … 'F_n,' governed by a true axiom or a finite list of such, we can switch to a new and equally economical set of undefined predicates and define 'F_1' … 'F_n' in terms of them, plus auxiliary arithmetical notations, in such a way that the old axioms become true by arithmetic" (1964, 71).

Now, in the 1940–41 conversations, Quine is presented with the converse possibility. Instead of expanding the conventional, and thus analytic, truths from logic (and mathematics) into natural science, Tarski proposes a philosophically motivated language form in which the number of supposedly analytic truths is contracted. When Tarski presents Quine with a scenario that appeals to Quine's nominalist scruples in which arithmetical assertions become synthetic, this shows him concretely that the boundary between the analytic and the synthetic can be considered porous in both directions.[13] In "Truth by Convention," in contrast, only one of the directions is considered, and the analytic status of logic and mathematics is not in doubt.[14] After stating that the behavioristic criterion of a sentence's analyticity is its being held true come what may,[15] Quine writes: "There are statements that 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 of their status in the course of a subsequent sophisticated philosophy" (1976, 102; my emphasis).

That is, in "Truth by Convention," Quine still considers logic and mathematics analytic because we will not give them up—his worries involve

13. An anonymous reviewer for this journal highlighted the following important reservation concerning to the importance of Tarski's finitist-nominalist arithmetic for the notion that the analytic truths could be contracted. The revolution in geometry, occasioned by the rise of non-Euclidean geometries and culminating in Einstein's General Theory of Relativity, could be seen as another instance in which empirical considerations led scientists to contract the number of analytic truths (in particular, the parallel postulate). However, as the reviewer also noted, the thesis that geometry is analytic did not hold the same sway that the analyticity of arithmetic did in Quine's milieu. Nonetheless, the example of geometry shows that Tarski's arithmetical project is not completely singular.

14. Quine himself was fully aware of this. He states that "Truth by Convention" did not claim that there are no analytic truths (1960, 65n). Additionally, another important difference between "Two Dogmas" and "Truth by Convention" should be noted because the simple claim 'the class of analytic truths can be indefinitely expanded' actually ignores an important distinction. In "Truth by Convention," Quine only applies the 'indefinite expansion' argument to analyticity construed as truth in virtue of linguistic fiat. Interestingly, in "Truth by Convention," Quine does not apply this indefinite expansion argument against the alternative behavioristic characterization of analyticity mentioned above—he apparently does not think in 1936 that the class of sentences that 'we will not surrender' can be indefinitely expanded. So this is another step that Quine must take in his journey from "Truth by Convention" to "Two Dogmas": the indefinite expansion argument does not apply only to the linguistic-fiat conception of analytic truth.

15. Whence Quine's view that a priori (and thus, for him and the logical empiricists, analytic) claims can be thought of as claims that can be held true come what may? Interestingly, this phrase echoes C. I. Lewis, Quine's erstwhile graduate school teacher and later colleague: "that is a priori which we can maintain in the face of all experience, come what will" (1929, 231). See Baldwin (2007) for more on Lewis's influence on Quine; Baldwin quotes an interview with Donald Davidson, in which Davidson suggests that Quine's epistemology is Lewis's minus the analytic/synthetic distinction.

understanding analyticity in terms of convention or linguistic fiat. However, 15 years later in "Two Dogmas," we see Quine question even the analytic status of logic because logical truths can be surrendered in the course of empirical investigation: "Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. … Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics" (1951, 40). That is, Quine believes that certain developments in the empirical enterprise of quantum physics could lead to changes in the logical laws and, therefore, even logic can be considered a synthetic matter since it is responsive to new discoveries about the empirical world.[16] Quine's claim here is that the class of paradigmatically analytic sentences can be contracted, a suggestion found in Tarski's nominalist project—although in "Two Dogmas," the contraction appears even more severe than in the finitistic language construction project, since it reaches beyond arithmetic to propositional logic. However, Quine's position in "Two Dogmas" is not stronger than Tarski's 1935 claim, made in conversation, that "he had never uttered a sentence which he had not considered to be revisable" (Mancosu 2005, 331), if we make the Quinean identification of 'analytic' and 'unrevisable'. Tarski makes similar claims in a 1944 letter to Morton White: "I am ready to reject certain logical premises (axioms) of our science in exactly the same circumstances in which I am ready to reject empirical premises. … Certain new experiences of a very fundamental nature may make us inclined to change just some axioms of logic. And certain new developments in quantum mechanics seem clearly to indicate this possibility" (White 1987, 31–32).[17] And it seems unlikely that Tarski never voiced these views about logic in Quine's presence during their year together at Harvard.

