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The Methodological Roles of Tolerance and Conventionalism in the Philosophy of Mathematics: Reconsidering Carnap's Logic of Science

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A thesis submitted in partial fulfillment of the requirements for the degree in Doctor of Philosophy

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by

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Abstract

This dissertation makes two primary contributions. The first three chapters develop an interpretation of Carnap's *Meta-Philosophical Program* which places stress upon his methodological analysis of the sciences over and above the Principle of Tolerance. Most importantly, I suggest, is that Carnap sees philosophy as *contiguous* with science—as a *part of the scientific enterprise*—so utilizing the very same methods and subject to the same limitations. I argue that the methodological reforms he suggests for philosophy amount to philosophy as the *explication* of the concepts of science (including mathematics) through the construction and use of suitably robust meta-logical languages. My primary interpretive claim is that Carnap's understanding of logic and mathematics as a set of formal auxiliaries is premised upon this prior analysis of the character of logico-mathematical knowledge, his understanding of its role in the language of science, and the methods used by practicing mathematicians. Thus the Principle of Tolerance, and so Carnap's logical pluralism, is licensed and justified by these methodological insights.

This interpretation of Carnap's program contrasts with the popular *Deflationary* reading as proposed in Goldfarb & Ricketts (1992). The leading idea they attribute to Carnap is a *Logocentrism*: That philosophical assertions are always made relative to some particular language(s), and that our choice of syntactical rules for a language are constitutive of its inferential structure and methods of possible justification. Consequently Tolerance is considered the foundation of Carnap's entire program. My third chapter argues that this reading makes Carnap's program *philosophically inert*, and I present significant evidence that such a reading is misguided.

The final chapter attempts to extend the methodological ideals of Carnap's program to the analysis of the ongoing debate between category- and set-theoretic foundations for mathematics. Recent criticism of category theory as a foundation charges that it is neither autonomous from set theory, nor offers a suitable ontological grounding for mathematics. I argue that an analysis of concepts can be foundationally informative without requiring the construction of those concepts from first principles, and that ontological worries can be seen as methodologically unfruitful.

Keywords: Rudolf Carnap, Explication, Conventionalism, Principle of Tolerance, *The Logical Syntax of Language*, Foundations of Mathematics, Philosophy of Mathematics, Rational Reconstruction, Logic of Science, Scientific Philosophy, Logical Pluralism, Logical Empiricism, Logical Positivism, Philosophy of Science.

Modern science arose from the marriage of mathematics and empiricism; three centuries later the same union is giving birth to a second child, scientific philosophy, which is perhaps destined to as great a career. For it alone can provide the intellectual temper in which it is possible to find a cure for the diseases of the modern world.

Bertrand Russell (1936)

Dedicated to my parents,
for their love and support.

And to Mark Bronson, for introducing
me to philosophy, and for encouragement
and sound advice every step of the way.

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The enthusiasm displayed by my examiners David DeVidi, Stathis Psillos, Wayne Myrvold, and Robert Mercer has redoubled my own enthusiasm for the project. I must admit that by the time of submission I was looking forward to taking some time away from Carnap, but their encouragement and insightful comments have reminded me of why I decided upon this topic in the first place.

In my (not insignificant) time at Western I have benefited from the instruction and example of many exceptional philosophers, but with regard to my own philosophical development, none more so than William Demopoulos. It was Bill's courses and our conversations that instilled in me a deep appreciation for early analytic philosophy. His own work has inspired much of my thought, and represents an ideal of philosophical analysis and argumentation to which I may only ever strive.

With respect to this project, no one has had a greater influence on its immediate ideas and writing than my friend and colleague Steve Bland. My understanding of Carnap and the Deflationary interpretation owes an incredible debt to our conversations. Steve also patiently and carefully read through many drafts of most of the chapters, at each stage asking thoughtful questions and offering deep and insightful commentary. In many ways this project would not have been but for Steve's help.

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Chapter 1

Introduction: A Bittersweet Renaissance for Logical Empiricism

> *Likewise our own discipline, logic or the logic of science, is in the process of cutting itself loose from philosophy and of becoming a properly scientific field, where all work is done according to strict scientific methods and not by means of "higher" or "deeper" insights.*
>
> Rudolf Carnap ([1934]1987)

The last twenty years has seen a renaissance of interest in Logical Empiricism, especially the work of Rudolf Carnap. Much of this attention has been of an historical nature. While I cannot here endeavour a comprehensive literature review, I will begin by mentioning a few worthwhile or otherwise influential sources. Book-length treatments placing the movement in its historical and intellectual context include Coffa (1991), Friedman (1999), and Carus (2007). Exemplars of proceedings from conferences, collections of papers, and special journal issues include Giere & Richardson (1996), the six-volume series *Science and Philosophy in the Twentieth Century* edited by Sahotra Sarkar (1996), Hardcastle & Richardson (2003), Awodey & Klein (2004), Wagner (2009*a*), and the recently published *Cambridge Companions* to *Carnap* and to *Logical Empiricism*, respectively Creath & Friedman (2007), and Richardson & Uebel (2007). Finally, noteworthy examples of the renewed enthusiasm for the recovery and careful scrutiny of primary sources includes Creath (1991), Psillos (2000), Awodey, Reck, & Gabriel (2004), Goldfarb (2005), a series of papers by Awodey & Carus (2001; 2006; 2010), another by Awodey & Reck (2002*a*; 2002*b*), and the forthcoming *Collected Works of Rudolf Carnap*—currently being compiled by a host of Carnap scholars overseen by Richard Creath, and projected to span fifteen volumes.

Besides this historical and interpretive interest, there have also been recent efforts to reinvigorate old, once-discarded Carnapian doctrines and to extend their application to surprising new areas. A recent example is Carus' (2007, chp. 11) application of Carnap's ideal of explication to the Rawls–Habermas debate regarding the necessary conditions for the development of a just and reasonable society.[1] Another is Robert Hudson's (2011) application of Carnap's Principle of Tolerance to the foundations of ecology. In both cases the idea is to apply Carnapian-style rational reconstructions to the conceptual foundations of a discipline. The intent is either to provide conceptual clarity to a debate—where Rawls and Habermas might be talking past each other—and to propose some transparent, mutually agreeable preliminary definitions; or to make apparent the possibility of a conflation of concepts—e.g., offering an analysis of the concept 'ecosystem' that shows the notion to have a whole constellation of differing and perhaps mutually-exclusive definitions at different levels of application. Regardless as to the success of such analyses (which I will not evaluate here) both projects are executed within the rubric of Carnap's methods and practical aims—what I will in the sequel sometimes call *Carnap's Meta-philosophical Program*. We will see that this program is best described as an attempt to establish a truly *Scientific Philosophy*.

This dissertation aims to make both interpretative and practical contributions. The first three chapters outline a relatively novel interpretation of Carnap's meta-philosophical program, while the last chapter will draw on lessons from our interpretation to extend some of the fruitful aspects of this program to the analysis of a contemporary philosophical debate. Our purpose in this application will not be limited to exemplifying the continuing relevance and fruitfulness of Carnap's program—the last chapter also attempts to make a novel contribution to philosophy. Our extension of Carnap's methods will not fall nearly so far afield as those of Carus and Hudson however. Rather, we will address a current debate in the philosophy of mathematics, a topic that was often at the fore of Carnap's own work.

[1] Bird (2009), and especially Mormann (2008), provide excellent overviews, and in the latter case extended criticism, of Carus' interpretation of Carnap's thought.

Specifically, I will treat the question of category- versus set-theoretic foundations. This is the subject of ongoing controversy in the literature, but for reasons of definiteness and tractability I intend to focus my attention upon a prominent exchange between Steve Awodey and Geoffery Hellman from the journal *Philosophia Mathematica*.[2] Awodey utilizes category theory to propose a structuralist foundation for mathematics, while Hellman argues that both Awodey's program and a more traditional set-theoretic program fall victim to longstanding ontological worries. He suggests that both programs become tenable only after being embedded within his own Modal Structuralism (Hellman, 1989), since this provides the ontological grounding he deems necessary for a suitable foundations for mathematics. Using the conceptual tools furnished by my interpretation of Carnap's program, I will argue that such ontological worries as Hellman expresses are largely beside the point, and that category-theoretic and set-theoretic foundations need not conflict. In fact, I think that the programs are complementary with respect to deepening our understanding of mathematics and its application, and as such both ought be pursued.

The remainder of this chapter introduces the interpretive issues that will be further discussed in the next few chapters. One currently-popular reading of Carnap's meta-philosophy, known as the *Deflationary* interpretation, has been developed in a series of articles by Warren Goldfarb and Thomas Ricketts dating back to their 1992.[3] Note that some of the recent interest in Logical Empiricism has been of a critical nature, focusing upon historical objections to the program as well as introducing a few new ones. Goldfarb and Ricketts seem to have been at least partially motivated in their reading by providing Carnap with a way out of many of these criticisms. This is done by interpreting his Principle of Tolerance as having such wide scope that at any place where it seems Carnap is making a doctrinal *assertion* which may be the subject of criticism (e.g., empiricism or mathematical conventionalism), it can be argued that he is in fact merely *proposing* that we *choose* to speak in this way or that on the basis of purely practical considerations. Choosing one form of language or means of expression over another is not a matter to be judged true or false, correct or incorrect. In this way any potential criticism can be side-stepped as misinterpreting Carnap's philosophical ambitions. Alternately, refusing out of

[2]The relevant articles are Awodey (1996; 2004) and Hellman (2001; 2003). Cf. Bell (1981) and Feferman (1977) for discussion of the various background issues involved. Indeed, the general line of Hellman's critique seems largely inspired by these papers.

[3]Besides Goldfarb & Ricketts (1992), cf. Ricketts (1994; 1996; 2003; 2004; 2007; 2009), and Goldfarb (1995*b*; 1996; 1997; 2005; 2009).

hand to accept an attitude recommended by the Principle of Tolerance amounts to a critic's being hard-headed, rooted in an old-fashioned, dogmatic attitude toward philosophy that Carnap is proposing we abandon.

I will argue in chapter 3 that the Deflationary reading is seriously misguided, and actually fails to adequately address the historical objections that they explicitly set out to treat with their interpretation. Moreover, I contend that a Deflationary reading weakens the program to the point that it cannot carry through the methodological reforms that Carnap recommends, and so I argue that Goldfarb and Ricketts' reading leaves Carnap's program *philosophically inert*, with little to recommend it in application to contemporary issues in philosophy or the sciences. The crux of my argument will show that although the Principle of Tolerance is certainly an important component of Carnap's meta-philosophy, it does not have the primacy of place attributed to it on the Deflationary view.

For the moment, however, our concern will be limited to a very brief sketch of this interpretation in contrast to my own. Below I will also introduce the aforementioned historical and contemporary criticisms to Carnap's program, as I think they should be addressed by any interpretation of Carnap's thought that makes claim to any sort of completeness. The full treatment of these objections is saved for chapter 2. Finally, I will round out the present chapter with a more substantial outline of the entire dissertation.

1.1 Carnap's Meta-Philosophy—Overview

The general spirit of the reading of Carnap's meta-philosophical program that I wish to develop is perhaps best expressed by Carnap in the epigraph to this chapter. This sentiment is reinforced on the second-to-last page of *The Logical Syntax of Language* ([1937]2002):

> He who wishes to investigate the questions of the logic of science must, therefore, renounce the proud claims of a philosophy that sits enthroned above the special sciences, and must realize that he is working in exactly the same field as the scientific specialist, only with a somewhat different emphasis: his attention is directed more to the logical, formal, syntactical connections.[4]

[4]Hereafter I shall refer to the book as *Logical Syntax*. The original German edition was published in 1934. Significant additions were made to the more technical sections for its 1937 English

Most importantly, Carnap sees philosophy as *contiguous* with science, as a *part of the scientific enterprise*, and so utilizing the same methods and subject to the same limitations. It is this attitude which underlies almost all of Carnap's work, and acts as the premier tenet of his mature philosophical outlook beginning with at least *Logical Syntax*. This is especially true of his famous rejection of metaphysics, insofar as that discipline claims access to a special kind of synthetic knowledge attainable *a priori*—knowledge of the world that is somehow "deeper" than that accessible by any empirical methods. Indeed, Carnap rejects as confused or misleading any metaphysical (and especially ontological) questions or theses which purport to assert factual knowledge.[5]

1.1.1 Transforming Philosophical Debates

In fact, Carnap goes so far as to suggest the wholesale *replacement* of traditional philosophical methods with what he in the above-quoted passage calls "The Logic of Science", which in the Foreword to *Logical Syntax* he glosses as "the logical analysis of the concepts and sentences of the sciences", or equivalently as "the logical syntax of the language of science" (p. xiii). It is this call for methodological reform that determines why I regard Carnap's program as a *meta-philosophical* position. One of his primary interests is an evaluation of philosophy itself, concluding with the suggestion that we replace its methods with a novel set of logical methods facilitating the possibility of agreement and progress in philosophical inquiry. Just what this project amounts to is a matter of some debate in the literature, but at least one important aspect of the program on all accounts is the systematic transformation of seemingly substantial philosophical debates into questions of language choice.

translation by Amethe Smeaton, the Countess von Zeppelin. Carnap tells us that these additions were omitted from the German original due to a lack of space, but were published separately in a series of articles. Every English edition of the book I have seen follows identical pagination.

[5]The rejection of synthetic *a priori* knowledge because it is not amenable to usual scientific methods, and the rejection of metaphysical or ontological questions and assertions as without cognitive content, are the primary topics of Carnap ([1932]1960; [1935]1996; [1950]1956; and 1963, pp. 44–46). Cf. *Logical Syntax*, Part V, and Carnap ([1966]1974, chp. 18). It is important to stress that Carnap never argues that metaphysics be entirely abandoned. He merely urges that such theses and methods cannot by themselves result in factual knowledge, and are apt to confuse when it is assumed that they can. Viewing the world through a certain metaphysical lens may still have psychological relevance by influencing a person's behaviour, as when the realist attitude of a physicist provides insight or motivation in the development of a physical theory. Still, Carnap would prefer it if the physicist in question were to describe herself as merely choosing to use a physicalist (rather than nominalist or instrumentalist) language.

Example: Realism vs. Idealism

Consider, for example, the debate between realists and idealists (who in *Logical Syntax* Carnap calls "positivists") over the most fundamental constituents of reality. Whereas realists would insist

> **1a:** *Things* are fundamentally composed of matter,

idealists would, on the contrary, insist

> **2a:** *Things* are fundamentally complexes of sense-data.

Carnap observes in *Logical Syntax* (p. 301) that as metaphysical or epistemological theses about the world—*viz.*, expressed as factual claims—there exists little possibility for eventual agreement, or even any real progress, between advocates of these positions. We can take the history of philosophy as evidence for this claim. His diagnosis of this impasse is that the disputants do not have a common set of criteria by which the controversy might be decided. Given that our best scientific theories are the most reliable means of making predictions about our future experience, we suppose both sides to agree upon the science and empirical evidence. So they are at root merely *interpreting* the empirical evidence in fundamentally different ways. Thus a lack of agreement or progress in the dispute, because the disputants are not disagreeing about an issue that can be decided by empirical methods, but really on a question of personal preferences or practical considerations.[6] Rather than languish endlessly in this argumentative *cul-de-sac*, Carnap maintains that each position should be transformed into a proposal to adopt a certain language or way of speaking, what in later works he calls a "linguistic framework".[7]

An important aspect of this transformation is the rigorization of each proposal within the context of a suitable formal language. This reconstruction of the original assertions serves at least three functions: (i) It helps to make clear exactly what is being proposed and the consequences thereof; (ii) The transformation demonstrates that whereas the initial sentences seemed to be assertions about objects in the world,

[6] Carnap (1963, p. 45). Cf. p. 41, where Carnap laments the fact that even when philosophical progress is made, this fact is often missed. In a personal anecdote from his time at the University of Chicago in the 1930s, he recalls that in spite of the fact that the ontological argument had been shown invalid by the likes of Kant, Frege, and Russell, it was still being considered not only for its historical interest, but as a live topic of study. Carnap attributes this to a lack of understanding of the decisiveness of logical methods.

[7] See especially Carnap's article "Empiricism, Semantics, and Ontology" ([1950]1956). Hereafter I shall refer to this article as *ESO*.

in fact this is a confusion, as they are best understood as about language; and (iii) The process of reconstruction places stress upon the idea that all assertions are made relative to some set of purposes, are applicable only in some limited domain, and thus many terms and assertions turn out to be *language-relative*.

Taking all this into account, Carnap suggests that the realist may more perspicuously express her thesis as

> **[1b:]** Every sentence in which a thing-designation [say, a name] occurs is equipollent to a sentence in which space-time co-ordinates and certain descriptive fuctors (of physics) occur. (Ibid.)

Whereas the idealist could rigorously express her thesis as

> **[2b:]** Every sentence in which a thing-designation occurs is equipollent to a class of sentences in which no thing-designations but sense-data designations occur. (Ibid.)

Each of these sentences can be further and uncontroversially translated into a completely rigorous formal language (e.g., the predicate calculus), and so the implications of each thesis should be reasonably clear. Notice also that since the disputants agree upon all of the scientific evidence that could be brought to bear upon the issue, by transforming their further, philosophical assertions 1a and 2a into rules of a language, or into proposals for how to best structure the language we wish to use to express that evidence, none of the empirical content of the original evidence or scientific theories need-be lost.

Once transformed into these meta-level assertions about the possible structure of our language rather than object-level assertions about the world, it becomes clear that the theses are not incompatible with each other, as sentences formulated using any of the three forms of expression (thing-designations, descriptive functors, or sense-data designations) are mutually equipollent when considered in the same language. We can therefore think of each reformulation as just a possible rule to include in our language, i.e., we can structure our language such as to explicitly include 1b, 2b, neither, or both. Each assertion is just the suggestion to limit the forms of expression in our language to one or more of the available modes. Were we alternately investigating the structure of a previously-established language, it would be a simple matter of logic as to whether or not the equivalences asserted by 1b and 2b actually held in that language. Carnap has thus transformed a seemingly intractable philosophical debate into a question of logic or a choice of linguistic rules.

Notice as well that in the case of language construction there is no longer even a question as to the *truth* of either thesis, since they become mere proposals for different ways of phrasing the same empirical content. Or in other words, each thesis is transformed into an assertion regarding which linguistic convention would be most fruitful and psychologically helpful to adopt for the discussion of empirical theories. While conventions can be more or less fruitful, they cannot be true or false. Carnap concludes that "*the controversy between positivism and realism is an idle dispute about pseudo-theses*" (Ibid. Original emphasis.).[8] Where a "pseudo-thesis", or more precisely a *pseudo-object sentence*, is a sentence which superficially (perhaps because of its surface grammar) seems to be about objects, but upon analysis is best taken as a meta-level assertion about language, as with 1a and 2a.[9]

Example: Intuitionism vs. Classical Mathematics

Another, much more involved example is Carnap's treatment of the debate in the foundations of mathematics between intuitionists who, like Brouwer, argue that the forms of proof in mathematics should be restricted (e.g., proof by contradiction is barred because it is not constructive), and classical mathematicians. Rather than arguing for or against one of these positions, in *Logical Syntax* Carnap instead shows that treating both as proposals regarding the forms of inference we wish to admit into our language as valid allows for a resolution to the controversy in the same manner as above:

> Once the fact is realized that all the pros and cons of the Intuitionist discussions are concerned with the form of a calculus, questions will no longer be put in the form: "What *is* this or that like?" but instead we shall ask: "How *do we wish to arrange* this or that in the language to be constructed?" or, from the theoretical standpoint: "What consequences will ensue if we construct a language in this way or that?" (*Logical Syntax*, pp. 46–47. Original emphasis.)

The controversy between these positions is removed by recognizing that the questions and assertions which seem to be contentful theses about the world—which methods of

[8]In later work Carnap more often addresses the debate between scientific realists and instrumentalists, regarding whether our scientific theories are really "true" and so accurate guides to ontology, or merely useful calculating devices for predicting our future experiences. He addresses this debate in pretty much the same way. Cf. Carnap ([1966]1974, p. 256).

[9]Cf. Carnap ([1932]1960; [1935]1996) and *Logical Syntax*, Part V. We will discuss the notion of a 'pseudo-object sentence' in more detail in the Anti-Metaphysico Philosophical Interlude.

proof are *correct*—can be transformed into meta-linguistic questions and assertions about the most fruitful way to structure our language.

Again, a key feature of Carnap's method is the rigorous formalization of each position. The rational reconstructions here help us to recognize that the question of which thesis is "correct" can be treated as a question of which object-language we wish to use, and they also serve to help us in making our choice. Once the theses are reconstructed, we can investigate the meta-logical properties of each and weigh their fruitfulness for our purposes.[10] In effect, what Carnap is doing here is offering a mathematical solution to what has ostensibly become a mathematical problem. The real question at issue regards the modes of inference that we are comfortable admitting into our language. The best way to make progress on this question is to investigate the properties and consequences of different formal systems that encapsulate the various options. Carnap supposes that the restrictions advocated by intuitionism result in a language safer from contradiction, whereas the wider means of expression available in classical mathematics is better suited to formulating the theories and methods of empirical science.[11] Arguing over a metaphysically-loaded question of "correctness" ends up beside the point.

As with philosophical questions and assertions, what facilitates Carnap's approach here is the insight that logico-mathematical sentences lack empirical content. Extending Wittgenstein's doctrine of tautology, Carnap argues that we can take the truth of a logico-mathematical sentence to be independent of the contingent state of the world. Instead, logico-mathematical sentences are *analytic*, which is sometimes cashed-out as true solely in virtue of the meanings of the logical constants of the sentence.[12] Likewise, purely logico-mathematical sentences do not by themselves imply any factual sentences about the nature of the world's actual configuration. This

[10] We will see that this is something Carnap actually does in *Logical Syntax*. His Language I is a primitive recursive arithmetic, meant to act as the formal representation of finitist and constructivist tendencies. His Language II, on the other hand, is powerful enough to recover most of classical mathematics, including set theory. Hereafter I will refer to Carnap's example languages from *Logical Syntax* as LI and LII respectively.