I am not claiming that Quine's willingness in "Two Dogmas" to renounce the supposed analyticity of logic and mathematics must stem from Tarski's

16. Because I stress the distinction in Quine's development between inflating and contracting the class of analytic sentences, I must demur from Mancosu's assertion that making certain apparently empirical sentences "unrevisable despite all observations [what I'm calling 'inflation'] … is just the other side of the coin of claiming that logical propositions might be just as revisable as the physical ones [what I'm calling 'contraction']" (2005, 330). The phrase 'the other side of the coin' is, of course, metaphoric, so Mancosu may not intend to say that they are very similar, but 'Every synthetic sentence can be unrevisable' and 'Every analytic sentence can be revisable' are not equivalent (each is the converse of the other, if we make the standard Carnapian identification of 'synthetic' and 'not analytic').

17. It should perhaps be noted that Tarski does not express complete certainty in this view of logical truth, writing immediately afterward: "Whether this description is true and adequate—I don't know" (White 1987, 32).

nominalist proposal. However, these conversations with Tarski and Carnap, in which certain portions of arithmetic are considered dependent on empirical facts about the world, certainly could have planted the idea in Quine's head or (perhaps more likely) cultivated the germ of an idea he had already entertained. Additionally, I am not suggesting this radicalization of Quine's critique of analyticity—from 'The corpus of analytic truths can be indefinitely expanded' to 'The corpus of analytic truths can be indefinitely expanded or contracted'—is the only conceptual step needed to move from the Quine of 1936 to the Quine of 1950. The radicalization described in this section is only part of Quine's break with Carnapian analyticity. Another part, perhaps more important, is given in the next section, where I describe how Quine's antecedent antipathy toward intensional languages is transformed into a criticism of Carnapian analyticity in the early 1940s.

4. Quine Prefers Extensional and Syntactic Analyses of Concepts

In Carnap's 1934 *Logical Syntax of Language*, which Quine read as the manuscript pages issued from Ina Carnap's typewriter, the treatment of every logico-linguistic notion—analyticity included—aims to be explicitly extensional and syntactic.[18] This section argues that Quine's public and private view of how language should be analyzed around 1940 (as well as before and after) is very similar to Carnap's view in *Logical Syntax*, but it is rather different from the explicitly semantic and intensional viewpoint Carnap advocates from 1939's *Foundations of Logic and Mathematics* onward. Thus, Quine's break with Carnap over analyticity can be seen as the result of Carnap changing his position as much as Quine changing his: Carnap moves toward a semantic and intensional approach to language analysis, while Quine retains the more syntactic and extensional approach exhibited in *Logical Syntax*. For example, in the 1934 lectures on Carnap, in expositing Carnap's notion of quasi-syntactic utterances, Quine writes: "It is in sentences dealing with reference, mention, meaning, denotation that we must be on our guard; also in modal sentences, both logical and empirical" (Creath 1990, 101). Concerning semantic and intensional language, Quine's guard stays up over subsequent decades, while Carnap relaxes his (although Quine does allow for a certain type of semantics, which he calls the 'theory of reference'; more on this below).

18. That said, parts of Carnap's definition of 'analytic-in-language-II' (the language meant to capture all of classical mathematics) will probably appear semantic to current readers—the division between syntax and semantics that we find in current textbooks does not precisely match the usage of Carnap and other early twentieth-century logicians. But as the quotation in the next paragraph makes clear, Carnap certainly thinks his work in *Logical Syntax* eschews all appeals to the meanings of expressions.

In the opening pages of *Logical Syntax*, Carnap writes:

> Even those modern logicians who agree with us in our opinion that logic is concerned with sentences, are yet for the most part convinced that logic is equally concerned with the relations of meaning between sentences. They consider that, in contrast with the rules of syntax, the rules of logic are non-formal. In the following pages, in opposition to this standpoint, the view that logic too is concerned with the formal treatment of sentences will be presented and developed. We shall see that the logical characteristics of sentences (for instance, whether a sentence is analytic, synthetic, or contradictory; whether it is an existential sentence or not; and so on) and the logical relations between them … are solely dependent on the syntactical structure of the sentences. (1934/1937, 1–2)

It is difficult to imagine a more devoutly syntactic credo for logico-linguistic analysis than this. In *Logical Syntax*, Carnap specifically attacks "sentences about meaning" in section 75. He also explicitly subscribes to the "thesis of extensionality," which states that "a universal language of science may be extensional" (1934/1937, 245).[19] (Carnap's extensionalism predates *Logical Syntax*: the "thesis of extensionality" appears in the *Aufbau* as well [1928/1963, sec. 43].) Additionally, Carnap considered intensional languages suspect, on the grounds that many sentences couched within it are quasi-syntactic (or in the 'material mode') and thus misleading at best (1934/1937, 246).[20] Carnap explains how to translate suspicious, "quasi-syntactic" intensional language (including modal language) into scientifically hygienic language in sections 68–70.

Let us briefly elucidate the extensional, syntactic position Carnap articulates in *Logical Syntax*. Carnap's term 'syntactic' corresponds to the current term 'proof-theoretic': a purely syntactic characterization of a language L consists of nothing but formation rules, transformation rules (usually called 'inference rules' today), and (if desired) uninterpreted axioms. No mention is made of

19. Actually, the type of extensionalism we find in *Logical Syntax* is nonstandard, since Carnap disallows the notion of truth in that work. Extensionality is usually characterized in terms of truth: two linguistic expressions have the same extension if and only if they are intersubstitutable *salva vertitate*, and a language (or language fragment) L is *extensional* if and only if substitution of coextensive expressions (potentially within larger expressions) in L never changes any expression's extension.

20. Contrast this view with what Carnap says to Nelson Goodman in 1940, in a conversation about Goodman's dissertation: "My objection against my *Aufbau*: … the extensional conception: definition of qualities etc. by enumeration" of the individuals the quality instantiates (RCC 102-44-11). This conception of a quality or property is familiar from Russell (1915, 42), as the 'principle of abstraction'.

what the linguistic symbols denote or how strings of these symbols are related to truth-values. For example, in *Logical Syntax*, roughly stated, two predicates P_1 and P_2 are synonymous (with respect to a given language, i.e., proof-system L) if and only if, for any two sentences S_1 and S_2, which are identical except that the first has P_1 wherever the second has P_2, there exists a proof in L of S_1 from S_2, and conversely. And an analytically true sentence is, very roughly stated, a generalization of the notion of theorem, that is, the last line of a proof with no premises.

Shortly after *Logical Syntax* appears, Carnap learns of Tarski's groundbreaking "On the Concept of Truth in Formalized Languages." Carnap quickly realizes that semantic notions can now be framed in a scientifically respectable form and are potentially useful for scientific philosophy. Carnap then begins reworking many of his fundamental philosophical concepts—including the notions of analytic truth and synonymy—to fit within Tarski's new semantic framework. Which extralinguistic entities the terms of the language designate becomes an essential part of the specification of a language. Additionally, Carnap allows the meanings assigned to linguistic expressions to now be intensional, specifically modal. Thus, for example, we find Carnap offering the following characterization of synonymy in the 1940 discussion notes: two predicates are synonymous if and only if they designate the same property, which means that the two predicates "have the same extension not only in the actual world, but rather in every possible world, so in every total state of affairs" (RCC 102-63-07). And correspondingly, a sentence is analytically true if and only if it is true in each 'possible world', that is, total state of affairs. And languages are now characterized in terms of semantic rules (rules such as "The name 'Socrates' designates Socrates" or "The predicate 'blue' designates the property of blueness," as well as familiar truth-table characterizations of the logical connectives); purely formal rules of inference no longer play the fundamental role in specifying a language. This is important for later developments in the analyticity debate, for although 'No bachelors are married' is not a logical truth (since the sentence schema $\ulcorner \neg \exists x(Bx \wedge Mx) \urcorner$ is not true in every model), it is true in every possible world allowed by the semantic rules governing the expressions in 'No bachelors are married'.

Quine, in contrast, is fundamentally hostile to intensional languages in general, and modal languages in particular, throughout his life. There is abundant evidence of this well-known fact; I will briefly rehearse some of this evidence. First, Quine claims to have been an extensionalist from his college days at Oberlin and was disappointed that his teachers at Harvard did not share this viewpoint, even the logically inclined ones such as Lewis, Sheffer, or his thesis advisor Whitehead (Quine 1991, 2001). Second, Quine reports his

dissertation aimed to "rework the foundations of *Principia Mathematica* in purely extensional terms" (1991, 266). Third, in one of his first publications, 1934's "Ontological Remarks on the Propositional Calculus," Quine argues that the intensional concept of proposition is obscure and unscientific. Finally, Quine argued vehemently against quantified modal logic from the 1940s onward in print, declaring it unclear, unintelligible, and nonsense.[21] Quine holds that if a (logico-mathematical) concept is to be scientifically respectable, then it must be extensional. For example, Quine writes: "In mathematical logic … a policy of extensionality is widely espoused: a policy of admitting statements within statements truth-functionally only" (1976, 162). Quine's suspicions concerning modality and intensional languages are manifest in the 1940–41 discussions as well. For example, Quine commends one of Tarski's characterizations of 'analytic' on the grounds that by using it "we avoid 'state of affairs,' intensional language, and the unclear concept 'logically-possible'" (RCC 090-16-10).