[11] Consider: "It is true that certain procedures, e.g., those admitted by constructivism or intuitionism, are safer than others. Therefore it is advisable to apply these procedures as far as possible. However, there are other forms and methods which, though less safe because we do not have a proof of their consistency, appear to be practically indispensable for physics. In such a case there seems to be no good reason for prohibiting these procedures so long as no contradictions have been found." (Carnap, 1963, p. 49).

[12] This is strictly speaking incorrect within the context of *Logical Syntax*, since that work defines its concepts (usually) without reference to meanings at all. But this characterization is accurate after his adoption of semantics. E.g., in his reply to Quine's paper in the Carnap-Schilpp volume (Carnap, 1963, p. 916), Carnap endorses this gloss.

means that logic and mathematics can be treated as a set of syntactical rules—i.e., a set of linguistic conventions—which are constitutive of the deductive structure of our language. Approaching logic and mathematics in this way, questions of truth or correctness become inappropriate. Only questions of the fruitfulness of certain modes of expression with regard to the purposes at hand are legitimate. Carnap elaborates upon this idea in *Foundations of Logic and Mathematics*:

> Concerning mathematics as a pure calculus there are no sharp controversies. These arise as soon as mathematics is dealt with as a system of "knowledge"; in our terminology, as an interpreted system. Now, if we regard interpreted mathematics as an instrument of deduction within the field of empirical knowledge rather than as a system of information, then many of the controversial problems are recognized as being questions not of truth but of technical expedience. The question is: Which form of the mathematical system is technically most suitable for the purpose mentioned? Which one provides the greatest safety? (Carnap, 1939, p. 50)

However, whereas those philosophical assertions that admit of transformation become useful primarily for their descriptive and psychological applications, Carnap stresses that formally reconstructed logico-mathematical theories characterize the possible inferential structures of the language of science. Indeed, according to Carnap, logico-mathematical sentences are most usefully thought of as comprising a set of "formal auxiliaries"—inferential and calculating machinery—which make possible prediction and explanation when interpreted over the contentful sentences of a language. This is the role that Carnap takes logic and mathematics to play in the sciences.

1.1.2 Conventionalism and the Principle of Tolerance

It is from this attitude toward logic and mathematics that Carnap derives his well-known logical pluralism. Differing choices of syntactical rules will result in differing inferential structures for an object-language. And since the choice of syntactical rules in the construction of a framework is just not the kind of thing that can be judged correct or incorrect, there is a large element of convention in the construction of and choice between linguistic frameworks.

These ideas are expressed in Carnap's (in)famous Principle of Tolerance:

> **[The Principle of Tolerance:]** *It is not our business to set up prohibitions, but to arrive at conventions.*
>
> [. . .]
>
> *In logic, there are no morals.* Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments. (*Logical Syntax*, pp. 51–52. Original emphasis.)

Returning to our previous example, the choice of a limited set of syntactical rules constituting only those modes of inference licensed by intuitionistic logic (e.g., LI) as the basis of our linguistic framework would limit the kinds of inferences we can represent when compared to the use of a set of more expressive syntactical rules including the axiom of choice, unbounded quantification, etc. (e.g., LII). As we saw, the choice between these two frameworks is what remains of the philosophical dispute between intuitionism and classical mathematics in Carnap's program.[13]

But of course this is not all that needs to be done. Carnap tells us that the choice of one set of syntactical rules over another is based upon practical considerations: fruitfulness, expedience, simplicity, etc. However, such considerations must themselves rest upon the meta-logical investigation of the various possible choices of framework. An inconsistent set of syntactical rules acts as an unfruitful and inexpedient choice for the logico-mathematical portion of our linguistic framework, to take an obvious example, because (*modulo* an assumption about typical rules of inference) from such a set of sentences anything whatever follows. So such a framework is useless as a base logical language for axiomatizing empirical theories, one of the key aims of Carnap's project.[14] We thus sometimes discover the relevant properties of

[13]Similarly for inductive logic. We may choose to supplement our language with rules that permit only very basic inductive generalizations. Or we may choose to include rules that implement a more sophisticated representation of the confirmation of hypotheses by evidence with, e.g., Bayesian conditionalization. This is Carnap's very mature view, after he had begun serious work on probability. Interestingly, in *Logical Syntax* Carnap takes a much more negative stance toward the possibility of a rigorous inductive logic: "One sometimes speaks in this connection of the method of so-called *induction*. Now this designation may be retained so long as it is clearly seen that it is not a matter of a regular method but only one of practical procedure which can be investigated solely in relation to expedience and fruitfulness." (*Logical Syntax*, p. 317). Compare to Carnap (1945; 1950).

[14]Cf. Carnap ([1954]1958), Carnap's introductory logic text, where the focus is placed squarely upon the *application* of logic. In fact, the entire second half of the book is given over to the axiomatic reconstruction and investigation of various empirical and mathematical theories (including, e.g., basic concepts of biology!). He takes as their base language the "extended language C" presented in the first half of the book, which is roughly equivalent in power to LII. He also discusses the value

candidate frameworks through meta-logical investigation—in other words, *via typical mathematical methods*. This point will be of key importance below when discussing the historical criticisms of Carnap's program, the Deflationary response, and the extension of Carnap's ideas to modern foundational debates.

P-Rules and a Constraint on Tolerance

It is also important to stress that our choice of framework is meant to be neither final nor absolute. For example, after choosing LII as the logico-mathematical component of our framework, it may later be discovered that classical mathematics is inconsistent. In this very unlikely scenario the adoption of a weaker set of syntactical rules as the logico-mathematical component of our framework would be prudent for the reasons noted in the previous paragraph. And besides, different frameworks will be useful for different purposes.

The transient nature of our choice of framework is even more important to recognize with respect to the *physical syntactical rules*, or P-Rules, of a language, should our framework of interest include any. Carnap takes the general replaceability of the descriptive predicates in a primitive sentence, or the general replaceability of the descriptive predicates in the sentences of an inference schema, as the characteristic feature of the *logical*, or L-Rules, with all others being P-Rules.[15] P-Rules are meant to encode laws of nature that we might for certain purposes want to include as part of the inferential structure of our linguistic framework. For example, in a framework which characterizes classical mechanics we might supplement the usual logico-mathematical rules with physical rules corresponding to Newton's laws of motion. These P-Rules are still constitutive of the framework in question, in the same way as the L-Rules, but Carnap arranges the meta-logical definitions in *Logical Syntax* in such a way that physical rules and their consequences are not analytic, but rather synthetic as we would expect.[16]

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of axiomatic methods for clarifying problems and concepts in the target theories. Perhaps because of the purpose of the book—an introductory text—many of Carnap's doctrines are expressed in an uncharacteristically categorical tone.

[15]Recognize that this distinction depends upon a previous distinction of the vocabulary into Logical and Descriptive components. The details of Carnap's definitions will be saved for the Logico-Mathematical Interlude.

[16]Carnap distinguishes between sentences which are L-Valid, or in other words follow from or by the purely logical rules of a framework (i.e., are analytic), and sentences that are P-Valid, or in other words follow from or by the physical rules (i.e., are synthetic). *Indeterminate* sentences are neither L- nor P-Valid, since their truth cannot be determined on the basis of the rules of the framework alone. As with P-Rules and their consequences, indeterminate sentences are also synthetic. I should note that the terms 'L-Rule' and 'P-Rule' may seem ambiguous. This is because Carnap counts

Besides highlighting the idea that our choice of framework is situational—we would only utilize a framework including P-Rules corresponding to classical mechanics if we were interested in studying the inferential structure of classical mechanics or some question or concept involving classical mechanics—the point of introducing this complexity at this stage is to raise the question as to the conventionality of the P-Rules. As with the logico-mathematical rules, is it equally inappropriate to call certain proposed P-Rules false or otherwise incorrect? Or, in other words, does the Principle of Tolerance extend to the choice of the empirical component of a framework in such a way that a set of P-Rules is also to be considered no more than a linguistic proposal?

In *Logical Syntax*, Carnap states that the choice of *whether or not to include* P-Rules in our framework at all is indeed a matter of convention:

> We may, however, also construct a language with *extra-logical rules of transformation*. The first thing which suggests itself is to include amongst the primitive sentences the so-called laws of nature, i.e. universal sentences of physics ('physics' is here to be understood in its widest sense). [...] Whether in the construction of a language S we formulate only L-rules or include also P-rules, and, if so, to what extent, is not a logico-philosophical problem, but a matter of convention and hence, at most, a question of expedience. (*Logical Syntax*, p. 180. Original emphasis.)

Alternately, we might introduce the laws of motion as indeterminate premises rather than as P-Rules. But being told which types of sentences we are licensed to include as syntactical rules does not really answer the question, which asked whether our choice of P-Rules—being that they are *descriptive* and presumably the formal correlates of *empirical hypotheses*—is restricted in some sense. Unfortunately, in *Logical Syntax* Carnap does not treat any examples.

However, in chapters 2 and 3 we will look to some of Carnap's other work to show that there are indeed significant restrictions placed upon the selection and formulation of both physical *and logico-mathematical* sets of rules. At the least they must be *adequate* as formal corollaries of the scientific theory or concept that they are meant to reconstruct.[17] This involves capturing certain characteristic features of

both primitive sentences (i.e., axioms) and rules of inference (both rules of derivation and transfinite rules of consequence) as what he calls *Transformation Rules*. So, e.g., both the axiom of choice and *modus ponens* count as L-Rules. This is why I said "follow *from or by*" in the explanations above.

[17] As with Carnap, I will in the sequel construe 'science' broadly, to include also mathematics and the "inexact" sciences such as psychology or economics.

the target concept or theory—i.e., of the *explicandum* to be explicated. Regarding the adequacy of physical rules, Carnap does motion in this direction in §82 of *Logical Syntax*, the only other discussion of P-Rules of any significant length in the book:

> *The construction of the physical system is not effected in accordance with fixed rules, but by means of conventions.* These conventions, namely, the rules of formation, the L-rules, and the P-rules (hypotheses), are, however, not arbitrary. The choice of them is influenced, in the first place, by certain practical methodological considerations (for instance, whether they make for simplicity, expedience, and fruitfulness in certain tasks). This is the case for all conventions, including, for example, definitions. (*Logical Syntax*, p. 320. Original emphasis.)

The context of this passage is an extremely skeletal discussion of the way we might go about reconstructing a physicalist (as opposed to phenomenalist) language that would be suitable as a *total language of science*, that is, including the formulation of our best empirical theories and their methods. He still does not develop any specific examples, instead keeping the discussion at a very general level. However, he continues:

> But in addition the hypotheses *can and must be tested by experience*, that is to say, by the protocol-sentences—both those that are already stated and the new ones that are constantly being added. [...] That hypotheses, in spite of their subordination to empirical control by means of the protocol-sentences, nevertheless contain a conventional element is due to the fact that the system of hypotheses is never univocally determined by empirical material, however rich it may be. (Ibid. My emphasis.)

Protocol-sentences are taken by Carnap to encode a scientist's basic observations, which she can then compare to the independently derived protocol-sentences that are L- and P-consequences of the empirical theories formulated in a framework.[18] Carnap's model of the methodology of science in *Logical Syntax* is thus that we use observations (formulated as protocol-sentences) to test scientific laws and hypotheses (formulated as P-Rules or indeterminate premises) according to a basic hypothetico-deductive method. Leaving to one side the naïvety of this model, what is interesting is that Carnap locates the "conventional element" of the P-Rules as a result of the

[18]Cf. Carnap ([1932]1987) for a fuller discussion of protocol-sentences. We will return to this topic in chapter 3.

underdetermination of theories by evidence, and not substantially as a matter of any kind of purely linguistic choice that we have in the construction of a framework.

What this discussion suggests is a methodological constraint on the Principle of Tolerance. The overarching purpose of Carnap's meta-philosophical program is the logical analysis *of science*. The first step in such an analysis must be the adequate characterization of the relevant parts of science (including mathematics) in formal terms. In other words, the formulation of syntactical rules must be dictated by the concepts and theories we mean to explicate. If your linguistic framework fails this first step, then *you are doing it wrong*. While Tolerance provides us great flexibility in choosing the form of our language, we must remember that its construction serves always a practical end, and so our choices are constrained at least in this sense.

1.1.3 Should We Be Tolerant About Tolerance?

A related question we have yet to address is the scope of Tolerance in the sense of its self-applicability to Carnap's own program. We have just seen that our choice of syntactical rules for particular linguistic frameworks is constrained given the purpose Carnap envisions for his project as a whole. But, taking a step back, is the Principle of Tolerance applicable to our choice regarding the methods of philosophy? Or to the meta-logical concepts that Carnap develops in *Logical Syntax*? Should we be Tolerant about the Principle of Tolerance? These are questions that we will address more fully in chapters 2 and 3, since they are intimately connected to both the analysis of the historical objections we will treat and the tenability of the Deflationary reading.

For the moment I want to stress the point flagged in the previous section, that the philosophical methods recommended by the Logic of Science are in the main meta-logical—in other words, they are *mathematical methods*. This comports with what I noted in the beginning as the general spirit of Carnap's meta-philosophy: That philosophers are in fact scientists (broadly construed), and so should rely upon the very same methods as the sciences. The question is simply whether those methods end up being empirical or mathematical (for Carnap this means 'formal'), and this depends upon whether philosophy is in the main a formal or an empirical science.

Philosophy as Science

This question is explicitly addressed in *Introduction to Semantics*, where in an appendix Carnap reflects back upon *Logical Syntax* and reformulates some of the positions therein to take account of his adoption of semantics:

> The *chief thesis* of Part V [of *Logical Syntax*], if split up into two components, was like this:
>
> a. "(Theoretical) *philosophy* is the logic of science."
>
> b. "Logic of science is the syntax of the language of science."
>
> (a) remains valid. It is a terminological question whether to use the term 'philosophy' in a wider sense, including certain empirical problems. If we do so, then it seems that these empirical problems will turn out to belong mostly to pragmatics. Thesis (b), however, needs modification by adding semantics to syntax. Thus *the whole thesis is changed to the following: the task of philosophy is semiotical analysis*; the problems of philosophy concern—not the ultimate nature of being but—the semiotical structure of the language of science, including the theoretical part of everyday language. We may distinguish between those problems which deal with the activities of gaining and communicating knowledge and the problems of logical analysis. Those of the first kind belong to pragmatics, those of the second kind to semantics or syntax—to semantics, if designata ("meaning") are taken into consideration; to syntax, if the analysis is purely formal. (Carnap, [1942]1975, p. 250. Original emphasis.)

The "empirical problems" that Carnap mentions are as he says mostly questions of *pragmatics*, which he takes to include the physiological, psychological, and sociological aspects of language and communication. But this class includes also the studies of *descriptive semantics* and *descriptive syntax*. That is, the semantical and syntactical analysis of historically-given languages rather than explicitly constructed linguistic frameworks. Carnap argues that these descriptive studies are fundamentally dependent upon pragmatics, in the sense that their object of study is determined entirely by the speaking habits of people. In §5 of *Introduction to Semantics* he encourages us to consider this entire set of studies to be properly a part of the empirical science of *linguistics*.

This leaves to philosophy the studies of *pure syntax* and *pure semantics*—in other words the meta-theoretical investigation of explicitly constructed linguistic frameworks, including formalizations of purely logical languages of many forms, frameworks supplemented with canonically mathematical rules, and those supplemented also with P-Rules. But philosophy also includes the development of the meta-theoretical concepts that are utilized in these investigations. Carnap tells us that this is the primary purpose of *Logical Syntax*:

> In recent years, logicians representing widely different tendencies of thought have developed more and more the point of view that in this context [an "expository context" wherein the signs and rules of the object-language are explained] is contained the essential part of logic; and that the important thing is to develop an exact method for the construction of these sentences about sentences. The purpose of the present work is to give a systematic exposition of such a method, namely, of the method of "logical syntax". (p. xiii)

Indeed, historically speaking *Logical Syntax* is one of the first explicitly meta-logical texts. So on Carnap's view philosophy is concerned with the analysis of the possible forms of language *and* the development of suitable meta-languages and concepts for this task. Thus the statements and conclusions of philosophy are in the main analytic, without empirical content. Hence formal, mathematical methods are appropriate in philosophy.

But if the statements and conclusions of philosophy are themselves analytic, then there is reason to think that the Principle of Tolerance would apply to them as well, and Carnap's program as a whole becomes a matter of practical considerations. In other words, the Principle of Tolerance becomes itself a linguistic proposal to adopt Tolerance as part of our philosophical methodology. Consider, for example, a dispute about the correct methods of philosophical investigation—what we might characterize as a *meta-philosophical dispute*. Applying Carnap's own rubric to such a dispute, we would transform each meta-philosophical program into a proposal to adopt a certain set of conventions, a certain set of syntactical rules which codify the suggested methods of each program. The choice between them becomes just a matter of practical considerations: convenience, fruitfulness, simplicity, etc.

Although there may be an air of circularity here, it does not seem especially vicious. This same sort of self-applicability does however become a potential problem with respect to some of the basic presumptions of the Logical Empiricist position, especially verificationism and mathematical conventionalism. We now turn to circularity criticisms of this sort.

1.2 A Bittersweet Renaissance—Circularity Objections

As surveyed at the outset, the last twenty years has seen revitalized interest in Logical Empiricism. However, in many philosophical circles the overwhelming sentiment toward Carnap's ideas—where they are considered at all—often continues to be one of casual dismissal. For example, while Thomas Mormann is himself an active Carnap scholar, in his ultimately positive review of Carus' (2007) book he labels the assertion that Carnap's philosophical views and particular "style of philosophy" remain relevant today a "bold thesis" (Mormann, 2008, p. 263). This is obviously in stark contrast to the attitude taken here.

An even less sympathetic source is Scott Soames' (2003) two-volume survey of 20th century analytic philosophy. Soames devotes only two chapters (of 34) to discussion of Logical Empiricism, relying upon Ayer's *Language, Truth, and Logic* ([1946]1952) as his primary source. Soames claims that the movement ultimately succumbed to "intractable difficulties" (p. 270) stemming from attempts to formulate an empiricist criterion of meaning,[19] and to Quine's famous arguments against a logico-mathematical conventionalism in "Truth by Convention" ([1935]1976) and against the analytic/synthetic distinction in "Two Dogmas of Empiricism" ([1951]1980).[20]

Given what has already been said about Carnap's attitude toward philosophical disputes, it is somewhat ironic that much of the less dismissive recent attention paid to Logical Empiricism has amounted to constant re-examinations of the seemingly intractable disputes engendered by the movement, such as the aforementioned Carnap-Quine debate on the analytic/synthetic distinction.[21] Our analysis in chap-

[19]The difficulty in relying upon Ayer's book as one's primary source for the doctrines of Logical Empiricism is that Ayer neglects developments and refinements made during the 1930s, arguably the movement's most productive and philosophically sophisticated period. For example, there is little mention in Ayer's book of *Logical Syntax*, and no mention of the Principle of Tolerance. But it has been persuasively argued (by for example Creath (1990), Ricketts (1996), and Carus (2007)) that Carnap's views changed radically during the writing of *Logical Syntax*, and it was the doctrines therein that were the core of his mature view. Furthermore, in his Intellectual Autobiography (1963, pp. 56–59) Carnap relates the development of his views from the late 1920s through to the 1930s from a straightforward verificationism such as the kind discussed by Ayer to the more sophisticated account presented in "Testability and Meaning" (1936; 1937). Finally, there is little to no mention made by Ayer of Carnap's emphasis upon a distinction between observational and theoretical terms—a distinction essential to an effective empiricist criterion of meaning. Cf. Demopoulos (2007) for the philosophical import of Carnap's mature view.

[20]Cf. Quine's contribution to the Carnap-Schilpp volume, Quine (1963).

[21]Although I suppose Soames at least does not see the situation as being intractable. Creath's (1991) compilation of the Carnap-Quine correspondence is a vital resource for the analysis of this

ter 2 will instead focus on criticisms such as Quine's earlier (Quine, [1935]1976) and later (Quine, 1963) writings regarding Carnap's views on mathematics.

1.2.1 Carnap's Philosophy of Mathematics

As observed above, Carnap's philosophical methodology involves in large part the construction and meta-theoretical investigation of linguistic frameworks—basically formal systems in the usual sense. These frameworks will of course be of varying mathematical strengths, as with LI and LII in our example from §1.1.2. We know—thanks to Gödel ([1931]1992)—for frameworks including at least elementary arithmetic, that if we want to prove mathematically interesting theorems about them, then the meta-language used in their investigation must be strictly more expressive than the object-languages themselves.[22]

This is a potential problem for Carnap's program if one of his aims is to offer his views on logic and mathematics as providing what has traditionally be thought of as a *system of foundations* for mathematics. In other words, as presenting an account of, or purporting answers to questions regarding, the nature of logico-mathematical truth, our knowledge thereof, or its justification. But considering his "Intellectual Autobiography", it seems that this is indeed how the Logical Empiricists thought of their extended doctrine of analyticity:

> What was important in this conception from our point of view was the fact that it became possible for the first time to combine the basic tenet of empiricism with *a satisfactory explanation of the nature of logic and mathematics*. Previously, philosophers had only seen two alternative positions: either a non-empiricist conception, according to which knowledge in mathematics is based on pure intuition or pure reason, or the view held, e.g., by John Stuart Mill, that the theorems of logic and of mathematics are just as much of an empirical nature as knowledge about observed events, a view which, although it preserved empiricism, was certainly unsatisfactory. (Carnap, 1963, p. 47. My emphasis.)

What Carnap is discussing here is what I will call the *Problem of Empiricism*: How

debate, and his commentary offers a more balanced take on the issue. George (2000) and Lavers (2012) also provide very good, although not altogether agreeing, analyses that have the potential to move the discussion forward.

[22] In fact, Devidi & Solomon (1995) demonstrate with regard to Carnap's program that in many cases a meta-language of equal strength to the object-language is all that is required. Interesting as they are, these results have limited bearing upon our discussion.

is a robust empiricism to account for logic and mathematics, which seems to furnish non-empirical, *a priori* knowledge? Wittgenstein's understanding of logic as tautologous provides a solution, since logic is not considered to be properly knowledge. Rather, it is the inferential and structural residue that results from the representational function of language. The Logical Empiricists adopted this conception of logic, extending it to mathematics as well. Carnap's more nuanced view, as expressed in *Logical Syntax* and *Foundations of Logic and Mathematics*, which considers logico-mathematical sentences as sets of L-Rules without empirical content and constitutive of the deductive structure of their linguistic framework (i.e., as sets of formal auxiliaries), can be seen as a rigorization of this traditional Logical Empiricist view. It is also positioned here as an *alternative* to Kantian or empiricist foundations.