Furthermore, speaking generally, Quine is not as impressed as Carnap by semantic approaches to logico-linguistic issues and prefers syntactic analyses of language. However, Quine's attitude toward semantics (at least, toward what Quine calls the 'Theory of Reference' as opposed to the irredeemably intensional 'Theory of Meaning') is far less antagonistic than his attitude toward intensional idioms. For example, Quine accepts Tarski's notion of satisfaction and the resultant definition of truth. However, Quine is not the unequivocal booster of even extensional semantic methods (typified today in classical model theory) that Carnap is. First, very generally, as Decock has noted, "it is quite remarkable that Quine has spent unaccountably little attention to model theory. … One can even trace a slight contempt for the methods of model theory" (2002, 162; see references therein for evidence). This stands in direct contrast to Carnap in the 1940s, who explicitly states that the proof-theoretic view of language is derivative on or secondary to the semantic one in many respects (1942, sec. 39). Furthermore, a few months after the 1940–41 conversations ended, Quine delivered a lecture to Boston University's Philosophy Club on November 5, in which he suggested that the semantic program that Tarski brought to prominence had not lived up to its hype: "I feel that many of the most prominent claims that have been made for semantics are as yet unwarranted. I can't see that any really objective, scientific progress along semantic lines has been made in connection with such supposedly semantic topics as:

21. Current commentators on the analyticity debates often side against Quine's antipathy to modal locutions, and not merely because possible-worlds talk is more fashionable among philosophers today. Stein (1992) and Sober (2000) both point out that, in our current best natural science, modal language is apparently indispensible, and Quine of course holds that there is no higher court of appeal than what our current best scientific theories require.

meaningfulness, protocol sentences, analytic vs. synthetic sentences, indicative vs. expressive use of language. Perhaps progress will be made on some of these topics; but I can think of nothing that I would point to as a definitive semantical accomplishment" (Quine Papers, item 3058). Clearly, Quine has not given up on semantics in 1941—and he never does reject it—but he is far less impressed with the program than Carnap is: apparently not even Tarski's seminal "On the Concept of Truth in Formalized Languages" counts as a 'definitive semantical accomplishment'.

This general Quinean trait shows up clearly in remarks Quine makes about logical truth. For example, in a December 1940 lecture, Quine says, "'Logically true' can be defined syntactically. … This is *more elementary* than the semantic characterization with the help of 'true'" (RCC 102-63-04; my emphasis). What is this syntactic characterization? Quine still holds it 30 years later: "We can simply define a logical truth as any sentence produced by these rules of proof" (1970, 57), where 'these rules' refers to any standard set of complete textbook rules for first-order logic. This is somewhat surprising to contemporary sensibilities: today, most people think that the 'more elementary characterization' of logical truth is semantic—for how could logical truth be a type of truth, if it is not semantic? Similarly, Quine writes: "Standards of logical truth can be formulated in terms merely of more or less complex notational features of statements; and so for mathematics more generally" (1958, 4). So the meanings conferred on notational features need not be considered when attempting to discern whether a given sentence is a logical (or mathematical) truth.

Although the above texts show that Quine strongly prefers proof-theoretic approaches to semantic ones, someone might reasonably object that, in other texts, Quine does use other formulations of logical truth apart from the completely syntactic one just described. In "Truth by Convention" and "Two Dogmas," Quine uses 'truth' in the *definiens* when defining 'logical truth'. There, Quine gives the following definition of logical truth: "If we suppose a prior inventory of *logical* particles, comprising 'no', 'un-', 'not', 'if', 'then', 'and', etc., then in general a logical truth is a statement which is true and remains true under all reinterpretations of its components other than the logical particles" (1951, 23). (This is known as the *substitutional* characterization of logical truth; it contrasts with the most common definition today, the *model-theoretic* one, in which a formula is logically true if and only if that formula is true in all models.) Now, when Quine compares all three various notions of logical truth in *Philosophy of Logic*, he favors the substitutional definition over the model-theoretic one.[22]

22. Why? "The evident philosophical advantage of resting with this substitutional definition, and not broaching model theory, is that we save on ontology" (Quine 1970, 55).

However, he also favors the purely proof-theoretic view, in which a logical truth is just a theorem, over the substitutional characterization, again calling the former "more elementary" (1970, 57), just as he had 30 years earlier, which is clear evidence that he prefers syntactic analyses to semantic ones, at least in the case of 'logical truth'. So although Quine uses the substitutional characterization in various places, his all-things-considered preferred view of logical truth is "independent of the notions of truth and satisfaction" (57). To repeat, Quine certainly does not completely reject the notion of truth (or even model theory), as he unreservedly rejects modal and other intensional idioms. However, he does not favor it, as Carnap does, and he does without it when possible.

Finally, returning to the historical question of what else was happening around 1940 that could play some role in ending Quine's discipleship, we know that Quine read Carnap's *Introduction to Semantics* manuscript for the University of Chicago Press during 1940 as well (Creath 1990, 291), a book in which an intensional and fundamentally semantic approach is front and center. In short, part of Quine's growing skepticism of the notion of analyticity may be traceable to Carnap's characterizing analyticity (and other logico-linguistic notions) in modal and semantic terms, instead of Quine's preferred extensional and syntactic characterization. Once Quine's erstwhile hero commits to spelling out 'analytic' in a modal idiom, Quine now considers Carnap's current characterization of analyticity unacceptable. Quine rejects Carnapian analyticity from 1940 onward, so his 'discipleship' is over (since disciples abandon a teacher when the teacher espouses new principles the disciple cannot accept). More speculatively, this move on Carnap's part may have led Quine to be more skeptical of any characterization of analytic truth that purports to do heavy epistemological lifting. Why? Seeing the best and brightest (in Quine's eyes) defender of analyticity say that analyticity is intensional and semantic could have further inclined Quine to think that no scientifically acceptable characterization could be found. Note that this is a rather indirect influence: the fact that Carnap embraces an intensional account of analyticity from 1940 onward is of course not an argument showing that no account of analyticity could satisfy Quine's various philosophical scruples. But, in general, if you are somewhat skeptical about some claim p, and the world's leading expert on that topic comes forward and states that p really means that pigs can fly, your skepticism about p will likely increase. You certainly could still believe that there is an alternative interpretation or understanding of p that does not require porcine aviation and work to find such an interpretation, but in light of the worldwide expert's testimony, you might be less optimistic about finding such an interpretation. Similarly, the notion that seemed somewhat suspicious to

Quine in "Truth by Convention" becomes even less appealing, after the great Carnap comes to view analytic truth in much the same way as C. I. Lewis did—a viewpoint Quine found unacceptable from his student days onward.[23] However, to repeat, this point is speculative. It is much more certain that Quine rejects the conception of analyticity Carnap endorses after 1940. (The distinction between Quine's attitudes toward Carnapian analyticity vs. analyticity tout court will loom large below, in sec. 5.)[24]

Further evidence for the picture suggested here comes from the context of the quotation discussed earlier from Quine's "Homage to Carnap": Quine writes that "Carnap was my greatest teacher. … I was very much his disciple for six years. In later years Carnap's views went on evolving, and so did mine" (Creath 1990, 464; see also Quine 1991, 267). If my hypothesis is correct, then Quine's description of the historical development of both men is exactly right: in *Logical Syntax*, Carnap's approach to logico-linguistic questions was explicitly syntactic and extensional, which is Quine's preferred approach throughout his life. This forces us to revise the usual picture of the Quine-Carnap debate: it is not that Carnap has a static position that Quine advances away from as the Viennese scales drop from his eyes—instead, as Quine says, Carnap's views "went on evolving."[25] Quine and Carnap have a more or less shared vision of logico-philosophical analysis in 1934, and both men depart from that shared vision as the years pass, although in different directions. Interestingly, Quine, in certain respects, maintains the old standards; in these respects, Carnap is actually the progressive one, at least insofar as he liberalizes the analysis of language by including intensional tools and by placing semantics front and center. Quine, in contrast, maintains the stricter, more conservative standards both men shared in the mid-1930s, although of course in other ways Quine's views changed too. In short, the end of Quine's discipleship is perhaps as much a product of Carnap changing his views (toward fundamentally semantic and intensional approaches and away from exclusively syntactic and extensional ones) as Quine changing his.

Let us now consider how Quine's preference for the syntactic approach to language analysis could contribute to his mature aversion toward a traditional, substantive notion of analyticity. (His rejection of modal locutions immediately

23. Daniel Isaacson appears to suggest that this is part of Quine's distancing himself from Carnapian analyticity as well (2004, 233–35), although Isaacson holds that this is an "accidental reason" for their later divergence (238).

24. An anonymous HOPOS referee helped me a great deal with this paragraph, forcing me to resolve importantly unclear claims.

25. Isaacson (2004, 238) also notes the changing nature of Carnap's views, and the relative stability of Quine's—e.g., Quine never recants any claims in "Truth by Convention."

motivates his rejection of analyticity: in "Two Dogmas" and before, Quine says that if we admit modal vocabulary into our scientific language, then A is analytic exactly when ⌜Necessarily A⌝ is true; Quine 1951, 29; cf. 1947, 45.) In both "Two Dogmas" and "Notes on the Theory of Reference," Quine claims that characterizations of analyticity can only be done language by particular language, so that giving a "definition of 'analytic-in-L' for each L has seemed rather to be a project unto itself" (Quine 1961, 138; cf. 32–36). Setting aside the question of whether this is a pressing objection (there are reasonable arguments that it is not),[26] I wish to note here that this complaint will seem more plausible if our conception of language is syntactic. For example, in Carnap (1939), in the section describing a particular proof calculus (as opposed to its corresponding semantic system), there are a few axioms and two rules of inference, namely, *modus ponens* and a rule that effects the synonymy of two particular expressions (nonsense words Carnap has introduced in his example language):