Several of Carnap's most eminent interlocutors—including philosophers no less than Gödel ([1953/9]1995), Quine (1963), and Beth (1963)—have taken Carnap to be doing just this: Arguing for what we might call a *conventionalist* foundations for mathematics. In other words, as arguing that mathematics is analytic and without empirical content, so that there is great variability in the choice of logico-mathematical system that we use, and that the truth of logico-mathematical theorems and our knowledge thereof can be accounted for purely formally, deriving somehow from our choice of linguistic principles. However, these authors have all observed in various ways that the above-mentioned technical situation with Gödel's theorems means that this conventionalist program seems to be viciously circular in its need to *presuppose* at some level the very notions for which the program was supposed to provide an account. To see this, note that for any given framework of interest, Carnap *must* rely at the meta-level upon a set of mathematical principles at least as powerful as those he is treating to demonstrate that the mathematics of the framework is purely a matter of the syntactical rules we have chosen for that framework. Carnap is thus accused of requiring an element of mathematical intuition or experience after all, at the meta-level, and so the Logical Empiricist thesis fails.

1.2.2 A Possible Carnapian Reply

In the sequel I will call the various criticisms of this sort *circularity objections*. Chapter 2 provides a detailed treatment of the objections from the authors mentioned. I will argue that Carnap's program avoids these objections in the main because he is not attempting to provide a foundation for mathematics in the way his interlocutors suppose. Instead, we will see that Carnap is providing an *explication* of logical and arithmetical truth—an analysis and replacement of colloquial notions with precise definitions in a systematized context. My interpretation of Carnap's philosophy of mathematics thus responds to the circularity objections by placing emphasis upon the idea that the formulation and choice of syntactical rules for a linguistic framework are motivated by Carnap's prior methodological understanding of the sciences. His reconstructions are then constrained by the need to adequately capture the original *explicanda*, and this allows his explications to be informative even though his treatment is not foundational. And since Carnap is not concerned to furnish a foundation, but is instead focused on bringing mathematical methods into philosophy, he is licensed to use meta-languages of any required strength to carry out his reconstructions and analyses.

Notice that this again suggests limitations upon the Principle of Tolerance, now in two distinct ways: (i) Our choice of syntactical rules is constrained by the informal theories we are attempting to reconstruct; more vitally, (ii) Recall that Carnap's dissolution of philosophical disputes is amenable to Tolerance precisely because such controversies are linguistic matters. Similarly, we can appeal to the Principle of Tolerance in regard to our choice of logico-mathematical principles only if that choice is a formal, rather than a factual, matter. Thus the application of Tolerance is *licensed* and *justified* by our ability to treat logic and mathematics as a formal science. In other words, recognition of mathematics as a formal science is methodologically prior to Tolerance in Carnap's meta-philosophy. But this is not to say that Carnap argues for a conventionalist foundation for mathematics—I just indicated that he did not do this. Rather, Carnap must only show it plausible in his reconstruction of the language of science that we can treat logic and mathematics as a set of formal auxiliaries and yet recover the characteristic features we take those disciplines to have. We will see that this is exactly what he does.

1.3 Interpreting Carnap—A Deflationary Logic of Science?

A different way to approach the circularity objections is to lean upon Carnap's Principle of Tolerance. As suggested in §1.1.3, a broad reading of Tolerance takes Carnap's rubric for dissolving philosophical disputes and applies it at the meta-level, to Carnap's entire program. It is thus tempting to see Carnap's meta-philosophical program as consisting *exclusively* of the construction and formal investigation of linguistic proposals. Taking this idea to its extreme, one can interpret Carnap as making *no assertions at all*, philosophical or otherwise, but merely advancing proposals in all cases. For example, Creath arguably skirts quite close to describing Carnap's program in these terms:

> Philosophers will make proposals for the logic of science, that is, for the structure of reasoning within science, and cooperatively explore the technical consequences of adopting them. The standard of appraisal for the proposals is their utility within science. Thus, *philosophy is considered as a kind of conceptual engineering that serves science* rather than a mysterious enterprise that somehow locates its own domain of facts that are deeper than those that science can reveal. (Creath, 2007, p. 331. My emphasis.)

This formulation does not exclude the possibility that Carnap's program may involve more that mere "conceptual engineering" in the service of science, but it does suggest that accepting Carnap's replacement of traditional philosophical methods is to discard entirely the possibility of philosophy's contributing to the ever-increasing stock of knowledge discovered by the sciences. Instead, philosophers are to spend their time developing new and more useful "structures of reasoning" for practicing scientists, but remain fundamentally disengaged from the scientific enterprise itself.

1.3.1 The Deflationary Reading

Goldfarb and Ricketts' Deflationary reading is an even more radical interpretation along these same lines, but approached from a slightly different direction. The leading idea attributed to Carnap on the Deflationary reading is a principle they call

> **Logocentrism:** Our choice of linguistic framework includes the choice of all epistemic standards for justification, truth (*viz.*, analyticity), and correctness, and therefore all such notions must be language-relative.[23]

I call this a 'principle' although it is actually quite a strong assumption. It is a formulation of Carnap's insight that philosophical assertions are always made relative to a particular language or set of languages, in combination with the idea that

[23] This is my own formulation of the principle, which is only occasionally labeled as such by Goldfarb and Ricketts—e.g., Ricketts (1994, pp. 182–183). However, we will see that the assumption that this is the guiding idea of Carnap's meta-philosophy is instrumental to the Deflationary interpretation. Not surprisingly, this is also their largest interpretive misstep.

our choice of syntactical rules for a linguistic framework is constitutive of the inferential structure and methods of possible justification for that language. Recall as well that the Principle of Tolerance suggests we offer linguistic proposals in matters of logic rather than making philosophical assertions. Now, since all inquiry (philosophical or otherwise) presupposes some language, Goldfarb and Ricketts conclude that the Principle of Tolerance must extend to the justificatory basis of all possible investigation, and so Tolerance acts as the foundation of Carnap's entire program.

Here is Goldfarb rehearsing this reasoning:

> A linguistic framework is given by the rules of formation of sentences together with the specification of the logical relations of consequence and contradiction among sentences. The fixing of these logical relations *is a precondition for rational inquiry and discourse.* There are many alternative frameworks, many different logics of inference and inquiry. Since *justification can proceed only grounded in the logical relations of a particular framework, justification is an intraframework notion.* Thus there can be no question of justifying one framework over another. Carnap voices this pluralistic standpoint in his Principle of Tolerance. (Goldfarb, 1996, p. 225. My emphasis.)[24]

The consequence of this logocentric reading is that for Carnap, philosophers are exclusively in the business of formulating and proposing linguistic frameworks, i.e, philosophical methodology is simply a matter of language construction or "conceptual engineering" as Creath says. This is because any philosophical assertion that might be made is recognized as embedded within a particular linguistic framework, and the Principle of Tolerance suggests that our choice of framework is not something that can be correct or incorrect. Indeed, to suggest that some framework or set of syntactical rules is correct, or even adequate, must itself be done from within a language we have presumably already adopted. Thus, there can be no neutral Archimedian platform set apart from any and all linguistic frameworks, a place from which to judge absolutely such questions. The methodological recommendations of the Principle of Tolerance thus seem our best guide, and so all philosophical questions are transformed into matters of language choice.

To return again to the discussion in §1.1.3, the all-encompassing scope of the Principle of Tolerance on the Deflationary reading takes seriously the idea that Carnap's program should itself not be read as an assertion about the correct way to

[24]Cf. Goldfarb (1995*b*, p. 326) for an earlier iteration of this same point.

practice philosophy, but as a linguistic proposal—a recommendation to adopt a language which expresses these methods and goals. Within the context of Carnap's preferred languages—empiricist languages—the *a priority* of logic and mathematics can be accounted for via his conventionalism. But this is simply the form of language Carnap prefers. There is no non-circular way for Carnap to *rationally justify* this choice of language over any other. Hence Goldfarb and Ricketts' response to the circularity objections: The Carnap of *Logical Syntax* has simply given up the traditional foundational goals of providing any sort of broadly applicable analysis of the epistemic character of logico-mathematical knowledge, explaining its role in the sciences, or furnishing an account as to how we come to grasp this knowledge.

In fact, the consequences are even more radical than this eschewal of the philosophy of mathematics. With the Principle of Tolerance acting as the sole methodological guideline for Carnap's program, the basic Logical Empiricist tenets of verificationism, empiricism, and the conventionality of logic and mathematics do indeed become nothing but proposals for how to structure our language conveniently for the purpose of scientific investigation. Consider:

> In a word, I am suggesting that Carnap's position in *LSL* is *deflationary.* It is not based on any substantial theoretical commitments of its own. (Goldfarb, 1997, p. 61. Original emphasis.)[25]

From this vantage, even the Principle of Tolerance itself is not something Carnap can *argue* we should adopt, since that would be to forward a philosophical thesis:

> [T]he principle of tolerance itself is *not a thesis, but a proposal, the expression of an attitude or standpoint.* The principle of tolerance is not formulatable as a statement in a Carnapian language. There is no question of "correctness" that is applicable to it. (Ricketts, 1994, p. 196. My emphasis.)

On the Deflationary reading, Carnap's meta-philosophical program is merely an invitation for clarification in all cases, with Tolerance acting as an expression of the *attitude* of logical pluralism—the attitude which Carnap prefers.

[25] And again: "Rather, from the specification of the positivist picture [in *Logical Syntax*], we learn that positivism is not essentially a combination of empiricism and conventionalism." (Ricketts, 1994, p. 177)

1.3.2 Some Evidence for the Deflationary Reading

Despite the substantial number of articles outlining their interpretation, Goldfarb and Ricketts provide surprisingly little textual evidence for their reading. And as we will see in chapter 3, they must actually *explain away* a number of passages in *Logical Syntax* where Carnap seems to contradict their interpretation. For the moment I will only point to two strong pieces of textual evidence in their favour.

In several places Goldfarb and Ricketts quote a passage from Carnap's monograph-length article "Testability and Meaning". He there seems to confirm that he is not actually *asserting* even the Logical Empiricists' most fundamental and consistently held doctrine: empiricism. Rather, Carnap is merely proposing it as a possible way to structure the language of science:

> It seems to me that it is preferable to formulate the principle of empiricism not in the form of an assertion—"all knowledge is empirical" or "all synthetic sentences that we can know are based on (or connected with) experiences"' or the like—but rather in the form of a proposal or requirement. (Carnap, 1937, p. 33)

This also comports with the line of thought developed above that suggests the Principle of Tolerance is self-applicable. Philosophers are not in the business of making assertions, since philosophical theses—like metaphysical theses—do not admit of empirical resolution. Thus, they are a matter of linguistic choice.

Another piece of evidence often noted by proponents of the Deflationary reading is an anecdote reported by Howard Stein of Carnap's remarks during a discussion period occurring in 1951, after a colloquium talk given by Quine at the University of Chicago. The relevant section of Stein's recollections concerns Carnap's description of the difference between himself and Quine regarding the usefulness of introducing formal languages in the analysis of the structure and methods of science.

> This is a difference of opinion which, *despite the fact that it does not concern (in my own terms) a matter with cognitive content*, is nonetheless in principle susceptible of a kind of rational resolution. In my view both programs—mine of formalized languages, Quine's of a more free-flowing and casual use of language—ought to be pursued; and I think that if Quine and I could live, say, for two hundred years, it would be possible at the end of that time for us to agree on which of the two programs had proved more successful. (Stein, 1992, p. 279. My emphasis.)

Just to be clear, these are Stein's words. He is reporting what he remembers Carnap as having said. This is exactly the situation described above of two philosophers in a meta-dispute over philosophical methodology. The passage does seem to suggest that Carnap thought of his own program as a single, all-encompassing proposal, as a linguistic framework concerning one out of an unlimited number of possible meta-philosophies that we might choose to adopt. Quine's naturalist program is simply an alternative framework that we might choose instead.

1.3.3 Objection: The Priority of Science for Carnap

My primary interpretive claim is that despite this evidence, the logocentric reading of Carnap is a mistake. Where it goes too far is actually pointed out by Carnap in the continuation of the passage (quoted at the beginning of §1) from the final pages of *Logical Syntax*:

> Our thesis that the logic of science is syntax must therefore not be misunderstood to mean that the task of the logic of science could be carried out independently of empirical science and without regard to its empirical results. The syntactical investigation of a system which is already given is indeed a purely mathematical task. But the language of science is not given to us in a syntactically established form; whoever desires to investigate it must accordingly take into consideration the language which is used in practice in the special sciences, and only lay down rules on the basis of this. (*Logical Syntax*, p. 332)

Carnap reminds us here that the explication, or rational reconstruction, of the language of science is a key component of his program. This reconstruction is presumably not just for the purpose of providing working scientists new conceptual tools, or for transforming and then dissolving philosophical debates, although these are both promising features of Carnap's program. Beyond this, the purpose of a rigorization of the concepts and theories of science is suggested as a means to provide genuine philosophical insight—by clarifying the inferential structure of a theory, or providing an analysis of a set of concepts, we learn something about those theories and concepts. I think that in Carnap's mind, this is the true value of philosophy.

He continues:

> In principle, certainly, a proposed new syntactical formulation of any particular point of the language of science is a convention, i.e. a matter

> of free choice. But such a convention can only be useful and productive in practice if it has regard to the available empirical findings of scientific investigation. [...] All work in the logic of science, all philosophical work, is bound to be unproductive if it is not done in close co-operation with the special sciences.

Carnap's overall goal is to transform philosophy into a science, to develop a *scientific philosophy* with a method from which genuine progress will result. As with all other areas of science, philosophy cannot be done in a vacuum, apart from consideration of our best scientific knowledge in other disciplines. A key aspect of Carnap's program, then, is that it adequately captures scientific theories and concepts so that Carnap's meta-theoretical tools can be put to use in their analysis. Although there will be a variety of non-equivalent explications for any given theory or concept, and some will be better suited to certain purposes than others, the processes of language construction and explication are not arbitrary.

So the suggestion that we use, e.g., an empiricist language because the methods of science are in the main empirical, is not merely a linguistic proposal. Rather, it has behind it an understanding of the actual practices of science. Similarly in philosophy and mathematics. An analysis of the methods of mathematicians, *qua mathematicians*, demonstrates to Carnap that a broad selection of principles and methods of proof are accepted, as long as one's assumptions are made clear. Carnap argues that philosophy is closer in character to mathematics than empirical science, and so should adopt a mathematical methodology. Therefore, I submit that the Deflationary reading has the methodological hierarchy of Carnap's program right backwards. On my reading of Carnap's work, the Principle of Tolerance is not the foundation of Carnap's entire program, but a methodological principle licensed by his prior understanding of the character of logico-mathematical sentences and the methods used in those formal sciences.

1.4 Summary of What Follows

Thus far we have surveyed some of the main features of Carnap's program, I have introduced my interpretation of that project in contrast to the Deflationary reading, and we have touched upon some serious challenges to the logico-mathematical component of Carnap's meta-philosophy.

The next section is one of two Interludes included in this dissertation, the second being between chapters 3 and 4. The purpose of each interlude is to introduce terminology and concepts which are important for what follows, but tangential to our main line of argument. Thus their topics, while too broad to include in an existing chapter, are not of sufficient gravity to warrant an entire chapter of their own. This presentation was a happy compromise.

In the Logico-Mathematical Interlude I will present the relevant technical details of Carnap's program. Our attention will be primarily directed toward the definitions and theorems in *Logical Syntax*. This specificity is for two reasons: (i) Most of the past interpretive work that we will engage, both the criticisms of Carnap's program, and the Deflationary reading, treats *Logical Syntax*. The reason for this, and so (ii) is that while the details change, the main tenets of Carnap's meta-philosophy remained largely stable after about 1932.[26] The first interlude will thus lay the technical groundwork that will be presumed throughout the rest of the dissertation.

Chapter 2 will then address the circularity objections. I agree with Goldfarb and Ricketts insofar as they suggest Carnap's concerns regarding mathematics were not foundational *in the sense* that Gödel, Quine, *et al.* suppose. However, neither was it Carnap's intention to completely eschew traditional questions in the philosophy of mathematics. Rather, I will argue that Carnap's program is more subtle than either characterization, walking a fine line between traditional foundational concerns and a completely "logocentric" linguistic relativism. Carnap's aim was the formal explication of logico-mathematical concepts and theories for use in the Logic of Science. For this task one may assume whatever mathematical resources are required. However, from this vantage one can still provide insights into the nature of logico-mathematical concepts and their application in the sciences.

Chapter 3 will turn to the Deflationary reading in earnest. Special emphasis is placed upon the Deflationary reply to Gödel, since this is how Goldfarb and Ricketts motivate their interpretation. In this chapter I will offer a significant amount of textual evidence against their reading, and argue that it is at best an uncharitable way to look at Carnap's program. By extending the scope of the Principle of Tolerance

[26]See, e.g., Creath (1990), Ricketts (1996), and Carus (2007). That Carnap's views remained stable throughout much of his career might at first blush seem surprising, given the expansion of his program to include semantics around 1935, and the evolution of his analyses of probability and his "theory of theories". However, his attitude toward metaphysics, the stress on formalization and mathematical methods, and most importantly, his scientific outlook toward philosophy, did not change. Beaney (2004) argues that there is also significant continuity between Carnap's early notion of a rational reconstruction and his mature notion of explication. Throughout the dissertation I will flag areas where there is a significant divergence between *Logical Syntax* and later work, both in cases that may affect my argument and where it is historically or philosophically interesting.

to such an extent, the Deflationary reading leaves Carnap's program in an unstable position internally, and with no way to advocate for its adoption externally.

In this regard, we will examine a further circularity-like objection to Carnap's program that has received some recent attention.[27] Specifically, although we saw above how Carnap addresses the debate between intuitionism and classical mathematics, certainly an intuitionist would not find this treatment convincing. This is for the simple reason that in order to carry out a meta-logical investigation of the languages in question, we must presuppose concepts that the intuitionist takes to be illegitimate.[28] Notice that this objection is importantly different from what I have called the circularity objections. Each of those take issue with Carnap's purported mathematical conventionalism as a foundation for mathematics. This objection, on the other hand, questions the very possibility of a non-question begging Carnapian analysis. In essence it attacks the entire methodology of his meta-philosophical program. While on a Deflationary reading there is little to be said on Carnap's behalf—since Carnap's program involves only the forwarding of linguistic proposals, the two parties must simply agree to disagree—on my interpretation Carnap can marshal arguments in favour of his position. Finally in this chapter, we shall return to the question: Should we be Tolerant about Tolerance?

The Anti-Metaphysico-Philosophical Interlude will address Carnap's rejection of metaphysics in more detail. I will argue that Carnap's mature scheme for identifying and dissolving pseudo-questions and disputes as outlined in *ESO*, while much less formal than that presented in *Logical Syntax* and related works, is of a close kin with these earlier methods. The purpose of this section is primarily the introduction of terminology and concepts that will be utilized in the final chapter, and to summarize my interpretation of Carnap's meta-philosophical program as a scientific philosophy.

The final chapter will address the debate between Awodey and Hellman regarding the foundations of mathematics, with special emphasis placed upon Hellman's objections to category theory as a suitable foundation. My tack will be to extend Carnap's conception of philosophy as a scientific enterprise in response to Hellman's objections. Specifically, I will argue that, in this case at least, ontological questions

[27]See, e.g, Richardson (1994), Ricketts (2007), and Friedman (2009).

[28]Consider for example Carnap's statement of the L-Determinacy of all logical sentences of LI, Theorem 14.3. As we will see in the Logico-Mathematical Interlude, this theorem is equivalent to a truth definition for the language when restricted to its logico-mathematical portion. The statement of the theorem requires that we not restrict the interpretation of our quantifiers to a domain that includes only those properties definable in the language, but instead consider all (syntactical) properties whatever. Cf. *Logical Syntax*, §34*d*, which we discuss in more detail below.

can be seen as beside the point and that Carnap's not-quite-foundational attitude proves fruitful.

1.4.1 Two Final Caveats

As a warning to the reader—although this has likely already been surmised—note that I will lean heavily upon textual evidence to support my interpretative claims. Carnap's works and correspondence are voluminous, and there are a surprising range of positions attributed to Carnap in the literature, many of which are given only limited textual support. This is perhaps because Carnap was famously deferential to his interlocutors, always interpreting them in the most charitable light and taking pains to find common ground between his view and theirs. So it is often difficult to find Carnap asserting his positions categorically. There can be no doubt that an attitude of tolerance permeated almost all of his philosophical work. But a careful reading of many sources will allow us to extract a strongly supported, coherent, and philosophically interesting program from the points at which those sources overlap. This is a program I hope Carnap would recognize as approaching his actual philosophical orientation. At the least, this should explain the multitude of sometimes long quotations in what follows.

With that said, it is important to be clear that I do not plan to defend Carnap's meta-philosophical program as entirely successful. The specific set of meta-theoretical concepts he develops for the purpose of proposing and investigating linguistic frameworks, and the particular details of the analysis of foundational problems that goes along with them, fail for both technical and philosophical reasons. Still, I think that there is much which remains of value in Carnap's work. It is only misinterpretation that has caused this value to be largely overlooked for the last twenty years. Overall this dissertation argues that Carnap's analysis of logico-mathematical truth as conventional, and with this his call to bring to the philosophy of science and mathematics a more scientifically informed methodology, offers an insightful and fruitful perspective from which to approach contemporary philosophical issues.

Logico-Mathematical Interlude

Kronecker only made the mistake of declaring the transfinite mode of inference to be inadmissible. [...] *At the time, the whole of mathematics unanimously rejected his prohibitions and went on to the business of the day.*

David Hilbert ([1931]1998)

This section will elucidate the technical aspects of Carnap's meta-philosophical program relevant to the rest of our study. For reasons stated near the end of the previous chapter, this means that our focus will be concentrated upon *Logical Syntax*. The primary goal in this section is not to interpret, but merely to present Carnap's results and discuss their historical and philosophical context. For more comprehensive treatments of the oft-overlooked technical innovations and influence of Carnap's work, especially in *Logical Syntax*, I highly recommend Sarkar (1992), Awodey & Reck (2002*a*), and de Rouilhan (2009). I draw from these sources in parts of my discussion below.

One conclusion that we will establish in this section is related to Carnap's proof of the non-contradictoriness of LII.[1] Sarkar asserts that this proof, along with Carnap's proofs that the axiom of choice and the principle of complete induction are analytic in LII, "remain little more than formal exercises of somewhat dubious value." (p. 205). Presumably this is because the nature of the meta-language assumed in the proofs guarantees the results achieved—the meta-language itself includes these very principles. In the case of Carnap's proof of non-contradictoriness, it is straight-

[1] Carnap's "proofs" throughout *Logical Syntax* are no more than proof-sketches, sometimes extremely sketchy. Still, the main line of his argument is usually clear.

forwardly a meta-theoretical proof, and so has little epistemic value.[2] Regardless, I think Sarkar's remark seriously misrepresents the methodological role that these results play in the development of Carnap's arguments throughout the book. We will postpone discussion of the analyticity results until the next chapter, since understanding the use Carnap makes of them is essential to a correct interpretation of Carnap's program, and so for responding to the circularity objections. However, it will be conducive to our discussion here to observe that Carnap uses his proof of non-contradictoriness both to make an instructive point about the relationship between meta- and object-languages, and as a step along the way to his novel sketches of Gödel's incompleteness results.

A.1 *Logical Syntax*—Overview

Logical Syntax is divided into five Parts. In Parts I and III Carnap presents and explains the machinery of LI and LII respectively. Recall that LI is a rather conservative language including only a version of primitive-recursive arithmetic; while LII is a much more powerful typed language with the axiom of choice, expressive enough for most of classical mathematics including set theory. LI is distinguished by allowing only *restricted* quantifiers. Universality can be expressed by free variables, but there is no way to express unrestricted existential quantification. As was Carnap's aim, this restriction results in a language concordant with many of the tendencies of intuitionism or finitism. LI is a proper sub-language of LII, in the sense that all of the symbols and sentences of LI are also symbols and sentences of LII. Both languages include *Descriptive* along with *Logical* predicates and functors, with the former being those predicates and functors that are undefined, or defined with the help of another descriptive predicate or functor.

Both languages can be supplemented with P-Rules, of which more below. The *Formation Rules* define what counts as a sentence in each language, and are presented in a relatively standard way, but with detailed explanations. Carnap calls the rules of inference for a language *Transformation Rules.* Their most interesting feature is that Carnap does not limit his languages to definite rules of *Derivation* (*viz.*, inference rules with a finite number of premises), but includes also indefinite (*viz.*, transfinite) rules, which he calls rules of *Consequence.* The transformation rules

[2] Cf. Ricketts (2007, p. 211), who observes that "Carnap places no justificatory weight on the proof [of the non-contradictoriness of LII]." In a sense this is true enough, but Ricketts completely misses its methodological import for Carnap's program.

of each language also include detailed rules for the introduction of new logical and descriptive definitions, and for substitution, including substitution with arguments. Carnap notes (p. 97) that his is the first completely rigorous presentation of substitution with arguments. Besides presenting the languages, these Parts also contain many digressions for discussion of then-controversial or logically interesting topics: e.g., §16 on intuitionism, §35–36 deriving a version of the incompleteness results, §38 on the elimination of class expressions, and §38*a* on existence assumptions.

Part II treats LI in a more formal way, using a version of arithmetization to show how we can treat a portion of the "syntax-language" (*viz.*, the meta-language) of LI using LI itself. Carnap takes this to be one of the most novel and important parts of the book, for reasons we will discuss below. While the definitions here are sometimes ingenious, overall the actual constructions are a rather tedious affair, hampered by a somewhat byzantine notation. I should note that Carnap's terminology and notation throughout is most often non-standard. Whenever I use Carnap's terminology or notation for the first time, I either place it in parentheses, or, if I use it in the text, I give a more modern explanation or equivalent in parentheses or a footnote.

Part IV is titled "General Syntax", and is the most interesting and relevant for our purposes. It is here that Carnap provides the definitions required for making a distinction between L- and P-Rules, as well as definitions for important meta-logical concepts such as logical consequence, analyticity, etc. What is interesting about this Part is that Carnap attempts to offer very general definitions, such that they will be applicable to the meta-logical analysis of a wide variety of languages:

> In this section, we shall attempt to construct a *syntax for languages in general*, that is to say, a system of definitions of syntactical terms which are so comprehensive as to be applicable to any language whatsoever. (*Logical Syntax*, p. 167. Original emphasis.)[3]

In this regard, Carnap's languages LI and LII are not properly *examples* in *Logical Syntax*. Rather, they are model languages through which the syntactical method is demonstrated. In the first Parts of *Logical Syntax*, Carnap not only constructs LI and LII, but more importantly demonstrates how to investigate their syntax (i.e., prove meta-theorems). So one test of the adequacy of his developments in general

[3]After this remark he includes, in square-brackets: "We have, it is true, had chiefly in mind as examples languages similar in their principle features to the usual symbolic languages, and, in many cases, the choice of definitions has been influenced by this fact. Nevertheless, the terms defined are also applicable to languages of quite different kinds." Carnap was perhaps a little naïve as to the variety in possible language forms.

syntax are whether they can recover the theorems and definitions for LI and LII.

I should note that immediately after the above-quoted passage, Carnap observes that his work in Part IV is only a first attempt at such a general scheme. And it turns out that some of the definitions are indeed inadequate in certain respects, which we will flag below. This however does not ruin the philosophical worth of Carnap's program, since these distinctions and definitions can often be made on a case-by-case basis where necessary, and the main tenets of his wider meta-philosophy are largely unaffected.

Finally, Part V of the book deals specifically with the application of the "method of logical syntax" to philosophical debates, roughly as we outlined in our first chapter. The idea is to show, both in a general way and by use of examples, how to analyze and then translate sentences from the "material-mode of speech" into the "formal-mode of speech". Sentences of the material-mode, our colloquial language and the usual language used in the practice of science, are often ambiguous and so are apt to confuse. Once translated into the formal-mode, we can recognize that a sentence which may have seemed to be about an object in the world is actually a meta-level sentence about our language. Our first example from chapter 1 derives from this Part of the book. We will save further discussion of Part V for the second interlude.

A.2 Carnap's Motivations and Technical Goals

Logical Syntax has at least four main technical goals, determined and influenced by the state of logic and philosophy in the late 1920s and early 1930s. We offer a brief examination of each.

Attempt at a Meta-Logic

In the first place, recall from chapter 1 that Carnap was interested in the development of a set of meta-linguistic concepts and methods for the construction and investigation of linguistic frameworks with a wide variety of possible structures. This eventually came to be represented in Parts IV and V of the book. His "Intellectual Autobiography" reinforces our earlier discussion:

> The chief motivation for my development of the syntactical method, however, was the following. In our discussions in the Vienna Circle it had turned out that any attempt at formulating more precisely the philosophical problems in which we were interested ended up with problems

> of the logical analysis of language. Since in our view the issue in philosophical problems concerned the language, not the world, these problems should be formulated, not in the object language, but in the metalanguage. Therefore it seemed to me that the development of a suitable metalanguage would essentially contribute toward greater clarity in the formulation of philosophical problems and greater fruitfulness in their discussions. (Carnap, 1963, p. 55)

Carnap there also asserts that in the development of his "syntactical method" he was chiefly influenced by the meta-mathematical work of Hilbert, Tarski, and, of course, Gödel—especially the latter's then-recent discovery of arithmetization, that is, a method for correlating the individual symbols and formulae of a formalization of Peano Arithmetic with unique natural numbers. This is to assimilate the study of a formal language expressing arithmetic into a part of arithmetic itself. Keep in mind that all of this work was either brand new, or still ongoing, at the time of Carnap's "sleepless night" in January 1931, when the ideas that form the core of *Logical Syntax* came to him "like a vision" while in bed with a fever (Ibid., p. 53).[4]

The kernal of Carnap's vision, which he wrote down and titled *Attempt at a Metalogic*, was that Hilbert's meta-mathematics could—via Gödel's technique of arithmetization—be extended beyond its original application to proving the consistency of arithmetic. Instead, these ideas could be developed into a general theory which treats the formal structure of an entire language of any sort so long as that language could be cast in a suitably formal way. Indeed, within any given language, *limited only by the means of expression of that language*, the "syntax" (*viz.*, the syntactical portion of the meta-language) of any other language whatever, or even the meta-language of the language in question, can be expressed using Gödel's method. Carnap highlights this in Part II of *Logical Syntax*, which is an extended demonstration of the fact that the syntax of the *definite* (*viz.*, finite or effective) part of LI can be completely formalized within LI.[5] Carnap stresses that this can be accomplished without any contradictions arising—still a pressing worry at the time.

[4]Cf. Awodey & Carus (2006) for further details on this intriguing anecdote and an excellent discussion of Carnap's thought before and immediately after that fateful night.

[5]Carnap's choice of LI here is telling. LI is basically the weakest (and therefore least controversial) then-known language for which arithmetization is possible. And so likewise with more expressive languages. This observation derives from Oberdan (1992).

Proving Wittgenstein Wrong

However, Carnap's main philosophical target in Part II seems to be Wittgenstein, and so this brings us to the second primary goal of *Logical Syntax*. In the *Tractatus*, Wittgenstein follows Frege in assuming that there can be only one possible logic. For Frege, this is because the laws of logic are the laws of *truth*, "boundary stones set in an eternal foundation, which our thought can overflow but not dislodge." (Frege, [1893]1997, p. xvi). For Wittgenstein, this is because the very structure of language corresponds to the structure of the world, and so languages are bound to be structurally similar. Wittgenstein furthermore asserts that the logical structure of our language cannot itself be discussed using language.[6] Contrary to this line of thought, Carnap's rigorous construction of the syntax of LI in LI shows in quite substantial detail that this is indeed possible. Here is Carnap summarizing this point:

> In opposition to this view, our construction of syntax has shown that it can be correctly formulated and that syntactical sentences do exist. It is just as possible to construct sentences about the forms of linguistic expressions, and therefore about sentences, as it is to construct sentences about the geometrical forms of geometrical structures. (*Logical Syntax*, pp. 282–283)

This passage is interesting for several reasons. Not the least of which is that Carnap seems to be making a full-blooded philosophical *assertion* here: He is stating that Wittgenstein was *wrong*, since he (Carnap) has shown that we can indeed treat the logical form of a language, even using that very language itself. This speaks against a Deflationary reading which argues that Carnap could not have been making any philosophical assertions at all.

Logical Pluralism

But we are getting ahead of ourselves, and will return to this issue in chapter 3. For the moment we need only reinforce that it was Carnap's goal in Part IV of *Logical Syntax* to provide the first steps toward a *general* theory of logical syntax. In other words, to develop a set of concepts and methods that would allow for the construction of useful sentences about sentences in the service of bringing to philosophy a precise

[6] See especially his remarks in §4.1, e.g., "4.121: Propositions cannot represent logical form: it is mirrored in them. What finds reflection in language, language cannot represent. What expresses *itself* in language, *we* cannot express by means of language. Propositions *show* the logical form of reality. They display it." (Wittgenstein, [1922]1974, Original emphasis.)

methodology with the potential for making genuine progress. This, coupled with the realization that languages could be treated entirely formally, led Carnap to the Principle of Tolerance.

First, from his autobiography:

> I thought of the logical syntax of language in the strictly limited sense of dealing exclusively with the forms of the expressions of the language, the form of an expression being characterized by the specification of the signs occurring in it and of the order in which the signs occur. No reference to the meaning of the signs and expressions is made in logical syntax. (Carnap, 1963, p. 54)

As we have seen, in *Logical Syntax* Carnap considers languages only as *calculi*, that is, understood as systems of conventions or rules about symbols distributed into various classes that make up the language (*Logical Syntax*, p. 4). This treatment of language is possible only because a general theory of linguistic forms—i.e., meta-logic or logical syntax—is possible. Carnap is very clear that he does not mean to suggest that languages are *nothing more* than mere calculi. It is rather that investigations of logical syntax are only concerned with those formal aspects of a language treated as a calculus. If our desire is to introduce a scientific methodology to philosophy, then the method of logical syntax seems like a good candidate.

But with this formal perspective toward languages, there is little barrier to the consideration of other forms of language. After all, they are just other sets of rules and conventions that can be equally-well investigated via the methods of syntax. So Carnap arrives at his pluralism, as recounted in the Foreword of *Logical Syntax*:

> The fact that no attempts have been made to venture still further from the classical forms is perhaps due to the widely held opinion that any such deviation must be justified—that is, that the new language-form must be proved to be 'correct' and to constitute a faithful rendering of 'the true logic'.
>
> To eliminate this standpoint, together with the pseudo-questions and wearisome controversies which arise as a result of it, is one of the chief tasks of this book. In it, the view will be maintained that we have in every respect complete liberty with regard to the forms of language; that both the forms of construction for sentences and the rules of transformation

> (the latter are usually designated as "postulates" and "rules of inference") may be chosen quite arbitrarily. (*Logical Syntax*, pp. xiv–xv)

So what we will call Carnap's third goal in *Logical Syntax* is the forwarding of this logical pluralism. Notice that the pluralistic standpoint encapsulated in the Principle of Tolerance is engendered by the ability to fruitfully treat languages as calculi from a meta-theoretical perspective. This allows for the transformation of what seem to be assertions into questions of a choice between object-languages. The success of this scheme thus rests on the ability to treat languages completely formally, and so to recover or define the characteristic features of our informal mathematical theories in the domain of logical syntax.

Defining a Complete Criterion of Validity

This requirement dovetails nicely with what we will call Carnap's final primary goal in *Logical Syntax*, which is to develop a complete and formal criterion of validity for logico-mathematical sentences in the face of Gödel's incompleteness theorems. Recall that *Logical Syntax* was written in the wake of the incompleteness discoveries, devastating the foundational ambitions of both Hilbert's finitist program, and, to a certain extent, the logicism of Frege, Russell, and the early Carnap.[7] What Gödel's first theorem showed, in Carnap's terminology, is that any definite, non-contradictory language including a formalization of arithmetic will include irresoluble sentences. Gödel's second theorem, again in Carnap's terminology, tells us that for any definite, non-contradictory language including a formalization of arithmetic, the statement of the language's non-contradictoriness is irresoluble in that language.

Gödel's results were a blow to logicism and finitism, since both programs had attempted to construct a definite formal system from which to either: (i) Derive all the theorems of mathematics; or (ii) Prove the consistency of a formal axiomatization of all mathematics. To simplify the situation somewhat, the goal of both programs was to secure the higher, infinitary parts of mathematics (what Hilbert called "ideal mathematics") on the basis of a conservative set of principles whose consistency and self-evidence was indubitable. Gödel's first theorem of course states that there exists

[7]Cf. Carnap ([1931]1983) for an expression of his early (and very traditional) logicism. This paper was presented at a roundtable discussion on the philosophy of mathematics during the famous 1930 Königsberg Conference on the Epistemology of the Exact Sciences. The other contributers to the roundtable included Arend Heyting and John von Neumann. It was at this very conference—the day before the roundtable discussion—that Gödel first publicly announced his incompleteness theorems, September 6, 1930.

no such set of principles expressed as a formal system—i.e., there is no definite formal language within which to recover all of our informal mathematics at once. In the case of Hilbert's program, the second incompleteness result strikes the further blow of telling us that there can be no consistency proof of a system strong enough for all of mathematics within some weaker, finitary system.

Apparently, however, the gravity of these results was not immediately recognized. Carnap was one of the first people Gödel informed of his results, in private conversation at a Viennese café in August 1930. Still, Carnap continued to recommend a traditional logicist program as foundationally adequate in September of that year. Goldfarb (1995*a*) likewise surmises that there was "little reaction" at the Königsberg Conference to Gödel's announcement.

The full force of these results would come to be recognized soon enough, however. Recall that the Logical Empiricist solution to the problem of empiricism involved extending to mathematics Wittgenstein's insight that all logical sentences are tautologous, or free from empirical content. To give this thesis weight requires that we can draw a sharp distinction between the formal and the factual components of our knowledge. In the context of Carnap's program in *Logical Syntax*, this amounts to an explication of our informal notion of logico-mathematical truth as analytic, that is, to a complete characterization of the class of logico-mathematical truths. Gödel's theorems call into question the possibility of drawing this distinction completely—according to Gödel's results, certain canonically mathematical sentences may be left out by any formal criterion. Moreover, the character of those unaccounted-for sentences is then called into question: Are they synthetic or analytic? *A priori* or *a posteriori*? The matter is thus of vital importance for Carnap's program, and he discusses it at some length in §34*a* of *Logical Syntax*:

> One of the chief tasks of the logical foundations of mathematics is to set up a formal criterion of validity, that is, to state the necessary and sufficient conditions which a sentence must fulfil in order to be valid (correct, true) in the sense understood in classical mathematics. (p. 98)

Given that LII is constructed in such a way as it can express classical mathematics, Carnap equates this general problem with the specific one of "*setting up a formal criterion of validity for the sentences of Language II*." (Ibid. My emphasis.) In other words, the problem is to provide an explication of logico-mathematical truth by defining a complete criterion of validity for LII.

This can be attempted in at least three ways according to Carnap, but only one is adequate to the problem at hand. In the first place, we may try to discover a *definite criterion of validity*. This would be a finite decision-procedure for every sentence of classical mathematics, as with the truth-table method for the sentences of the propositional calculus. A result would be that every sentence of classical mathematics (formulated in LII) could be determined true or false in a finite number of mechanical steps. Carnap notes that Gödel's results make the discovery of a complete criterion of this kind a "hopeless endeavor".

The second method we might try is the *Method of Derivation*. This is an *indefinite* criterion of validity, but one which is based on definite rules (i.e., our usual rules of inference and primitive sentences). The reason the criterion is called indefinite is because while we can effectively check every derivation, there is no generally effective method for generating proofs or refutations of arbitrary theorems. Carnap observes that "all modern systems which attempt to create a logical foundation for mathematics (for example, the systems of Frege, Peano, Whitehead and Russell, Hilbert, and others)" (p. 99) have so-far used this method. Indeed, here is included the aforementioned foundational programs of logicism and finitism. Carnap asserts that despite this shortcoming, "the method retains its fundamental significance; for every strict proof of any sentence in any domain must, in the last resort, make use of it." (Ibid.) Presumably this is because we use derivations in the meta-language to prove syntactical theorems about some object-language.

Given that the method of derivation does not capture the desired extension, Carnap's idea is to give up the condition of *definiteness* not only for our criterion, but also for the individual steps of a deduction. Such a method of deduction, in which the class of premises may be infinite and the individual deductive steps may be indefinite, he calls the *Method of Consequence*, or the *C-Method*. This distinction between the D- and C-Methods obviously corresponds to the distinction in the transformation rules for LI and LII we introduced above. This is the essence of Carnap's solution to the problem:

> In this way a *complete criterion of validity for mathematics* is obtained. We shall define the term 'analytic' in such a way that it is applicable to all those sentences, and only to those sentences, of Language II that are valid (true, correct) on the basis of logic and classical mathematics. We shall define the term 'contradictory' in such a way that it applies to those sentences that are false in the logico-mathematical sense. We shall call $\mathfrak{S}_1$ *L-determinate* if it is either analytic or contradictory; otherwise we

shall call it *synthetic*. (*Logical Syntax*, p. 101)[8]

In other words, Carnap's definitions state the necessary and sufficient conditions for the truth of every logico-mathematical sentence in LII—i.e., he provides an adequate explication of logico-mathematical truth. In the context of his program, this is a vindication of the Logical Empiricist tenet that there is a sharp divide between the factual and formal aspects of our knowledge, in the sense that we can *recover* such a divide in a rational reconstruction of the language of science. We will examine these definitions in a bit more detail in the next sections.

A.3 Languages I and II

Given what has just been said, the most novel and interesting feature of Carnap's presentations of LI and LII are the development and isolation of their rules of *Consequence*. Indeed, Carnap's definition of 'Analytic' for LII (§36) amounts to one of the first complete definitions of truth for classical mathematics, while his definition of 'Direct Consequence' for LII is likewise for our traditional notion of logical consequence. Tarski ([1936]1983, p. 413) even goes so far as to credit Carnap with the first *attempt* at a "precise definition" of logical consequence. In fact, Carnap's "syntactical" definitions in §36 of *Logical Syntax* are in all essential respects equivalent to the logico-mathematical portions of Tarski's semantical notions of truth, satisfaction, and logical consequence, although much more convoluted.[9]

The distinction in transformation rules for LI and LII between *Rules of Derivation* and *Rules of Consequence* is a matter of the D-Rules being definite, while the C-Rules are indefinite. The use of the term 'rule' here is slightly non-standard, even in the case of the D-Rules. LII, for example, includes amongst its D-Rules *Modus Ponens* and Universal Instantiation, as well as the aforementioned rules of substitution with arguments. However, LII also contains 23 Primitive Sentences (some are schemata), including typical axioms for the propositional and predicate calculus with identity, axioms for arithmetic, the axiom of choice, the principle of complete induction, and axioms of extensionality. Primitive sentences are counted by Carnap as D-Rules. Needless to say, these are all L-Rules, in the sense observed in chapter 1 and further

[8]Note that Carnap uses gothic letters as meta-variables: '$\mathfrak{S}$' for sentence variables, '$\mathfrak{pr}$' for predicate variables, '$\mathfrak{A}$' for expressions of any sort, etc.

[9]Kleene's (1939) review of *Logical Syntax* develops a considerably simplified definition of analyticity, which Carnap (1940, Carnap's review of Kleene's review) accepts and recognizes as being completely analogous to Tarski's definition of truth.

discussed below. Although LI and LII are L-Languages (contain no P-Rules), P-Rules can be introduced as rules of inference or as primitive sentences.