> R2. *Rule of synonymity*: The words 'titisee' and 'rumber' may be exchanged at any place (i.e., if [sentence] S_2 is constructed out of S_1 by replacing one of those words at one place by the other one, then S_2 is directly derivable from S_1 in [the proof system] B-C). (161)

And clearly, another rule of inference (or postulate) would have to be added to the calculus for each additional set of synonymous expressions we wished to represent. This somewhat ad hoc treatment of synonymous expressions does appear to make the definition of analyticity in each language a 'project unto itself': we need a separate rule for each set of synonymous words in the language. However, if we begin from the semantic (i.e., later Carnapian) point of view, synonymy does not appear so ad hoc: for example, every (referring) individual constant in the object language is assigned an object by the semantic rules, and if two individual constants happen to be assigned the same object by the semantic rules, then they are synonymous.[27] Similarly, every predicate is assigned a property, and if two predicates happen to be assigned to the same

26. See Martin (1952, 44–45), who argues that Tarski's definition of 'truth', which all parties agree is scientifically respectable, would suffer from the same supposed 'defect', and the same holds for many syntactic concepts as well. See also David (1996) for a more recent version of this line of thought. Quine's reply is that truth is different because Tarski's truth schema ('…' is true if and only if …) grants an antecedent clarity to the notion of truth, thereby obviating the need for an explication (Quine 1961, 134), while 'analytic' lacks this clarity and thus needs to be explicated. What counts as 'clear' for Quine is not always clear. But it is worth noting that the truth schema does allow for the notion of truth to be introduced syntactically, e.g., as an axiom in a Hilbert-style system, or via introduction and elimination rules in a natural deduction system.

27. One can urge the following well-known objection to such a view: if our semantic system assigns Venus to both 'Morning Star' and 'Evening Star' as each expression's semantic content,

property, then they are synonymous. In the cases of both names and predicates, synonymy falls out of the initial characterization of the language as a by-product, in a fairly natural way. However, a syntactically characterized language must 'tack on' extra axioms or rules of inference to accommodate each instance of synonymy, if one follows Carnap's example about 'titisee' and 'rumber' above. In short, Quine's preference for syntactic analyses may have led him to believe that the only way to capture analyticities in a formal language is by ad hoc tacking on of extra axioms—in which case characterizing 'analytic in *L*' would be a 'project unto itself' for each language *L*, as Quine asserts in "Two Dogmas."

5. Creath versus Mancosu on Quine's Timeline

Suppose the main points of the previous sections are correct; that is, suppose "Truth by Convention" does not contain the complete departure from Carnapian ideas that we find in "Two Dogmas," and that the 1940–41 conversations did play an important role in radicalizing Quine's critique of analytic truth. A further question to ask is, when exactly did Quine make the fundamental break or breaks? And in particular, why did "Two Dogmas" not appear for another 10 years after the Harvard discussions? Part of the answer may be that Quine spent the first part of that decade in the Navy. But as shown in section 2, even after he returned to academic life, Quine's publications in the 1940s hold out some hope for a substantive notion of analytic truth.

I believe these issues can be fruitfully addressed by drawing on the picture of Quine's journey detailed in the preceding sections in order to evaluate a criticism Mancosu levels at Creath's view of Quine's development. Creath claims that Quine's final rejection of analyticity does not come until 1947, in the context of his three-way correspondence with Goodman and Morton White (1990, 31). Further, this break occurs only after a slow, gradual disillusionment with the notion of analytic truth that had been building for years: "Quine arrived at that break" (i.e., his "reject[ion of] Carnap's doctrine that there are analytic truths") "*only by stages*" (Creath 1990, 31; my emphasis). Mancosu poses "the question of when exactly Quine arrived at the criticism of the analytic-synthetic distinction" (2005, 331). To phrase the issue in terms of

then 'Morning Star = Evening Star' will be true in virtue of the semantic rules, i.e., analytic. Examples of this sort push Carnap, in *Meaning and Necessity* and afterward, to assign to each individual constant an individual *concept*, which is defined as a function that assigns to each possible world a single object, which is the denotation of that individual constant in that possible world. Kripke's arguments that names are rigid designators have rendered this Carnapian view, in which the denotation of a name can vary from world to world, unattractive.