Carnap defines corresponding sets of concepts associated with each kind of transformation rule:

D-Terms	**C-Terms**
Directly Derivable	Direct Consequence
Derivable	Consequence
Demonstrable	Analytic
Refutable	Contradictory
Resoluble	L-Determinate
Irresoluble	Synthetic

Table A.1: Derivation and Consequence Terms in LI and LII

A sentence S is *Directly Derivable* from a sentence S_1, in either LI or LII, if it can be obtained using the D-Rules of the respective language. A sentence S is then *Derivable* from a class of sentences $S_1, \ldots, S_n$ if it occurs on a finite list of sentences of which all the sentences before it are either: (i) one of the S_n; (ii) a definition-sentence; or (iii) directly derivable from the preceding sentences. A sentence S is *Demonstrable* if it is derivable from the null-class of premises. In LI, a sentence is *Refutable* if any sentence of the form $\sim S_1$ is derivable, where S_1 is obtainable from S by substitution of a numerical expression for a free variable (if the sentence is closed, S is *Refutable* if $\sim S$ is demonstrable). LII includes variables of higher types to take into account, but the definition of *Refutable* otherwise corresponds. Finally, a sentence is *Resoluble* if either demonstrable or refutable, *Irresoluble* otherwise. The C-Terms do not correspond exactly to these definitions, but we will discuss them further below. In each case except the last, the D-Terms obviously determine a narrower extension of sentences than the corresponding C-Term.

A.3.1 Consequence for LI

To define the notion of *Consequence* and the resultant C-Terms for LI, Carnap takes a somewhat different tack than in LII. Rather than introducing a definition of *Analytic* and basing the definition of consequence upon it, he instead introduces a transfinite rule of inference. This rule has come to be known as the ω-rule (DC2 in §14 of *Logical Syntax*). The class of analytic sentences for LI is then those sentences that follow from the null-class of sentences in LI using either the D-Rules or the ω-rule.

The motivation for this rule in LI arises directly from the language's incompleteness. As Carnap explains, there may be a predicate in LI for which every sentence of the form $P(n)$ is demonstrable (where n is a numerical expression), but not the corresponding universal sentence $(x)\ P(x)$. But since this sentence can be expressed in the language, it may then end up irresoluble in LI. We introduce the ω-rule explicitly to allow this inference and overcome the incompleteness of the language. Where the notation '$[{}^{n}\!/_{x}]$' is the substitution of a numerical expression n for the variable x, we have in symbols:

$$\textbf{DC2 (}\omega\text{-rule):}\ \{P([{}^{1}\!/_{x}]), \ldots, P([{}^{n}\!/_{x}]), \ldots\} \vdash (x)\ P(x)$$

Since the term *Consequence* will thus be wider in scope than the corresponding term *Derivable* (and likewise *Analytic* is wider than *Demonstrable*, *Contradictory* than *Refutable*), while not demonstrable, our consequent universal sentence will now be analytic in LI.

Here are the requisite definitions. A sentence S is a *Direct Consequence* of a (not-necessarily finite) class of sentences K, if: (i) S is derivable from the class K; or (ii) S follows from K by DC2. Correspondingly for classes of sentences. A sentence is then a *Consequence* of some class of sentences, or a class of classes of sentences, if it is at the end of a chain of direct consequences. There are a few more intricacies, but that is the basic idea. Now, a sentence S is *Analytic* if it is a consequence of the null-class of sentences; *Contradictory* if every sentence is a consequence of S. A sentence is *L-Determinate* if it is either analytic or contradictory, and *Synthetic* otherwise. Carnap emphasizes that synthetic sentences "are the *genuine statements about reality*." (p. 41. Original emphasis) We will return to this point in chapter 3.

Of special importance is Carnap's Theorem 14.3, which, along with the corresponding Theorem 34*e*.11 (LII) and Theorem 52.3 (General Syntax), might be called fundamental theorems of *Logical Syntax*.[10] The theorems state that all of the Logical sentences of their respective language are L-Determinate, that is, either analytic or contradictory. Establishing these theorems is in each case essential, not only because they are the formal statements that the languages are indeed complete, but also because they act to demonstrate that the languages act as successful formal corollaries to the informal mathematical theories that they are attempting to reconstruct. In other words, these theorems secure the idea that the logico-mathematical sentences

[10]This term was suggested to me by the discussion in Demopoulos (2011), which provides a very insightful analysis of LI, although toward somewhat different ends.

of a language can be distinguished as following purely from the syntactical rules of the framework, and so can act as formal auxiliaries. This is in contrast to the synthetic sentences which, as Carnap emphasizes, are about the actual world. We will have more to say about this in chapters 2 and 3.

A.3.2 Consequence for LII

Carnap's definition of *Consequence* for LII is more typical than the definition for LI. It is based upon his definition of *Analytic* in LII, which, as noted above, is roughly equivalent in form to Tarski's later semantic definition of truth when restricted to logico-mathematical sentences. The complexities of Carnap's definition are beyond the scope of our discussion. It involves taking a sentence and subjecting it first to the rules of *Reduction*, which puts the sentence into prenex normal form. Rules of *Valuation* and *Evaluation* then correspond to Tarksi's notion of satisfaction and the recursive definitions necessary to determine the truth-conditions of a given sentence. The difference is that rather than assigning classes of objects as the interpretation of a predicate, Carnap assigns classes of expressions as possible valuations of a predicate.

In 1932 during the writing of *Logical Syntax*, Gödel in correspondence raises a problem with Carnap's procedure here.[11] He advises Carnap that using expressions of the language as valuations cannot work once we move on to higher-level predicates. Consider the open sentence '$M(F)$'. Its domain of discourse will be "all properties". A simple cardinality argument shows that the substitutional process used in the valuation of predicates of first-level will not secure a complete evaluation of this sentence, since there are more properties than there are predicate-expressions in the language to represent them. Carnap discusses this in §34*c* of *Logical Syntax*, concluding that we must

> follow Gödel's suggestions and define 'analytic' in such a way that '$M(F)$' is only called analytic if M holds for every numerical property irrespective of the limited domain of definitions which are possible in II" (p. 107)

This concession is striking, if only for the reason that Carnap is clearly relying upon *semantic* rather than syntactic ideas here.

But there is also the question of what such reference to arbitrary properties entails for Carnap's metaphysical views. He offers a short argument addressing this at the

[11] Part of this correspondence has been published in vol. IV of Gödel's *Collected Works* (Feferman, 1986–2005), along with an *Introductory Note* by Goldfarb (2003) which explains the issue in some detail. Cf. Goldfarb (2005).

end of the section:

> Thus the definition must not be limited to syntactical properties which are definable in [the meta-language] but must refer to all syntactical properties whatsoever. But do we not by this means arrive at a Platonic absolutism of ideas, that is, at the conception that the totality of all properties, which is non-denumerable and therefore can never be exhausted by definitions, is something which subsists in itself, independent of all construction and definition? From our point of view, this metaphysical conception [...] is definitely excluded. We have here absolutely nothing to do with the metaphysical question as to whether properties exist in themselves or whether they are created by definition. The question must rather be put as follows: can the phrase "for all properties" [...] be formulated in the symbolic syntax-language [...]? This question may be answered in the affirmative. The formulation is effected by the help of a universal operator with a [predicate variable], i.e., by means of '$(F)(\dots)$', for example. (p. 114)

Carnap observes that this argument depends upon the fact that the notion *Analytic* in the meta-language can be formulated in a meta-meta-language, and so at each level "the meaning intended is formally established" for the language in question by means of its meta-language. Whether or not this scheme is coherent in the context of Carnap's meta-philosophy is at the crux of the circularity objections, and so we will return to this issue in the next chapter. Observe that from a purely technical standpoint however, Carnap's definition is no more or less controversial than Tarski's.

Significance of Non-Contradictoriness for LII

With a definition of *Analytic* for LII secure, Carnap goes on to prove a number of meta-theorems, including the non-contradictoriness of LII. Recall from the beginning of this interlude that given the meta-linguistic nature of this proof, it has remained something of a puzzle as to why Carnap would bother to include it in the book, certainly it provides no reassurances or justification for LII. However, looking to the logico-historical context of *Logical Syntax* and to the form of the proof itself, we can discover its purpose.

As should be evident given our discussion thus far, Carnap is very clear about the technical situation here. In fact he highlights his proof's epistemic limitations:

> The proof which we have just given of the non-contradictoriness of Language II, in which classical mathematics is included, by no means represents a solution to Hilbert's problem. Our proof is essentially dependent upon the use of such syntactical terms as 'analytic', which are indefinite to a high degree, and which, in addition, go beyond the resources at the disposal of Language II. Hence, the significance of the presented proof of non-contradictoriness must not be over-estimated. Even if it contains no formal errors, it gives us no absolute certainty that contradictions in the object-language II cannot arise. For, since the proof is carried out in a syntax-language which has richer resources than Language II, we are in no wise guaranteed against the appearance of contradictions in this syntax-language, and thus in our proof. (*Logical Syntax*, p. 129)

So the proof is certainly not being used as some form of justification for LII, as evidence that classical mathematics is just as secure as intuitionistic mathematics, or something else of this sort.

But what then, is the point of the proof? As noted, the form of Carnap's proof here is telling. He begins by showing that every one of his Primitive Sentences in LII is analytic. He then generalizes this to show, as Theorem 34*i*.21, that every demonstrable sentence in LII is thus analytic. In other words, he shows that the derivability relation can be mapped into the consequence relation in LII. Or again in a horrible abuse of modern notation:

$$\text{For all sentences of LII } P,\ \vdash_{LII} P \text{ only if } \models_{LII} P$$

where we take '$\vdash_{LII}$' as the derivability relation in LII, and '$\models_{LII}$' as the consequence relation in LII. Next he shows that there is a non-demonstrable sentence in LII (Theorem 34*i*.23), by using the definition of analytic to show that LII has at least one analytically false sentence (*viz.*, '$0 \neq 0$'). And since there is at least one sentence not demonstrable in LII (i.e., the language does not explode), the language is non-contradictory (Theorem 34*i*.24).

The reason Carnap sketches non-contradictoriness in this way is so that he can utilize these results in his demonstration of the incompleteness theorems. Notice first of all that Carnap's proof here involves the D-Terms rather than the C-Terms. In *Logical Syntax*, Carnap makes a distinction between a language's being *Non-Contradictory*, and a language's being *Consistent*. He also sometimes uses the term *D-Consistent* for the former. In essence, a language is *Contradictory* if every sentence is demonstrable. It is *Inconsistent* if every sentence is *Valid*.[12] Basically Carnap's distinction here maps onto the standard distinction between consistency and ω-consistency.

We move now to Carnap's demonstration of the incompleteness of LII. In §35 Carnap uses arithmetization to give us a recipe for constructing, from any meta-linguistic predicate of LII (e.g., 'non-demonstrable'), a self-reflexive sentence that ascribes (truly or falsely) that predicate to itself. In symbols:

$$\vdash_{LII} \; S \Leftrightarrow P(\ulcorner S \urcorner)$$

This recipe, once it was rigorized, has come to be known as the Fixed-Point Lemma or the Diagonalization Lemma.[13] In any case, Carnap by this method constructs a so-called "Gödel Sentence", $\mathfrak{G}$, which we can gloss as a purely syntactical sentence of LII "asserting" of itself that it is not demonstrable in LII. He goes on in §36 to show that if LII is non-contradictory, then $\mathfrak{G}$ is not demonstrable in LII. Thus, by *modus ponens* and the non-contradictoriness of LII, $\mathfrak{G}$ is not demonstrable (Theorem 36.2). To show that $\mathfrak{G}$ is not refutable, he relies upon Theorem 34*i*.21 and a short *reductio*. Basically, if $\mathfrak{G}$ were refutable, then by Theorem 34*i*.21, $\sim\mathfrak{G}$ would be demonstrable, and so analytic. But this means that a proof with a certain Gödel-number exists, contradicting Theorem 36.2. And so $\mathfrak{G}$ is not refutable. Thus, Theorem 36.4 concludes that $\mathfrak{G}$ is irresoluble (the first incompleteness theorem); but we can also show that $\mathfrak{G}$ is analytic (Theorem 36.5). The second incompleteness result follows in the usual way (Theorem 36.7).

The point here is that Carnap's meta-proof of the non-contradictoriness of LII does indeed serve important functions in *Logical Syntax*. It both very carefully highlights the then-current situation in logic and mathematics regarding the relationship between object- and meta-languages, as well as the various notions of consistency and their relation to incompleteness. It also acts as a step in the much more important sketches of the incompleteness theorems. Carnap actually goes on to give another,

[12]'Valid' rather than 'analytic' here because these definitions are given in *General Syntax* and so take account of languages that might have P-Rules. In the case of an L-Language, it is *Inconsistent* if every sentence is analytic.

[13]See, e.g., the classic presentations of the incompleteness theorems in Mendelson (1987) and Boolos *et al.* (2002). Carnap is mentioned as having priority in both sources. From the fixed-point lemma follows not only the incompleteness results, but also Tarski's undefinability theorem. Carnap basically arrives at this result in *Logical Syntax*, as Theorem 60*c*.1. However, it is limited to analyticity rather than showing the undefinability of a full truth predicate in its own language. Also note Coffa (1987) for a discussion of *Logical Syntax*'s treatment of truth predicates and disquotation. The constructions in *Logical Syntax* come remarkably close to Tarski's T-schema. Coffa also speculates as to why Carnap may have failed to see the final steps in 1934.

more general, sketch of the incompleteness theorems later in the book (§60), using the semantic antinomies. And so his treatment of Gödel's theorems is really quite subtle and comprehensive.

A.4 General Syntax

As I have stressed above, Carnap's aim in *General Syntax* is the construction of a system of syntactical terms and methods which will be applicable to the investigation of a very wide range of linguistic frameworks. This system of concepts is meant to serve both to transform philosophical disputes into questions of language choice, and to provide a formal framework useful for the explication and meta-theoretical investigation of scientific theories and concepts.

Carnap takes the notion of *Consequence* as fundamental in this task:

> In the treatment of Language I and II we introduced the term 'consequence' only at a late stage. *From the systematic standpoint, however, it is the beginning of all syntax. If for any language the term 'consequence' is established, then everything that is to be said concerning the logical connections within this language is thereby determined.* (*Logical Syntax*, p. 168. Original emphasis.)

The ideal is that with a completely sufficient theory of general syntax, we should be able to take any arbitrary consequence relation (in a certain standard form) and be able to use the tools of logical syntax to give a complete meta-logical analysis of the language—that is, distinguish all of its logical and descriptive expressions, its L- and P-Rules, determine if the language is consistent, incomplete, whether it contains arithmetic, etc. This is certainly very ambitious given how widely Carnap wants to cast his net here (*any* form of language), and perhaps naïve in retrospect given the diversity of logical languages that now exist, but again, Carnap stresses that this is a first attempt.

In any case, he proceeds as if languages will be specified by their consequence relation, where 'consequence relation' here means *Direct Consequence*, which in general syntax consists always of two rules: (i) a conjunction of the *Formation Rules* for the language, defining all of its expressions; and (ii) a conjunction of the *Transformation Rules* for the language. The fundamentals of many languages (e.g., almost any language Carnap would have been familiar with) can be translated in such a way that they conform to this definition.

Next we get the usual distinction between D- and C-Rules, and a corresponding distinction in Terms. These are made as expected, depending upon whether the rule is definite or indefinite. He introduces some new terminology that is meant to be more general, since we will now have to account for both L- and P-Rules. The terminology is summarized in the table below.

D-Terms—LII	*General Syntax*	*General Syntax*	**LII—C-Terms**
Directly Derivable	Directly Derivable	Direct Consequence	Direct Consequence
Derivable	Derivable	Consequence	Consequence
Demonstrable	D-Valid	Valid	Analytic
Refutable	D-Contravalid	Contravalid	Contradictory
Resoluble	D-Determinate	Determinate	L-Determinate
Irresoluble	D-Indeterminate	Indeterminate	Synthetic

Table A.2: Derivation and Consequence Terms in *General Syntax*

Carnap is mostly interested in the C-Terms throughout general syntax. We say ‘Valid’ rather than ‘Analytic’ since we can have sentences which follow from the syntactical rules of the framework but are not canonically logico-mathematical, i.e., anything that follows with the help of a P-Rule. Thus the C-Terms in general syntax are wider than their corresponding C-Terms in LII (excepting the term ‘Indeterminate’). In LI and LII ‘Valid’ coincides with ‘Analytic’.

A.4.1 Distinguishing Between L- and P-Rules

Logical and Descriptive Vocabulary

The formal distinction between the L- and P-Rules rests upon a distinction in vocabulary. Carnap observes in §50 of *Logical Syntax* that if the *material interpretation* for a language is given, then we can usually divide its symbols, expressions, and sentences into those that have a purely logical meaning, and those that designate something extra-logical (*viz.*, empirical). At worst we can do this by stipulation, as with LI and LII. “Material interpretation” here means our usual, informal, colloquial language—the language most often used in practice in scientific investigation (including mathematics). Carnap’s reliance upon the “material” or standard interpretation of his formal languages as an elucidatory device will be of some importance in the next chapter.

For the moment what is at issue is the formal definition of this distinction between *Logical* and *Descriptive* symbols and expressions. Carnap reflects on this issue and suggests we find that

> all the connections between logico-mathematical terms are independent of extra-linguistic factors, such as, for instance, empirical observations, and that they must be solely and completely determined by the transformation rules of the language, we find *the formally expressible distinguishing peculiarity of the logical symbols and expressions to consist in the fact that each sentence constructed from them is determinate.* (*Logical Syntax*, p. 177. My emphasis.)

So, again, the problem is to provide an explication of an informal notion, in this case of the above characterization of a logico-mathematical expression.

The definition Carnap formulates has significant defects.[14] As written, it simply fails to do what Carnap intends. Rather than slog through the technical details, which in any case do not establish Carnap's desired results, here is the basic idea:

> **Logical Vocabulary:** The logical vocabulary of a language is the smallest non-empty class of basic expressions such that every sentence containing just those expressions is determinate (*viz.*, either valid or contravalid). All other vocabulary is descriptive.

In other words, the goal is to isolate the smallest set of expressions which lie at the intersection of all the sentences that follow purely from the syntactical rules of the language. These expressions will be in a sense implicitly defined by the language, since their meaning is completely determined by the language's inferential relationships without any external influence.

The main problem with a formal definition along these lines is that it proves surprisingly difficult to isolate all and only the customarily logical vocabulary. Mac Lane (1938) provides a counterexample using no more than the vocabulary of LI, in which the numerical expressions end up descriptive according to Carnap's definition (actually, a corrected version of the definition given in *Logical Syntax*, which is not only substantially but also trivially flawed). Therefore, we cannot take general syntax as entirely successful, since its tools do not even correctly recover all of the syntactical features of LI and LII.

However, this failure of Carnap's explication does not discourage the entirety of general syntax. As noted above, we can in most cases make the distinction for

[14]See Mac Lane's (1938) review for details. His brief article offers an extremely penetrating analysis of the book and many suggestions for reformulations and simplifications. Cf. Creath (1996) for another criticism of Carnap's definition. Dennis Bonnay (2009) has recently proposed a reformulation which aims to overcome these difficulties. Although I do not have the space to elaborate here, I think this reformulation encounters significant hurdles.

any given language of interest. This limits the scope of general syntax, since we do not have the means for a complete analysis of arbitrary languages, but the concepts developed need-not rely upon the definitions in §50. Again, we will return to this problem in chapter 2.

L- and P-Rules

We come at last to Carnap's distinction between L- and P-Rules in §51. Much like the distinction between logical and descriptive vocabulary, the first step is to make clear what we will be explicating (*viz.*, the *explicandum*). The basic idea is that the L-Rules are to be purely logico-mathematical, while the P-Rules will encode laws of nature, or hypotheses—in other words, empirical sentences.[15] It would be a mistake to think that this distinction maps cleanly onto the distinction between logical and descriptive expressions however, since L-Rules may sometimes include descriptive vocabulary. Consider:

$$Q(3) \supset \big(\sim Q(3) \supset Q(5)\big)$$

where '$Q(n)$' is a descriptive predicate of LI. Carnap observes that even though this sentence is descriptive, it is "obviously true in a purely logical way" (p. 181), since it is a typical axiom of the propositional calculus (PSI-1 in LI).

Why then should we consider this sentence true in a purely logical way? Carnap's answer is because substitution of the descriptive predicate '$Q(n)$' with any other predicate of LI maintains the sentence's status as an instance of PSI-1. Thus, Carnap concludes that we can take the general replaceability of descriptive expressions in a sentence as the "definitive characteristic" of L-Rules as distinct from P-Rules.

The formal definition of this distinction is too involved to present in full. Carnap splits it into cases of the consequence relation for the language according to whether there are descriptive predicates involved or not. In the case of a sentence S following from a class of sentences K where all the sentences are logical, S is automatically an L-Consequence of K. In cases where there are descriptive expressions in play, truth under all substitutions is required for S to be an L-Consequence, otherwise it is a P-Consequence. What is more important than this formal definition is Carnap's overall method here. Notice that Carnap takes it as a *desideratum* of the distinction that it adequately capture our informal or unreconstructed notion of a logical sentence or

[15] Carnap notes that there is no principled restriction against taking even individual observation sentences—*viz.*, protocol sentences—as P-Rules. However, we may then "frequently be placed in the position of having to alter the language" (*Logical Syntax*, p. 180).

rule. This is why Carnap chooses the example examined directly above. A distinction between L- and P-Rules that fails to construe this sentence as an L-Rule (should this sentence be expressible in the language under consideration) has simply failed as an adequate explication of the informal distinction Carnap is trying to capture, since we typically consider sentences of this form to be logically rather than empirically true due to the substitutability criterion.

The distinction between L- and P-Rules gives us a final distinction in the Terms of general syntax. The C-Terms in the table above are divided into L- and P-Terms. The expressions 'L-Valid', 'L-Contravalid', and 'L-Indeterminate' are replaced with their expected names: 'Analytic', 'Contradictory' and 'Synthetic', respectively. Theorem 52.3 tells us that every logical sentence is L-Determinate. Again, this is an essential theorem, in this case because it requires that all of the logico-mathematical sentences of a language follow purely from the syntactical L-Rules, such that they can act as formal auxiliaries. Carnap remarks: "*there are no synthetic logical sentences.*" (p. 184). This is an important condition of adequacy upon his explication of *Analytic Sentence* in general syntax.