'arriving at the criticism' of analyticity may prejudice the question against Creath's view, since he wants to portray Quine's criticism as slowly developing and changing over time, so there is not a particular moment at which Quine 'arrives' at his critique, just as there is not a particular instant in which a person arrives at adulthood from childhood. Furthermore, it may be misleading to speak of *the* criticism of analyticity, since Quine's attack has multiple prongs. But this is terminological quibbling; we can rephrase Mancosu's question as: when did Quine arrive at the radical criticism of analyticity expressed in "Two Dogmas"? Creath claims that it was in the summer of 1947, in the aforementioned three-way correspondence with Goodman and White. Mancosu argues that it was earlier. He offers as evidence a letter from Quine to J. H. Woodger, dated May 2, 1942, in which Quine discusses the 1940–41 academic year. "Carnap, Tarski and I had many vigorous sessions together, joined also, in the first semester, by Russell. Mostly it was a matter of Tarski and me against Carnap, to this effect. (a) C's professedly fundamental cleavage between the analytic and the synthetic is an empty phrase (cf. my 'Truth by Convention'), and (b) consequently the concepts of logic and mathematics are as deserving of an empiricist or positivist critique as are those of physics. In particular, one cannot admit predicate variables (or class variables) primitively without insofar committing oneself to 'the reality of universals'" (quoted in Mancosu 2005, 331). Mancosu believes this letter shows that "already in 1940–1941 Quine had explicitly rejected the notion of analyticity, and in 1942, he considered that rejection to be already in his 1936 paper 'Truth by Convention'" (331). In short, Mancosu, unlike Creath, believes that Quine's rejection of analyticity was complete well before 1947—by 1942 at the latest. He does not commit to a position in Mancosu (2005), concerning whether this completion comes in 1936, 1940–41, or some other particular time before sending the letter to Woodger.

Which picture of Quine's path to the rejection of analyticity is correct, Creath's or Mancosu's? The historical evidence, as we have seen already, appears to pull in opposite directions. One could accept Creath's picture and try to explain away all the apparently disconfirming evidence (such as Quine's 1942 letter to Woodger). Or one could adopt Mancosu's position and try to explain away why Quine continues to use the concept of analyticity (albeit reluctantly) throughout the 1940s, such as in the December 1946 lecture (quoted in sec. 2) in which Quine states, concerning the problems of adequately explicating 'analytic', "My attitude is not one of defeatism, nor one of dismissing the problem as illusory. We have real problems here, meaningful problems worth working on." Alternatively, one could attempt to steer a middle course, and that is what I will attempt here.

I propose that we can make sense of the apparently conflicting pieces of evidence as follows, drawing on a point I made in section 4. Quine's letter to Woodger indicates that by 1942 Quine had rejected Carnap's preferred contemporaneous explication of analyticity. However, Quine still thought that the notion might eventually be acceptably clarified along non-Carnapian lines—specifically, along empirical, extensional, and hopefully syntactic lines, as opposed to Carnap's a priori, modal, and semantic characterization. This would explain the otherwise puzzling fact that Quine spends relatively little time in "Two Dogmas" dealing directly with Carnap's then-current view.[28] Quine had abandoned Carnap's preferred characterization of analytic truth several years earlier, around the time of the Harvard discussions, and thus focused the article on what he considered more promising or plausible alternatives, such as Frege-analyticity.

This hypothesis provides a third way between Creath and Mancosu's views: Quine had completely rejected Carnap's attempts to explicate analytic truth by 1942 (at the latest), but he was not yet willing to commit himself to the radical view of "Two Dogmas" until shortly before writing that piece. However, certain important parts of Quine's eventual "Two Dogmas" position are already present in "Truth by Convention" (specifically, (i) on the 'linguistic fiat' explication of analyticity, the class of analytic truths can be indefinitely expanded, and (ii) analyticity must ultimately be cashed out in empirical terms); for this reason, it makes sense that Quine points to it in retrospect as providing reasons for dissent. So Creath may well be right that Quine's mature critique of analyticity, which claims that no coherent characterization of analyticity can do heavy epistemological lifting, does not surface until 1947, while Mancosu is correct that Quine had rejected Carnap's then-current account of analytic truth by 1942 at the latest, in the immediate aftermath of the 1940–41 discussions at Harvard.