On pp. 185 and 210 of *Logical Syntax* Carnap offers a set of diagrams that greatly clarify the divisions in the various sorts of languages he has considered. I reproduce them here so that they may be referred to in the sequel.

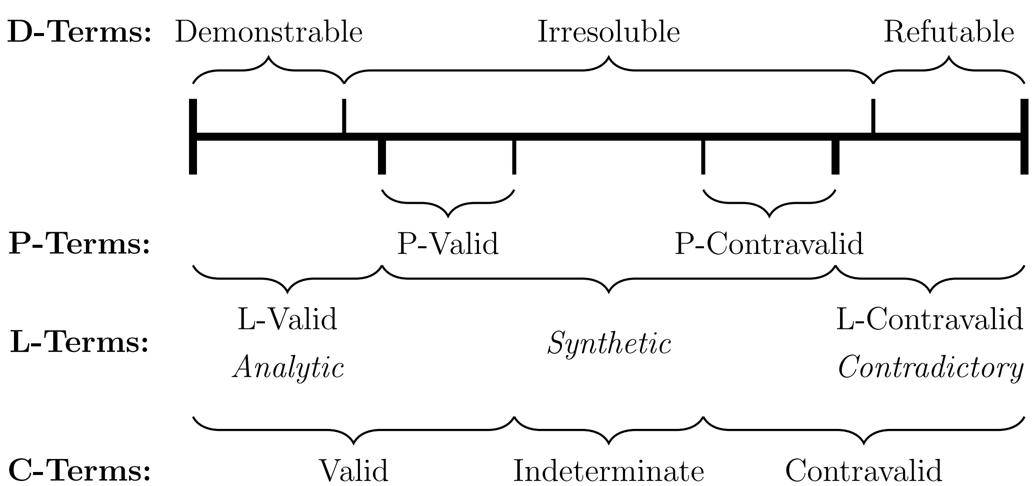

Figure A.1: Classification of Descriptive Sentences for Languages with P-Rules (recall that L-Rules may contain descriptive expressions)

As can be seen in the figure, the inclusion of P-Rules means that there will be Valid sentences that are nevertheless Synthetic. Such sentences follow from the rules of a linguistic framework despite being empirical, rather than logical. It is important to reinforce that Carnap has deliberately set-up the definitions of *General Syntax* in this way, allowing him to represent more detailed epistemic distinctions than a simple logical/empirical divide. Instead, well-established laws of nature can be represented as constitutive of our framework while we can at the same time acknowledge their empirical nature. I will argue in the next two chapters that capturing such epistemic characteristics is an important constraint upon Carnap's reconstructive program.

In the case of a language without P-Rules, recall that the L- and C-Terms coincide. Thus the classification of the descriptive sentences is much simpler:

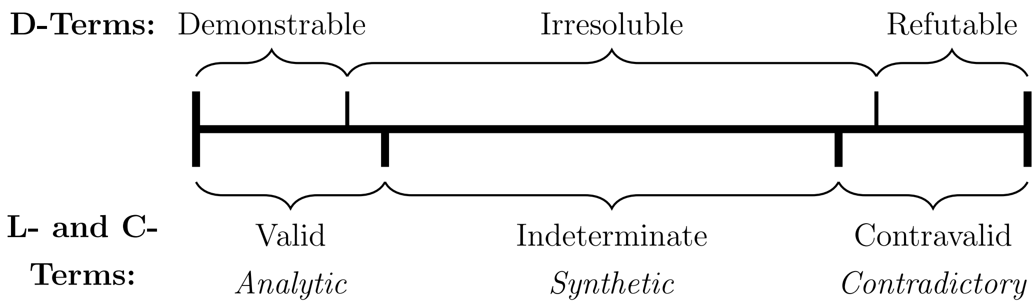

Figure A.2: Classification of Descriptive Sentences for Languages without P-Rules

For example, this figure is a representation of the descriptive sentences of LI and LII, since neither includes P-Rules (of course either can be extended with P-Rules).

Finally, we can compliment Figure A.2 with a classification of the *logical* sentences of an irresoluble but complete language.

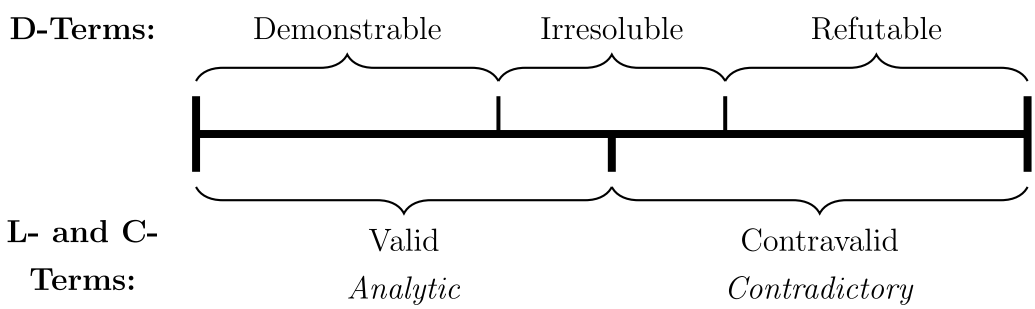

Figure A.3: Classification of Logical Sentences for Irresoluble but Complete Languages

Again, the figure is a representation of LI and LII, as well as any similar languages. Note that the figure graphically represents Carnap's solution to the problem of specifying a complete criterion of validity in the face of the incompleteness theorems. The lack of indeterminate sentences, thanks to an adequate definition of C-Terms, is contrasted with the set of irresoluble sentences determined by the D-Terms.

Chapter 2

Is Carnap's Meta-Philosophy Viciously Circular?

> *Definitions show their worth by proving fruitful. [. . .] Let us try, therefore, whether we can derive from our definition of the Number which belongs to the concept F any of the well-known properties of numbers.*
>
> GOTTLOB FREGE ([1884]1980, §70)

Our primary concern in this chapter will be the treatment of the *circularity objections* levied against the logico-mathematical component of Carnap's meta-philosophy, or what we have been calling Carnap's philosophy of mathematics. As noted in chapter 1, the kernal of each of these objections is the same: Carnap's program is charged with harboring a vicious circularity on account of the mathematical resources necessarily presupposed at the meta-level in order to carry through an object-level reconstruction of mathematics. This technical situation is then taken to undermine the supposed Carnapian claim that our knowledge of logic and mathematics can be accounted for in a purely formal way, free from any appeal to intuition, experience, etc.

Some of the renewed interest in Logical Empiricism has focused upon the re-evaluation of criticisms of this sort—such as those of Quine ([1935]1976; 1963) and Beth (1963). Or in the development of new criticisms along these same lines, as found in Potter (2000, chp. 11). However, their collective structural similarity has not to my knowledge been previously highlighted.

Of special interest for our purposes is Gödel's ([1953/9]1995) variation on this form of objection and his related criticism regarding the Logical Empiricist notion of 'content'. These objections derive from a series of drafts of a paper found in Gödel's *Nachlass* entitled "Is Mathematics Syntax of Language?" The paper was destined to be Gödel's contribution to Schilpp (1963), Carnap's *Library of Living Philosophers* volume. After numerous delays from 1953–1959 Gödel remained unhappy with the positive arguments in the paper, and so in the end it was not included. Two drafts of the paper have since been published in Volume III of Gödel's *Collected Works* (Feferman, 1986–2005), along with an *Introductory Note* by Goldfarb (1995*b*).[1] The publication of these drafts has spawned a veritable cottage industry of commentary and interpretation.[2] This is not without good reason, since at first blush these criticisms, along with the other circularity objections, can appear to mount a formidable challenge to Carnap's philosophy of mathematics.

We will begin with a detailed examination of Gödel's objections, including discussion of Gödel's own interpretation of Carnap's program. Goldfarb and Ricketts most often present their reading in direct contrast to Gödel's, and so an extended analysis of the latter's interpretation will help us come to grips with the formers'. Interestingly, our own interpretation of Carnap is much closer to Gödel's take than it is to the Deflationary reading.

This discussion will serve as background to §2, where we will investigate in greater detail other prominent circularity objections, from the authors mentioned above. We will see that these criticisms share a structure and point of criticism that allows a subtle reading of Carnap's program to reply *en masse*.

This possible Carnapian reply will be developed in §3, where we will focus especially upon Carnap's notion of an *explication*, or rational reconstruction. As was identified in chapter 1, understanding that Carnap's project is in the main the *reconstruction* of the language of science requires that our linguistic frameworks adequately capture key features of the scientific concepts and theories they formalize. This situation both engenders the circularity objections by requiring that Carnap's methods be meta-theoretical, and helps to respond to them by providing evidence that Carnap's program was not a foundational one in the usual sense. We will conclude in §4 that

[1]Goldfarb's *Introductory Note* provides further details on the origins and revision-history of the various drafts and their relations to each other. He also offers an iteration of the Deflationary response reviewed in our chapter 1 and evaluated in chapter 3 below, and an insightful analysis of Gödel's positive remarks regarding his mathematical realism.

[2]See, e.g., Goldfarb & Ricketts (1992) and the other papers of their series we have cited. Cf. Richardson (1994), Friedman (1999), Potter (2000), and Awodey & Carus (2004).

since Carnap is providing an explication of logico-mathematical concepts and not a foundation for them, it is not circular for him to appeal to whatever informal mathematical notions required in his meta-theoretical investigations. Gödel's particular circularity objection suffers from the further defect of being *unsound*, as was first observed by Awodey & Carus (2004). However, this fact does not impede our study of his objections from serving as a useful lens through which to better understand both Carnap's program and its Deflationary interpretation.

2.1 Gödel's Criticisms

Gödel attributes to the Logical Empiricists a position as to the nature of logic and mathematics "which can be characterized as being a combination of nominalism and conventionalism." (Gödel, [1953/9]1995, p. 334) We should observe immediately that even before the publication of *Logical Syntax*, Carnap would have rejected the label 'nominalism' as a characterization of his views. As we reviewed in chapter 1, one of the primary tenets of Carnap's meta-philosophy is the transformation of questions regarding the ontological status of various entities into questions that are either addressed by the linguistic choices we have made, or into questions about said choices to be investigated by meta-theoretical means. Nominalism about mathematical entities is therefore as much a pseudo-thesis as platonism (i.e., "realism") is for Carnap.[3]

In any case, Gödel soon elaborates upon his description:

> According to this conception (which, in the sequel, I shall call the syntactic viewpoint) mathematics can be completely reduced to (and in fact *is* nothing but) syntax of language. I.e., the validity of mathematical theorems consists solely in their being consequences of certain syntactical conventions about the use of symbols, not in their describing states of affairs in some realm of things. Or, as Carnap puts it: *Mathematics is a system of auxiliary sentences without content or object.* (Gödel, [1953/9]1995, p. 335. Original emphasis.)[4]

[3]Note that despite the terminology, the dispute between realists and nominalists should not be confused with the controversy between realists and idealists used as an example in chapter 1. The former more specifically addresses our ontological commitments, and can be directed separately toward any number of domains (e.g., mathematical entities, properties, propositions, etc.). The latter is a more general metaphysical dispute about the nature of reality. Carnap is most explicit about his rejection of the theses of realism *and* nominalism as regards mathematical entities in *ESO* and Carnap (1963, p. 871). For an earlier formulation of these same ideas, see Carnap ([1932]1960).

[4]In this quotation I have omitted four footnotes which support or qualify Gödel's characterization

The "syntactic viewpoint" is Gödel's characterization of Carnap's philosophy of mathematics, which he takes as comprising two primary theses: (i) Mathematics is nothing other than the syntax of language; and (ii) Mathematical statements have no content (Ibid., p. 337). Gödel's aim is to argue against each, to the conclusion that an element of mathematical intuition, somehow able to grasp an objective set of mathematical objects and truths, is shown to be inevitable for explaining our knowledge of mathematics. In other words, Gödel argues that merely pointing to a set of linguistic stipulations in the form of the syntactical rules constituting the logico-mathematical component of our framework is insufficient as an account of our knowledge of mathematics; and thus that mathematical propositions are not merely formal, but must instead be taken as having their own sort of (non-empirical) content.

Before rehearsing Gödel's arguments, it is worth reinforcing that his primary concern is not necessarily Carnap's distinction between analyticity and empirical truth, as long as analytic sentences are construed as what Gödel would call "conceptual truths". Indeed, he takes the clarification of this distinction to be one of the Logical Empiricists' key contributions:

> The syntactical point of view as to the nature of mathematics doubtless has the merit of having pointed out the fundamental difference between mathematical and empirical truth. This difference, I think rightly, is placed in the fact that mathematical propositions, as opposed to empirical ones, are true in virtue of the *concepts* occurring in them. (Gödel, [1953/9]1995, pp. 356–357. Original emphasis.)

Gödel instead takes issue with the construal of analytic sentences as purely formal, dependent only upon our choice of syntactical rules, along with the Logical Empiricist denial that the conceptual realm constitutes an objective domain of investigation in its own right, with objects and truths independent of our knowledge or understanding. Mathematics is the exemplar of such a domain for Gödel.[5] Continuing with the quotation above:

> However, by adopting the nominalistic point of view and identifying concepts with symbols, the syntactical conception transforms mathematical

in some way. For example, in support of the emphasized portion of these remarks, we are referred to Carnap ([1934]1953), where Carnap asserts: "*The formal sciences do not have any objects at all*; they are systems of auxiliary statements without objects and without content." (p. 128. Original emphasis). Cf. *Logical Syntax*, p. xiv.

[5]See Parsons (1995) and Tait (2001) for comprehensive and insightful discussions as to the evolution of Gödel's realist commitments and his understanding of conceptual or mathematical intuition (I use these terms interchangeably throughout).

truth into conventions and, eventually, into nothingness. (Ibid., p. 357)

He labels the idea 'nominalism', but whatever the label, his concern is in the main epistemological. Consider: "it was the primary purpose of the syntactical conception to justify the use of these problematic concepts [ideal mathematical concepts such as 'infinite set'] by interpreting them syntactically" (Ibid.) Gödel is of the opinion that a "reduction" of mathematics to syntax is simply insufficient to justify our knowledge of such concepts.

Of key importance here is that Gödel takes our knowledge of mathematics as justified via an appeal to mathematical intuition, and so if Carnap rejects such an appeal, Gödel supposes that he needs to substitute some other means of justification. The syntactic viewpoint is taken as *foundational* by Gödel in this sense. In chapter 3 will see that the Deflationary rejoinder suggests that Carnap in fact has no foundational proclivities whatsoever, and so Carnap and Gödel wind up at a philosophical impasse, with their respective positions aiming at cross-purposes.[6] Remember that this is just the sort of philosophical situation that Carnap wanted his program to help resolve.

But the importance of Gödel's orientation here is not simply the seemingly unrecognized disparity between him and Carnap regarding foundational questions. Rather, his epistemic orientation is important because it causes Gödel to impose a *requirement* upon the syntactic viewpoint in order that it count as a successful alternative to an appeal to intuition. Gödel takes it as obvious and necessary that the syntactic viewpoint restrict itself to "finitary concepts referring to finite combinations of symbols" (Gödel, [1953/9]1995, p. 341) in the demonstration that mathematics is no more than the syntax of language. This is because infinitary notions are a hard sell as *mere syntax*.[7]

We saw in the Logico-Mathematical Interlude that Carnap often relies upon indefinite notions for the investigation of the languages of *Logical Syntax*, especially his indefinite ω-rule in LI and the C-Terms of all his languages. In the case of the concepts defined for LII and in general syntax, Carnap reassures us that the "D-Method" remains fundamental, since proofs of key meta-theorems can be rigorized as derivations in a more expressive meta-language. However, these definitions still

[6]See especially Goldfarb (1995*b*; 1996), and Ricketts (2007). Cf. Ricketts (2003, pp. 267, 271), which makes roughly the same point regarding Carnap and Quine.

[7]Notice that this requirement moves Carnap's philosophy of mathematics toward a finitism not unlike Hilbert's. In fact, at various places in the text Gödel comes close to straightforwardly identifying the syntactic viewpoint with Hilbert's program. We saw in the Logico-Mathematical Interlude that this identification is not accurate.

rely upon concepts that are indefinite to a high degree (e.g., "all properties whatever"). The coherence of this reliance upon meta-linguistic notions in the context of Carnap's program is just what Gödel presses with his first objection.

2.1.1 Gödel's Circularity Objection

Recall the first thesis of the syntactic viewpoint is that mathematics is nothing more than the syntax of language. Gödel believes that Carnap's *reconstruction* of the language of science would include an adequate interpretation of mathematics only if he can recover—without appeal to intuition—all mathematical truths as consequences of "conventions about the use of symbols and their application" (Gödel, [1953/9]1995, p. 356); what we have called, following Gödel in fact, "syntactical rules". Gödel supposes that any such reconstruction will begin with a pre-existing collection of empirical facts, a subset of which we represent in our linguistic framework as a set of synthetic sentences—perhaps as P-Rules and their consequences. One then defines a collection of L-Rules (i.e., a logical consequence relation) over this set. Those sentences which follow purely from these rules without intersecting with the set of synthetic sentences comprise the logic and mathematics for the language. The claim that logic and mathematics are conventional amounts to the choice one has in deciding upon a specific set of L-Rules for the language—different choices of syntactical rules will result in different mathematical logics for our framework.

Gödel's complaint then begins with the observation that in order to count something as properly formal, and so as an admissible candidate for a L-Rule, we need to be sure *already*—at the stage of choosing—that it does not have any empirical content. An inconsistent rule will have as a consequence every sentence, including all the factual ones (its set of consequences thereby intersecting with the previously established set of synthetic sentences), and so in order to maintain the claim that logic and mathematics are conventional, our set of syntactical rules must at a minimum be consistent. But for most L-Rules of interest, by the second incompleteness theorem we cannot *prove* them consistent, and so we have no way to determine whether or not a potential L-Rule is admissible.

A natural response to this complaint might be to defer to a meta-language in order to determine the consistency of candidate rules. Gödel anticipates this response however, arguing that this is no way out of the difficulty. Recall that Carnap's conventionalist attitude toward mathematics is stronger than the claim that logic and mathematics are non-factual. Were this all Carnap was suggesting, then the possibility is left open that some particular set of L-Rules are the "correct set". As observed in chapter 1 however, Carnap suggests furthermore that as with metaphysical disputes, appeal to the Principle of Tolerance is appropriate to address questions about the acceptable methods of proof in mathematical investigations. This is to transform assertions about the legitimacy of certain methods of proof in mathematics into meta-level proposals about the language we wish to use. The logic and mathematics of our framework is then supposed to derive entirely from our choice amongst these syntactical rules. Gödel argues that appeal to a meta-language as a means to secure the admissibility of a candidate rule (via a consistency proof) would be to betray this idea that logic and mathematics are derived entirely from syntax by invoking something like mathematical intuition. In other words, to assume the very rules Carnap is trying to show purely formal for a proof of their consistency is either viciously circular, or *presupposes* that they have some substantive mathematical content.[8]

Gödel concludes that the syntactic viewpoint provides no alternative to mathematical intuition, and in fact goes further than this:

> [...] *at any rate it is clear that mathematical intuition cannot be replaced by conventions, but only by conventions plus mathematical intuition, or by conventions plus empirical knowledge involving, in a certain sense, an equivalent mathematical content.* (Gödel, [1953/9]1995, p. 358. Original emphasis.)

Gödel takes his argument not only as a refutation of the syntactic viewpoint, but as an argument *in favour* of his own epistemic story. Indefinite reasoning seems necessary at some level, since Gödel supposes that knowledge of a rule's consistency is required before we can lay it down as a legitimate convention, and in most cases such a proof will require mathematics which goes beyond mere syntax (in Gödel's sense). So in order to justify most interesting L-Rules Carnap seems compelled to invoke mathematical intuition after all.[9]

2.1.2 Mathematical and Empirical Content

The second thesis of the syntactic viewpoint is that logico-mathematical sentences lack content. Gödel objects to this claim with the straightforward observation that

[8] This objection is not unlike Poincaré's ([1908]1946, chp. IV) objection to logicism on the grounds that any specification of a formal language powerful enough to express mathematics will presuppose mathematical induction (but perhaps in a non-obvious way).

[9] Cf. Gödel ([1953/9]1995, p. 337)

the Logical Empiricists presuppose a notion of 'content' that excludes anything other than *empirical* content. Hence the syntactic viewpoint begs the question against anyone who, like Gödel himself, supposes that "[...]with mathematical reason we perceive the most general (namely the 'formal') concepts and their relations, which are separated from space-time reality" (Gödel, [1953/9]1995, p. 354). By "formal" here Gödel of course means 'abstract'—concepts that, while they lie fundamentally beyond sense experience, are just as independent and objective as empirical concepts, and figure in (mathematical) facts just the same. In this category Gödel includes both objects like sets or numbers and higher-order entities like properties or relations, all graspable via our faculty of mathematical intuition.

Recall that the Logical Empiricists derive their understanding of 'content' not only from a presumption of verificationism, but also from the Wittgensteinian doctrine of tautology presented in the *Tractatus* ([1922]1974). Wittgenstein supposes that the content of a sentence is a function of how it partitions the possible states-of-affairs of the world into those compatible and those incompatible with that sentence. A sentence is empirically informative in virtue of its making this partition. Since logically true sentences are necessarily true, they are compatible will all possible states-of-affairs and so exclude no possible configuration of the world. In other words, tautologies tell us nothing regarding the objects of our discourse, and so are content-free in this sense. Such an understanding is important in order to overcome the problem of empiricism.

It should not be immediately obvious how this supports the Logical Empiricist notion of 'content' in a way distinct from a straightforward appeal to verificationism. Goldfarb (1996) provides an insightful discussion in this regard with respect to early Logical Positivism.[10] If a tautology is simply taken as 'true no matter what the *experiential* facts are', then this is indeed just an appeal to verificationism, and so Gödel's criticism seems justified. By insisting that all cognitively meaningful sentences are either analytic or submit to empirical verification, the Positivists are here presumptively excluding the possibility of non-experiential content. Goldfarb argues that Schlick and Hahn understood logic and mathematics in just this way.