6. Conclusion: The Final Steps to "Two Dogmas"

At least one important question about Quine's journey to "Two Dogmas" remains. Given that Quine felt philosophers should be trying to draw the

28. Another possible reason is that Carnap is not the only proponent of analyticity whom Quine targets in "Two Dogmas": C. I. Lewis, at least, is included as well. Hylton writes: "Quine's attack [in 'Two Dogmas'] is … not narrowly focused on Carnap. In particular, he may have had C. I. Lewis more or less implicitly in mind; Lewis's version of analyticity, more explicitly than Carnap's, is understood in terms of meaning, mentalistically construed" (2007, 51). (Lewis is not merely an implicit target: he is also explicitly mentioned three times in the original version of "Two Dogmas" [Quine 1951, 28 n. 4, 35 n. 7, and 43] and once more in the revised version appearing in Quine [1961, n. 6].)

analytic/synthetic distinction as late as December 1946, what further conceptual steps between then and the publication of "Two Dogmas" led Quine to say that the distinction "is nonsense, and the root of much nonsense" (1951, 39)? This is an important historical question, and I lack the evidence needed for a definitive answer. But I can offer two suggestions. First, Quine's 1946 lecture does not consider the argument about synonymy that appears in section 3 of "Two Dogmas," in which Quine suggests that a language must contain modal idioms in order to use a 'interchangeability *salva vertitate*' standard to distinguish between expressions that are truly synonymous and expressions that merely have the same extension. It may be that Quine did not think the type of synonymy needed to underwrite an epistemologically useful notion of analyticity was unavoidably modal (as Carnap had in the Harvard discussions) until the late 1940s, and this later realization pushed Quine away from considering the characterization of synonymy a project that could yield rewards for the epistemology of mathematics and logic.

Second, one of the most important reasons for appealing to the notion of analyticity is to make sense of the prima facie special epistemological status of logic (and mathematics, for many friends of analyticity from Frege onward) within an empiricist framework. Carnap saw this as a, if not the, leading insight of logical empiricism: all substantive knowledge had its source in experience (thereby preserving empiricism), but the claims of math and logic are, in an important sense, empty or nonsubstantive—unavoidable by-products of the language scientists choose to use. Quine always viewed himself as a good empiricist, and perhaps he clung to hope for an eventual clarification of analyticity well into the 1940s in part because he saw no other way for an empiricist to account for the apparently special status of logic and mathematics. (Recall that in "Truth by Convention," Quine says that logic and mathematics are a priori, although he gives 'a priori' a behavioristic sense there.)

In "Two Dogmas," however, Quine finally presents an alternative explanation for the felt difference between the truths 'If grass is white, then grass is white' and 'Over 2 million people live in New York City on January 1, 2010'. This explanation is absent from Quine's criticisms of analyticity in the 1930s and '40s, including the December 1946 lecture mentioned above, which covers many of the same critiques found in "Two Dogmas." In the final section of "Two Dogmas," Quine appeals to the principles of *conservatism* and *simplicity* in theory choice to explain why logic and mathematics feel so different from physics and history. Rejecting *modus ponens*, unlike rejecting our estimate of the number of inhabitants of New York City, would likely require a massive overhaul of our sciences and conceivably would complicate our theories of the world horribly, and thus we are very reluctant to make such a change.

By invoking these principles of conservatism and simplicity, we no longer need the notion of analyticity to account for the prima facie felt specialness of logic and mathematics.[29] The type of analyticity Carnap and Lewis consider essential for understanding the relationship between logico-mathematical inquiry and the natural sciences becomes superfluous to our account of scientific activity and thus should be eliminated. (Of course, on Quine's "Two Dogmas" view, mathematics and logic are not genuinely epistemologically different in kind from natural science—Quine's principles of simplicity and conservatism explain why they appear different.) Before his appeal to conservatism and simplicity, no matter how unhappy Quine might have been with extant accounts of analyticity, perhaps he could not see how scientific philosophy could account for the apparently distinguishing features of logic and mathematics without some notion of analytic truth.

In this article, I have attempted to chart the intellectual trajectory of Quine's increasingly radical critique of analytic truth. Carnap's move to a modal and semantic account of analyticity, clearly in evidence during the 1940–41 discussions, made Quine reject Carnap's then-current account of analyticity and perhaps made Quine even more suspicious in general of a notion he had begun to be skeptical about in "Truth by Convention," written when Carnap still accepted the extensional and syntactic approach. And Quine's exposure to Tarski's nominalist language form could have suggested or strengthened the claim, found in "Two Dogmas" but not "Truth by Convention," that the class of analytic truths could be contracted, for intellectually respectable reasons. Finally, although Quine rejected the characterizations of analyticity Carnap offers in the 1940–41 conversations, "Two Dogmas" does not appear until 10 years later because Quine holds out hope for an alternative account of analyticity that would satisfy his philosophical scruples and do the epistemological work of explaining the intuitive difference between the formal and the empirical sciences.

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29. Hylton stresses this point as well. He writes: "Quine appeals to … a 'Maxim of Minimum Mutilation' … to explain why logic and mathematics are often … taken to be a priori and necessary." Hylton then quotes Quine: "We prefer to seek an adequate revision of some more secluded corner of science, where the change would not reverberate so widely through the system. This is how I explain … the inaccessibility of mathematical truth to experiment, and it is how I explain its aura of a priori necessity" (2007, 78).

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