[10]Throughout the dissertation I have so far refrained from distinguishing between Logical Positivism and Logical Empiricism, using the latter label exclusively. This is purely for reasons of simplicity, since the distinction is irrelevant to my argument. If one insisted, we might call Logical Positivism the movement originating with the group of scholars that were members of the Vienna Circle in the 1920s. Logical Empiricism is then a label for the extension of that movement in the 1930s and beyond, after many members of the Vienna Circle had relocated to North America. In any case, my identification of these two labels is concordant with, e.g., Richardson & Uebel (2007, n. 1) in their "Introduction" to the *Cambridge Companion to Logical Empiricism*.

Carnap's case is more complex. Although his formal explication of 'content' in *Logical Syntax* captures Wittgenstein's general idea, it is not premised on a straightforward appeal to verificationism. Carnap defines the content of a sentence to be the class of its non-valid consequences.[11] This definition allows him to maintain a distinction between sentences true purely in virtue of the rules of a framework, and those that require some sort of extra-linguistic justification. Thus analytic sentences have null content, contradictory sentences have total content, and logically equivalent sentences have the same content. In the less formally rigorous *Foundations of Logic and Mathematics*, Carnap instead emphasizes a contrast between *factual content*, possessed by empirical sentences, and logico-mathematical sentences, which have none since they act merely as formal auxiliaries for the transformation of empirical sentences. In either case, Carnap is relying upon the first thesis of the syntactic viewpoint to ground his understanding of 'content'. If mathematics can be recovered as merely the combination and manipulation of the symbols of a formal calculus, it is plausible to suppose that it has no independent content of its own. So unlike Schlick and Hahn, who *assume* that all content is empirical with an appeal to verificationism, Carnap aims to *show* via reconstruction that mathematics involves nothing that ought be construed as content. This difference will be of key importance below.

However, since Gödel takes his first objection to undermine Carnap's reduction of mathematics to syntax of language, if correct this avenue for justifying a purely empirical interpretation of 'content' is no longer open. Gödel concludes that there are no grounds to exclude the possibility of non-experiential content grasped by mathematical intuition.

2.2 Other Circularity Objections

We turn now to an examination of the other aforementioned circularity objections to Carnap's philosophy of mathematics. As noted, these criticisms share with Gödel's first objection a basic complaint about Carnap's program: If it is taken as a conventionalist foundation for mathematics either in the sense of *generating* mathematical truth, or in the sense of *justifying* or *explaining* our knowledge of mathematics, then the program seems to be viciously circular in its need to *presuppose* at some level

[11] In a language with P-Rules, this is equivalent to defining the content of a sentence as the class of its indeterminate consequences. Notice that this entails that P-Valid sentences without indeterminate consequences will have a null content, even though they are empirical (*viz.*, synthetic). Carnap discusses the possibility of alternate definitions of *Content* on p. 175 of *Logical Syntax*. He there argues that his definition comports best with standard use.

the very notions for which the program was supposed to provide an account. And so with Gödel, these authors each conclude that Carnap's superficially syntactical account of logic and mathematics must in fact make a tacit appeal to some further epistemic or ontological element such as experience, mathematical intuition, or a pre-existing realm of mathematical truth itself.

2.2.1 Quine on Conventions

The oldest instance of a circularity objection that targets Carnap's work (of which I am aware) is Quine's attack on conventionalism in his "Truth by Convention" ([1935]1976). There are several interesting arguments in this paper, and by no means are all of them aimed at Carnap's philosophy of mathematics as we have characterized it.[12] The argument of interest appears in the final pages of the paper, where Quine explicitly considers a program whose goal is to *generate* or *explain our grasp* of logical truths (*viz.*, analytic sentences) on the basis of a set of distinguished and explicitly laid-down conventions:

> In a word, the difficulty is that if logic is to proceed *mediately* from conventions, logic is needed for inferring logic from the conventions. Alternatively, the difficulty which appears thus as a self-presupposition of doctrine can be framed as turning upon a self-presupposition of primitives. (p. 104. Original emphasis.)

As Quine demonstrates in painstaking detail, the problem is just that any language of interest (take even the propositional calculus) will have an infinite number of logical truths. Due to this, we will need to rely either upon axiom schemata along with some rule of substitution, or otherwise the isolation of a certain set of primitive sentences along with rules of consequence, by which to derive the other logical truths of the language. But in the act of applying these rules (whether rules of substitution or consequence), we will need to tacitly presuppose them in order to derive the further truths. Or, alternately, in explaining the application of the rules to sentences we will need to presuppose the meanings of various logical particles for which, again, the axioms and rules were meant to offer an account. As Quine observes, the conventionalist ends up in a position akin to Achilles in his conversation with the tortoise:

[12]Indeed, the most extensive argument of the paper rather concerns a mathematical implicationism (sometimes called "if-thenism") as was held by Russell for a time, e.g., Russell (1920, chp. XVIII). Carnap also held this view at one point (Carnap, [1931]1983), but abandoned it in favour of his mature program developed in *Logical Syntax*.

Unable to justify the use of *modus ponens* in his reasoning for need of utilizing that most fundamental of principles in its own justification.[13]

2.2.2 The First Incompleteness Theorem

Quine makes this same point again in his contribution to the Carnap-Schilpp volume (Quine, 1963, §IV). The majority of this paper directly concerns Carnap's philosophy of mathematics, or what Quine calls the "linguistic doctrine of logical truth". In §VII he specifically discusses the technical situation in *Logical Syntax*, noting first that "[w]hatever our difficulties over the relevant distinctions, it must be conceded that logic and mathematics do seem qualitatively different from the rest of science." (p. 397) However, Quine concludes that this seeming distinction amounts to no more than a difference of degree, rather than kind.[14] The problem, according to Quine, is that both mathematical and empirical theories submit to formalization according to the same general scheme: Supplement a basic logical framework (say instantiating the first-order predicate calculus) with the choice of a further set of L- or P-Rules. The resulting frameworks can in both cases be thought of as just formal axiomatic theories, and so it is unclear why we should consider the one (with only L-Rules) analytic, or conventional, while the other (including P-Rules) is partially empirical.

Another way to see Quine's point is to recall from the Logico-Mathematical Interlude that Carnap expresses the complete division between the formal and factual components of his languages via the fundamental theorems which show that the logical sentences of their respective languages are L-Determinate. This shows that the logical sentences can act as formal auxiliaries, because they are completely determined by the syntactical rules of their language. In the case of LII this proof relies upon the prior definition of 'Analytic' in LII, which amounts to what Quine calls a "truth-definition" for the language. Quine observes that for a language as powerful as Carnap's LII, the proof that every canonical logico-mathematical sentence is L-Determinate (in the case of LII, Theorem $34e.11$) must be carried out in a meta-language more powerful than the object-language. As we saw, this is because of Gödel's first incompleteness theorem, which tells us that any definite language of sufficient strength will include sentences that are irresoluble within the language.

[13] Cf. Carroll ([1895]1979).

[14] The most famous expression of Quine's arguments in this direction are of course in his "Two Dogmas of Empiricism" ([1951]1980). As noted in chapter 1, the argument with which we are presently concerned is distinct from these arguments against the analytic/synthetic distinction and for his brand of holism.

But if *this* is the thesis forwarded by the linguistic doctrine of logical truth, then

> [...]the thesis that logico-mathematical truth is syntactically specifiable becomes uninteresting. For, what it says is that logico-mathematical truth is specifiable in a notation consisting solely of [names of signs], [an operator expressing concatenation of expressions], *and* the whole logico-mathematical vocabulary itself. (1963, p. 400. Original emphasis.)

So along the same lines as Quine's original 1935 objection to conventionalism, and Gödel's objection to the first thesis of the syntactic viewpoint, the need for Carnap to appeal to mathematics as tacitly understood at the meta-level is here taken to undermine the purpose of the linguistic doctrine of logical truth, which according to Quine was to *show* that logico-mathematical truth is "grounded in language".

Although Potter (2000, chp. 11) devotes a good deal of space to discussing Gödel's objection, he argues that Carnap's logico-mathematical program in *Logical Syntax* founders foremost upon a more sophisticated form of this Quinean criticism.[15] Note in the above that Quine, like Gödel, assumes Carnap must limit himself to *finitary* notions in order that his conventionalism have a chance to succeed. This is because an appeal to infinitary notions like a indefinite consequence relation strains the idea that the methods of syntax are merely the formally-directed combination and manipulation of signs. Potter sees this technical situation as placing Carnap in a dilemma: Either he goes ahead and specifies the definition of 'Analytic' for a language by invoking an indefinite consequence relation anyway,[16] or he must acquiesce to accepting that some of the vocabulary customarily taken to be logico-mathematical will end up descriptive in that language.

This latter result follows just because, again owing to Gödel's first incompleteness theorem, the language will be incomplete and so certain canonically logico-mathematical sentences will not be decided by the syntactical rules of the language. This is just to say that those sentences, while containing only customarily logical expressions, are indeterminate. Therefore Carnap will end up having to count the ostensibly logical expressions which figure in such sentences as descriptive.[17] Now,

[15]As far as I am aware, Potter does not attribute the criticism to Quine. Michael Friedman (1999) also independently discusses this same objection.

[16]As we saw in the Logico-Mathematical Interlude, one can accomplish this in at least two ways. From within a language, appeal to an infinitary rule of inference like Carnap's ω-rule (Cf. *Logical Syntax*, §14). From without, define a notion of 'Analytic' using a meta-language (Cf. *Logical Syntax*, §34*a–f*). We saw Carnap pursue both strategies, in LI and LII respectively.

[17]Carnap in fact addresses this issue in *Logical Syntax* (see *Example* on pp. 231–232). However, he discusses the issue only as a *criticism* of other presentations of logical systems (e.g., the *Principia*

since Carnap is arguing that logic and mathematics are non-factual, Potter observes that Carnap must pursue the former strategy of defining 'Analytic' for LII in an indefinite way—as he in fact does. This strategy requires Carnap to give up the ability to "explain how a finite intelligence can grasp arithmetical truths which appear to refer to an infinite domain of objects" (Potter, 2000, p. 286). But if his goal is to recover a plausible story as to how mathematical truths and their objects are knowable, presumably Carnap should neither appeal to blatantly infinitary reasoning, as in LI, nor presuppose the very notions for which he is attempting to account, as in LII. And thus Carnap's program seems to fail.

2.2.3 Non-standard Interpretations

The final circularity objection we will consider is once again found in Carnap's Schilpp volume, this time in the contribution by E.W. Beth (1963).[18] Beth's is unique amongst our collection of circularity objections in that it is cast partially in model-theoretic terms, and in the fact that it concerns not the *strength* of the mathematics which must be presupposed by Carnap to carry through his program, but rather that a *particular interpretation* of the meta-language must be assumed in the investigation of a rational reconstruction.

The technical details of this objection are quite involved, but the basic idea is that the languages Carnap constructs in *Logical Syntax* are such that they admit of non-standard interpretations. In the simplest case we need only recognize that formal languages admit of arithmetization, and so take a non-standard interpretation of $\mathbb{N}$.[19] Keeping this in mind, Beth introduces a fictional logician, Carnap*, whose intuitive understanding of the language LII is non-standard. Specifically, his intuitive understanding (which Beth identifies with some model M*) is guided by the extension of LII, called LII*, which includes as an axiom the negation of the arithmetization in LII of a sentence expressing the consistency of LII. What is essential here is that Carnap* takes his intuitive interpretation also as a guide to the *meta-language*

Mathematica (Whitehead & Russell, [1910–1913]1997)) because they include only finitary rules of deduction without specifying any indefinite rules of consequence needed to provide a language with a complete criterion of validity.

[18]Ricketts (2004) and Friedman (2009) also discuss this objection.

[19]This informative example derives from the discussion in Friedman (2009). The situation is in fact a bit more complex than this, since Carnap's LII is a higher-order theory of types. Given the standard semantics for such a language, Peano Arithmetic is categorical and so does not admit non-standard models. Assuming a Henkin semantics however, which Beth does, higher-order Peano Arithmetic does indeed admit non-standard models as in the first-order case.

within which LII is investigated.[20] The consequences of all this are that Carnap* will systematically misinterpret the main inductive definitions and lines of argument in *Logical Syntax*, leading up to Carnap's proof that LII is non-contradictory (Theorem 34*i*.23), and any subsequent theorems which rely upon this result.

Supposing that we take LII to in fact be consistent, Beth argues that in the situation as described, Carnap and Carnap* will disagree with regard to certain properties of LII:

> Now Carnap* could settle the dispute at once in his favor by exhibiting the inconsistency which, according to him, exists in Language II, that is, by actually deriving a contradiction. But this he is unable to perform [because LII is in fact consistent]. So he is compelled to proceed entirely by indirect argument, guided by his intuitive model M*. But in doing so, he will again and again resort to assertions which Carnap cannot accept and for which no basis can be found in *Logical Syntax*. Therefore, he will not be able to convince Carnap of an error.
>
> On the other hand, Carnap will no more be able to convince Carnap* for the statements to which Carnap* appeals are consistent with every statement made in *Logical Syntax*; if Carnap is to refute Carnap*'s assertions, he must resort to his "sufficiently rich syntax language" for Language II, and some statements provable in this syntax language are, for Carnap*, either false or devoid of meaning. (Beth, 1963, p. 481)

The point here is just that the Carnaps are approaching LII from different intuitive perspectives, owing to their conflicting informal understandings. Carnap* supposes the language to be inconsistent, while Carnap obviously does not—the assumptions made in their respective informal meta-languages reflect these divergent perspectives.

The situation is analogous to two logicians arguing over certain properties of the natural numbers while one of the participants holds a non-standard interpretation. When the numerals are listed: '$0, 0', 0''$, *and so on*', the first logician will understand the expression '*and so on*' in the customary way, while the second logician will include an additional, non-finite numeral. In this case however, the disputants can compare their divergent models in a suitable semantic theory. In the dispute between Carnap and Carnap* there is no such recourse, since *Logical Syntax* explicitly disavows the

[20]In his reply to Beth, Carnap notes (p. 929) that this assumption is essential for Beth's argument, although Beth does not make this quite explicit.

semantic treatment of formal languages.[21] Instead, the Carnaps can only resort to formalizing their meta-languages. Carnap can here display the consistency of LII—and Carnap* its inconsistency—but at this stage each will of course reject the other's formalization as an adequate characterization of the informal meta-language used throughout *Logical Syntax*.

Beth's conclusion is that *Logical Syntax* relies upon a tacitly understood interpretation (the so-called "standard interpretation") of the concepts and words of the informal meta-language used to convey the ideas in the book. But the assumption of such a tacit understanding is not innocent, since it amounts to a prior understanding of the mathematics in question. So Beth, as with Gödel and Quine above, concludes that "[...]Carnap has not been able to avoid every appeal to logical or mathematical intuitions[...]" (p. 502), or what amounts to the same thing for Beth, to the *ontological* assumption that mathematical objects exist in some robust sense.

2.2.4 The Crux of the Circularity Objections

Notice that Gödel's 1932 concern about Carnap's original definition of 'Analytic' for LII in *Logical Syntax*, and Carnap's solution to that problem, leads also to questions along the lines of Beth's objection. Recall from the Logico-Mathematical Interlude that Carnap originally intended to define 'Analytic' for LII via the straightforward substitution of expressions in his evaluation rules. We can see this today as a syntactical analogue to Tarski's use of satisfaction and reinterpretation in his definition of mathematical truth. However, while Carnap's approach works in the case of terms—e.g., the closed sentence '$P(n)$' is analytic just in case all the sentences that result from substituting numerical expressions for the numerical variable are analytic—due to cardinality restraints on the language, this same scheme will not work for evaluating sentences with variables of higher-level. Instead, Carnap is forced to take the notion of "all properties" in his definition to range over *all numerical properties whatsoever* and not just all those numerical properties *expressible* in LII.

The immediate question is whether this leads to the metaphysical assumption of the domain of properties as existent in some sense distinguished from considerations of language. Recall that Carnap argues not:

> We have here absolutely nothing to do with the metaphysical question as

[21] As we have seen, many of the concepts and techniques that Carnap uses in *Logical Syntax* would today be considered semantic rather than syntactic. However, there is nothing like a model theory available for comparing distinct interpretations of a formal language.

> to whether properties exist in themselves or whether they are created by definition. The question must be rather put as follows: can the phrase "for all properties..." (interpreted as "for all properties whatsoever" and not "for all properties which are definable in [the meta-language]") be formulated in the [meta-language]? This question may be answered in the affirmative. The formulation is effected by the help of a universal operator with a [predicate variable], i.e., by means of '$(F)(\ldots)$', for example. (*Logical Syntax*, p. 114)

His answer is as expected: We should not enter into a debate about metaphysics because we can treat the debate as a question of the means of expression we are comfortable allowing in our language—in this case our meta-language. But the standard answer is here not quite so straightforward. Carnap's definition of 'Analytic' for LII takes place in LII's meta-language, which in *Logical Syntax* is only informally stated—English supplemented with some logico-mathematical signs. Since it is the suitability of the meta-language that is here in question, according to Carnap's own scheme really it should be formalized. We can of course do this using a meta-meta-language. But the same problem then reappears at this level, and so on *ad infinitum*. If on the other hand we were to leave the meta-language informal, we cannot be sure that a correct extension for the phrase "for all properties" can be secured, or, at the least, we cannot ensure that all parties will understand the interpretation of that phrase in the same way. And this is just Beth's point.

The questions are: (i) Whether such a regress of formalized meta-languages is really vicious; or alternatively (ii) Whether appeal to informal means of expression at some level is coherent in Carnap's program. These questions are the crux of all the circularity objections. In the case of Gödel's circularity objection, it is a matter of Carnap's needing to appeal to informal mathematical notions in proving that syntactical rules are factually non-creative. In the case of Quine and Potter, the concern is instead whether appeal to informal and infinitary notions are acceptable in the proofs of fundamental meta-theorems like 14.3, 34*e*.11, and 53.2, which show that all the logical sentences of their respective languages follow purely from the L-Rules. And Beth's case was described above. In each case Carnap encounters either the requirement of a regress of ever-stronger meta-languages, or the need to appeal to informal mathematical notions, which is taken by his objectors as an appeal to intuition. Thus Carnap's program is either ungrounded, viciously circular, or at best cannot establish the Logical Empiricist thesis that mathematics is conventional.

2.3 Explication and Scientific Methodology

So a coherent reading of Carnap's meta-philosophy seems bound to confront at least one of the two circumstances brought to light by the circularity objections. Again, it seems necessary that Carnap appeal to either: (i) An infinite hierarchy of formal languages; or (ii) Informal mathematics at some level. As noted in our first chapter, the Deflationary interpretation leans heavily upon the Principle of Tolerance, suggesting that this principle grounds Carnap's entire program. In chapter 3 we will see that this logocentric reading embraces the idea of an infinite hierarchy of formal languages to make sense of Carnap's program, and responds to the objections by denying their premise that Carnap is doing anything at all foundational. On this reading he is instead only interested in the development of linguistic frameworks.

My interpretation also denies that Carnap was engaged in a foundational project *in the sense* of Gödel's "syntactic viewpoint" or Quine's "linguistic doctrine of logical truth". But contra the Deflationary reading and despite Carnap's stress upon formalization, I will argue that appeal to informal notions is an important aspect of Carnap's meta-philosophy. Carnap's fundamental goals are the *explication* of the language and concepts of science and the *introduction* of mathematical methods into philosophy. Both involve taking seriously the methods and conclusions of the sciences, and seeing philosophy as a partner in that enterprise. To these ends, Carnap can non-viciously utilize the methods and results of informal mathematics.

2.3.1 Informality and Material Interpretation

Carnap certainly appeals to our informal understanding of mathematical structures in many places. In the Logico-Mathematical Interlude we noted in passing that many of Carnap's elucidatory remarks reference what he calls our "material interpretation" of logico-mathematical notions.[22] For example, in §12 of *Logical Syntax* after presenting the D-Rules for LI he offers little justifications for their soundness. Along with these arguments he remarks:

> These rules are formulated in such a way that, when the sentences are

[22] In *Logical Syntax*, this is just our colloquial, informal way of speaking, that is, the language of most practicing scientists—English supplemented with some logico-mathematical signs. After Carnap's adoption of semantics this becomes a more rigorous notion, since Carnap then has the tools to treat interpretations in a more formal way. As we will see, he still often appeals to the "standard interpretation" of an expression, taking it as given that it is something all parties can come to understand.

> materially interpreted, they always lead from true sentences to further true sentences. (*Logical Syntax*, p. 32)

In fact, he justifies his RI4 (Rule of Inference 4) by noting that it corresponds to "the ordinary arithmetical principle of complete induction[...]" (p. 33).

The Analyticity of Induction and Choice in LII

While such remarks can be brushed aside as purely expository, there is strong evidence in *Logical Syntax* that our informal understanding is playing a more substantial role. Recall yet again from the Logico-Mathematical Interlude that Sarkar (1992) and Ricketts (2007) observe several of the technical results in *Logical Syntax* cannot be coherently interpreted as performing any kind of foundational or justificatory work. This causes Sarkar to dimiss the proofs outright as no more than technical exercises. In the interlude we discussed Carnap's proof of non-contradictoriness for LII, and showed that it actually possesses some pedagogical significance in *Logical Syntax*. The other puzzling results are Carnap's proofs that the axiom of choice and the principle of complete induction are analytic in LII. The puzzle regards the purpose of these proofs, given that Carnap blatantly presumes in the meta-language the very principles he shows analytic. However, we can interpret these results as being of fundamental importance to the goals of *Logical Syntax*.

As with the proof of the non-contradictoriness of LII, a cursory examination of Carnap's remarks makes clear that he is well-aware of the technical situation. He actually highlights his meta-linguistic assumptions in §34*h*:

> The proof of Theorems 1 and 2 [Induction and Choice] are interesting because they involve a fundamental question: in each one of these proofs, there is used a theorem of the syntax-language which corresponds with the theorem of the object-language whose analytic character is to be proved. (*Logical Syntax*, p. 121)

And he goes so far as to call the proofs "interesting" for just this reason. In light of our discussion in the previous interlude this should not be surprising. Carnap is first of all drawing attention to and explaining the relationship between meta- and object-language, and also making clear the fact that this distinction means that his proofs are not viciously circular:

> It is clear that the possibility of proving a certain syntactical sentence depends upon the richness of the syntax-language which is used, and es-

> pecially upon what is regarded as valid in this language. In the present case, the situation is as follows: we can work out in our syntax-language S (for which we have here taken a not strictly determined word-language) the proof that a certain sentence, $\mathfrak{S}_1$, of the object-language II is analytic, if, in S, we have a certain sentence at our disposal, namely, that particular sentence of S which (in ordinary translation) is translatable into the sentence $\mathfrak{S}_1$ of II. From this it follows that our proof is not in any way a circular one. (*Logical Syntax*, pp. 123–124)

While this shows that the proofs again serve a pedagogical purpose (remember that the careful observance of now-standard distinctions between meta- and object-levels was at the time relatively novel), more interesting is the fact that Carnap is again appealing to our understanding of concepts in their "ordinary translation" in the meta-language. In other words, a sentence of the informal meta-language—English supplemented with certain logico-mathematical signs—is here taken as a correlate for a formal sentence of LII, and utilized in the meta-linguistic proof that a sentence of that object-language is analytic.

Still, none of this is very surprising, and I will omit the technical details of the proofs because they are not particularly illuminating. However, what is interesting are Carnap's remarks regarding the results of the proofs:

> The proofs of Theorems 1 and 2 [Induction and Choice] must *not* be interpreted as though by means of them it were proved that the Principle of Induction and the Principle of Selection were materially true. They only show that our definition of 'analytic' effects on this point what it is intended to effect, namely, *the characterization of a sentence as analytic if, in material interpretation, it is regarded as logically valid.* (Ibid., p. 124. My emphasis.)

Just to be absolutely clear, Carnap is definitely not attempting to argue that induction or choice are valid (i.e., "materially true") in some absolute justificatory sense. Instead, what Carnap takes himself to have shown is that his formal notion of 'Analytic' in LII captures what it was intended to capture—that it successfully explicates our unreconstructed or intuitive notion of classical logico-mathematical truth. These proofs therefore serve as *evidence* for this by showing that the formal language LII recovers canonically mathematical principles as analytic.

He continues:

> The question as to whether the Principle of Selection should be admitted into the whole of the language of science (including also all syntactical investigations) as logically valid *is not decided thereby.* That is a matter of choice, as are all questions concerning the language-form which is to be chosen (cf. the Principle of Tolerance, §17 and §78). In view of the *present knowledge* of the *syntactical nature* of the Principle of Selection, its admission should be regarded as expedient. (Ibid. My emphasis.)

Besides showing considerable epistemic subtlety, what Carnap is saying is that we are justified in this attitude toward the axiom of choice precisely *because* it is logically valid in an informal or unreconstructed sense. In other words, Carnap is here *stating* that the axiom of choice is considered logically valid in our informal understanding of classical mathematics, and this is reason enough to consider it syntactical (i.e., as a formal auxiliary), and so analytic in LII.[23] So Carnap goes through the work of proving these theorems because they help to demonstrate the *adequacy* of the C-Terms of LII as an explication of classical mathematics—since classical mathematics includes the axiom of choice, it had better come out analytic in LII if Carnap wants to say that LII successfully captures the methods of proof utilized in classical mathematics.[24]

Carnap's Response to Beth

It seems then that appeals to our informal understanding of classical mathematics are doing some work in *Logical Syntax.* At the least this suggests that Carnap did not take his program as foundational in Quine's sense of our choice of linguistic conventions *generating* the mathematical truths. Instead, Carnap is presupposing the "syntactical nature" of the axiom of choice in mathematical practice, and this then justifies its analyticity in LII.

Such appeals seem to underlie Carnap's work throughout his entire career. For example, we are lucky enough to have Carnap's response to Beth's objections from the Carnap-Schilpp volume (Carnap's response is in the same volume), which sheds

[23] I should point out that Carnap is being somewhat optimistic here, since there was in fact significant controversy over the axiom's soundness throughout the 1920s and into the early 1930s, when Carnap was writing *Logical Syntax.* These doubts were somewhat allayed by Gödel's (1938) announcement of a proof of the axiom's consistency relative to the axioms of set theory. Our point is of course that in Carnap's assessment of the field at the time, he seems to have thought that consensus had settled upon the axiom's being considered a valid principle.

[24] There is much to say about these passages with regard to the methodological role of the Principle of Tolerance, but we will save this for the discussions below and in chapter 3.

even more light upon this issue:

> [...]Beth's thesis says that it is essential for the purposes of my theory that the English words of my metalanguage *ML* are sometimes used with a fixed interpretation. *I emphatically agree; I would even say that this is the case not only sometimes but practically always.* For the reasons explained earlier, *this seems to me so obvious that I am surprised that Beth should regard it as necessary to demonstrate it by particular examples.* It is of course not quite possible to use ordinary language with a perfectly fixed interpretation, because of the inevitable vagueness and ambiguity of ordinary words. Nevertheless it is possible at least to approximate a fixed interpretation to a certain extent, e.g., by a suitable choice of less vague words and by suitable paraphrases. (Carnap, 1963, p. 930. My emphasis.)

Carnap seems to have no qualms about utilizing an informal meta-language in fixing the meanings of our expressions. This sort of appeal applies *mutatis mutandis* to Carnap's strategy for defining 'Analytic' in LII using our informal understanding of "for all properties" in regard to Gödel's concern over the syntactical nature of Carnap's definition. We can formalize this construction in an expressive-enough meta-language using symbols for the quantification over predicates of any level, but at some point we must appeal to our tacit understanding of the correct range for this quantification. In the passage above Carnap assumes that as long as everyone is being earnest, even though our colloquial language is often vague in many cases substantial agreement as to our meaning can be reached (as distinct from agreement upon an issue itself).[25]

The "reasons" Carnap offers for why the informal meta-language should be taken as having a fixed interpretation are on the previous page:

> It seems to me obvious that, if two men wish to find out whether or not their views on certain objects agree, they must first of all use a common language to make sure that they are talking about the same objects. (Ibid., pp. 929–930)

[25]In the next chapter we will see that Carnap's response here is at odds with the Deflationary reading, which suggests that because of an appeal to the Principle of Tolerance Carnap requires an infinite hierarchy of ever-stronger formalized meta-languages. On this point see especially Goldfarb & Ricketts (1992, pp. 71–72) and Goldfarb (2009, p. 120).

So it should be clear that complete formalization at every level is not something Carnap thought his program required. Rather, our logical investigations are approached from the perspective of our informal, colloquial language—the language that scientists tend to use in practice. We take up the meta-logical, conceptual tools of logical syntax when we get down to the business of constructing or investigating linguistic frameworks, or forwarding reasons for the choice of one framework over another. These frameworks can act as proposals for modifying or rigorizing the structure of our language—introducing or refining concepts, suggesting new syntactical rules, or proposing some entirely novel means of description for an established set of empirical facts—but in the end they must be grounded by our established scientific theories.

The point is that Carnap is not interested to replace the methodology of *mathematics*, which in any case will get along fine without philosophy. Nor is his project to justify the practices of mathematics or the mathematician's use of whatever mathematical concepts, as Gödel supposes. Again, philosophers must "renounce the proud claims of a philosophy that sits enthroned above the special sciences" (*Logical Syntax*, p. 332). We are instead trying to bring scientific methods into philosophy, and this involves taking seriously the methods and conclusions of the sciences, including mathematics. So Carnap is using our informal understanding of mathematics almost as a premise in his reconstructive project. The Logic of Science is the logical analysis of the concepts and language of science, and so the actual theories and practices of science inform and constrain our reconstructions to a significant degree. A closer analysis of Carnap's notion of explication will make this clear.

2.3.2 Carnap's Notion of Explication

We have already seen many examples of Carnap offering explications for informal or colloquial concepts. Carnap's definitions of 'analytic' and 'logical consequence' in LII, and his definitions of 'logical expressions' and the division between L- and P-Rules stand as our most prominent examples. However, we should offer a more explicit conception of Carnap's notion.

> **Explication:** The replacement of a vague or imprecise scientific concept (the *explicandum*) with a more exact definition (the *explicatum*) embedded in a systematic structure of scientific concepts.

This definition is adapted from Carnap's discussion in "Two Concepts of Probability" (1945), which as far as I know is the first instance of this terminology appearing in his work.[26] However, in his 1961 preface to the second edition of the *Aufbau*, Carnap explicitly identifies his earlier notion of a rational reconstruction with explication:

> By rational reconstruction is here meant the searching out of new definitions for old concepts. The old concepts did not ordinarily originate by way of deliberate formulation, but in more or less unreflected and spontaneous development. The new definitions should be superior to the old in clarity and exactness, and, above all, should fit into a systematic structure of concepts. Such a clarification of concepts, nowadays frequently called "explication," still seems to me one of the most important tasks of philosophy, especially if it is concerned with the main categories of human thought. (Carnap, [1928]1961, p. v)

Carnap stresses here that *explicata*, these newly-defined, exact concepts, should "fit into a systematic structure of concepts". Not surprisingly, the most typical way that Carnap accomplishes this is to utilize a formal logical language for the definition and expression of the *explicata* and any related concepts. This sometimes occurs by including the required primitives in the syntactical rules of the language from the beginning, or by supplementing a basic logical language with an axiomatic theory—e.g., Peano's axioms or axioms for classical mechanics. This choice is for Carnap solely a matter of convention. In the case of the concepts developed in *Logical Syntax*, we saw that most are defined by explicit definition, while the other concepts developed at the same time perform the function of the systematic structure in a self-supporting way.

An important question that Carnap says frustratingly little about is the *desiderata* for an adequate or successful explication. Certainly some *explicata* are going to be more adequate—both in the sense of acting as more representative formal correlates (i.e., better capturing an *explicandum*), and in the sense of providing greater insight into the *explicandum* or being more fruitful—than others.[27] Carnap himself implies this in his discussion of probability:

[26]Beaney (2004) agrees with this observation. His paper provides an excellent overview of the evolution of Carnap's methodology, and an insightful discussion of the influence of Frege and Husserl on Carnap's notion of explication. Beaney (see esp. §4) also agrees with one of the key interpretive claims of this section: that while the label may have changed, there was little to no shift in Carnap's actual goals and methods between his early and mature periods with regard to explication/logical analysis/rational reconstruction. Also recall Carnap's reflections on his conception of philosophy from *Logical Syntax* in Carnap ([1942]1975) and reviewed in chapter 1, as well as the sources cited in n. 26 of that chapter.

[27]I will argue in the next chapter that a Deflationary reading of Carnap seems to require that we deny this—certainly an untenable consequence of that interpretation.

> [W]e may say that the problem of probability is the problem of *finding an adequate explication of the word 'probability' in its ordinary meaning*, or in one of its meanings if there are several. (Carnap, 1945, p. 438. My emphasis.)

We also saw that this was the case in the many instances of explication featured in the Logico-Mathematical Interlude. Carnap's definition of 'Logical Expression' proves to be an inadequate explication because it can fail to produce the correct demarcation between logical and descriptive vocabulary in LI and LII. Whereas his definition of 'Analytic' for LII seems adequate because it successfully recovers our informal notion of classical mathematical truth.

Carnap's fullest discussion of his notion of explication is found in §1–6 of *Logical Foundations of Probability* (1950), where he highlights that the problem of evaluating the adequacy of an explication is not an exact one, since the very reason we are giving an explication is that the *explicandum* is unclear and imprecise. Thus there is an element of conventionality involved:

> Since the datum is inexact, the problem itself is not stated in exact terms; and yet we are asked to give an exact solution. This is one of the puzzling peculiarities of explication. It follows that, if a solution for a problem of explication is proposed, we cannot decide in an exact way whether it is right or wrong. Strictly speaking, the question whether the solution is right or wrong makes no good sense because there is no clear-cut answer. The question should rather be whether the proposed solution is satisfactory, whether it is more satisfactory than another one, and the like. (Carnap, 1950, p. 4)

The choice between distinct *explicata* as explications of one *explicandum* is not a definite matter of being correct or incorrect. As Frege observes in our epigraph to this chapter, it is rather a question of which *explicatum* is in the end most convenient and fruitful. Carnap highlights this in his discussion of definite descriptions in *Meaning and Necessity* ([1947]1956). The various schemes of Frege, Russell, and Hilbert & Bernays for dealing with descriptions without unique referents is said by Carnap to be a matter of a practical choice between proposals, but as of yet researchers have not come to an agreement upon which proposal should be standardly adopted.

This element of conventionality in an explication is essential to an understanding of Carnap's project, since it relinquishes him of having to *prove* completely or absolutely that an explication is true or correct in some sense. The best we can do are arguments to the effect that some *explicatum* successfully captures the important characteristics of the *explicandum*. This is exactly what we saw Carnap doing in *Logical Syntax* with regard to his proofs for LII. Similarly, Carnap takes Gödel's incompleteness theorems to show that no intra-language set of D-Rules can be adequate as an explication of classical logico-mathematical truth. To establish a complete criterion of validity for classical mathematics we must therefore introduce C-Terms. Notice furthermore that the adequacy of a particular *explicatum* will depend upon how one chooses to characterize the *explicandum*, that is, which features or properties of the informal or unreconstructed concept one takes to be essential, or even just those properties in which one happens to be interested.

One can also imagine situations where multiple very different *explicata* each provide insight into an *explicandum* from a variety of perspectives, or suggest the previously unrecognized need for a distinction in an *explicandum* into two distinct concepts, requiring two distinct *explicata*. This is the course that Carnap proposes in his work on probability. He distinguishes between two concepts: (i) Representing probability as the degree of confirmation of a hypothesis with respect to evidence; and (ii) Representing probability as relative frequency. Carnap argues that the formal explication of these concepts shows them to be quite distinct, and he notes that many authors have failed to recognize this fact to their detriment.

So even though there is an element of conventionality in the solution to a problem of explication, this does not deny an explication the ability to provide genuine insight into the conceptual foundations of a theory. Indeed, Carnap was very much convinced that the process of explication bears philosophical fruit by what he took to be the wealth of examples, including the Frege-Russell definition of 'Number', Russell's theory of descriptions, Hilbert's axiomatization of geometry, and later, Tarski's definition of truth for formal languages.[28] Thus the process of explication provides just the kind of method that Carnap hoped to introduce into philosophy—one that is both progressive and provides opportunity for genuine insight into the methods and concepts of science.

[28]Indeed, Carnap very frequently cites the Frege-Russell definitions of 'Number' and the individual numerals as exemplars of an explication. Cf., e.g., Carnap (1945, p. 438; 1950, §6; [1947]1956, p. 8; [1954]1958, p. 2). Consider also Carnap's description of Tarski's definition of truth: "Tarski, however, succeeded in establishing an unobjectionable definition of truth which explicates adequately the meaning of this word in common language (but of course is also bound to restrict its employment, as compared with common usage, in order to eliminate the contradictions)." (Carnap, 1949, p. 119).

Requirements for a Successful Explication

Although it was observed above that Carnap says less than I would like about the methodological details for providing an explication, he does offer a list of requirements that a successful explication should fulfill in §3 of (1950):

> 1. The explicatum is to be *similar to the explicandum* in such a way that, in most cases in which the explicandum has so far been used, the explicatum can be used; however, close similarity is not required, and considerable differences are permitted.
> 2. The characterization of the explicatum, that is, the rules of its use (for instance, in the form of a definition), is to be given in an *exact* form, so as to introduce the explicatum into a well-connected system of scientific concepts.
> 3. The explicatum is to be a *fruitful* concept, that is, useful for the formulation of many universal statements (empirical laws in the case of a nonlogical concept, logical theorems in the case of a logical concept).
> 4. The explicatum should be as *simple* as possible; this means as simple as the more important requirements (1), (2), and (3) permit. (Carnap, 1950, p. 7. Original emphasis.)

Notice that only the first criteria regards the relationship between *explicandum* and *explicata*. Carnap suggests that the latter should be similar to the former, but that significant differences are acceptable. An example that illustrates this idea is Carnap's formulation of the D-Rules for LI.

As we have observed, Carnap took LI to represent the tenets of intuitionism, and we encounter him arguing that this language is a suitable formal correlate to intuitionism or finitism in §16 of *Logical Syntax*:

> Some of the tendencies which are commonly designated as 'finitist' or 'constructivist' find, in a certain sense, their realization in our definite Language I. "In a certain sense", let it be noted; for inasmuch as these tendencies are, as a rule, only vaguely formulated, an exact statement is not possible. (p. 46)

This strikes me as a problem of explication, and Carnap offers three reasons that his *explicatum* is successful. In the first place, only primitive-recursive predicate and function symbols are admitted in LI. Carnap notes that "[t]his fact corresponds to the Intuitionist requirement that no concept be admitted for which a method of resolution is not stated." (p. 47). Next, recall that LI admits only bounded quantifiers, and so negated universal sentences cannot be formed. Although LI does have the ability to express unbounded universal quantification in the form of free variables, this cannot lead to the expression of sentences without a finite method for constructing a witness, as with, for example, unbounded existential sentences asserting there is some number that *does not* have such-and-such property.[29] Carnap notes that this restriction corresponds to the intuitionist tendency that "*existence without rules for construction* is considered to be "inadmissible" or "nonsensical" ("meaningless")." (Ibid. Original emphasis.) Finally, Carnap observes that this restriction of the quantifiers addresses the intuitionist rejection of the law of excluded middle:

> It is in order to exclude this inference leading to an unlimited, non-constructive existential sentence that Brouwer renounces the so-called *Law of Excluded Middle*. The language-form of I, however, shows that the same result can be achieved by other methods—namely, by means of the exclusion of the unlimited operators. [. . .] *Thus Language I fulfils the fundamental conditions of Intuitionism in a simpler way than the form of language suggested by Brouwer* (and partially carried out by Heyting). (p. 48. My emphasis.)

In fact, excluded middle is a theorem of LI. Thus the D-Rules of LI are quite different in form from traditional, informal characterizations of the inference rules or logical connectives of intuitionism, although Carnap thinks that they recover the essential tenets and results, and by simpler means. Carnap suggests that this reformulation is actually quite significant because it offers evidence that the controversy surrounding intuitionism does not depend upon the legitimacy of any particular principle, but rather concerns just the expressive power of our language. So although the D-Rules of LI are somewhat dissimilar to the *explicandum* it is an insightful and successful explication nonetheless.

[29]Unbounded quantifiers allow for constructions such as $\sim(x)\ P(x)$, which we could translate as "Not all swans are white" (assuming x here ranges over swans). The problem is that such sentences are not necessarily resoluble. To see this, note that the above example is equivalent to the sentence $(\exists x) \sim P(x)$, which states that "There exists at least one swan that is not white." Now, how would I go about deciding this one way or the other? Going out into the world and failing to find a non-white swan does not prove the statement false, since I might have missed one—the search has no definite end-point. Similarly for natural number properties. Thanks to Gregory Lavers for clarifying Carnap's intent in this section to me and to the other members of the 2011 UWO Carnap Reading Group via correspondence.

A First Response to the Circularity Objections

On the first page of *Logical Syntax*, Carnap outlines his suggestion for the replacement of philosophical methodology. We have quoted this passage in parts throughout, but I quote it here in full:

> That part of the work of philosophers which may be held to be scientific in nature—excluding the empirical questions which can be referred to empirical science—consists of logical analysis. The aim of logical syntax is to provide a system of concepts, a language, by the help of which the results of logical analysis will be exactly formulable. *Philosophy is to be replaced by the logic of science*—that is to say, by the logical analysis of the concepts and sentences of the sciences, for *the logic of science is nothing other than the logical syntax of the language of science.* (p. xiii. Original emphasis.)

We saw in chapter 1 that Carnap later thought that this needed to be supplemented with the addition of semantics, so that the Logic of Science is nothing other than the (pure) semiotical analysis of the language of science. We have just seen that in even later years Carnap described his project more in terms of problems of explication, but that his approach is concordant with the work done in *Logical Syntax*. So the reconstructive part of the Logic of Science can be thought of as the explication of the concepts of science, according to Carnap's notion of explication.

Of key importance here is that our choice of *explicatum* can be neither correct nor incorrect. But neither is it arbitrary, since there are requirements that an explication must meet in order to be counted successful. We can see this as something of a rigorization of Carnap's earlier discussions of the choice of a language being determined by our goals and practical concerns. If the Principle of Tolerance tells us that we are free to choose whatever syntactical rules for our linguistic frameworks that best serve our purposes, then the requirements for a successful explication place an important constraint upon the scope of Tolerance by grounding our choice of rules in our informal understanding of our scientific theories.[30]

Recall that any problem of explication requires first a characterization of the informal concepts to be explicated. This is an essential step, and it necessarily takes place in an unreconstructed domain. Therefore Carnap's program requires some appeal to our colloquial language not only as a means of communication, but also in

[30] I will argue for this in the next chapter, but see especially Carnap's reply to Robert Cohen in Carnap (1963, p. 864).

the posing of our problems and in the evaluation of our proposed solutions. *Formalization* is not a goal of Carnap's program, the goal is a *clarification* of our concepts. Formalization is simply the means to this end. Our colloquial language will become more precise over time, since our explications can be permanently incorporated into that language, but this dichotomy between the "colloquial" language and formal linguistic frameworks is essential since explications are precisifications of language.[31]

So as a first response to the circularity objections, Carnap can point out that his project is not a foundational one, but rather the explication of logico-mathematical (along with other scientific) concepts. Yes there is an appeal to our informal understanding of mathematical structures, but this is a key aspect of that process: We need to show that our explications are adequate, and in any case we are explicating our informal mathematical concepts.

At the present point this response remains unsatisfactory, however. The reason for this is that the Logical Empiricists, Carnap included, seem to be *asserting* that logic and mathematics are nothing but formal auxiliaries. If this is the case, then Carnap's program still needs an argument to justify this position over a contrasting story like Gödel's, which appeals to some kind of mathematical intuition and a platonism to explain the nature of logic and mathematics. In other words: What is the use of insisting upon a process of explication in a completely formalized context when our justification and understanding of these concepts ultimately relies upon our informal understanding in any case?[32] Moreover, we observed in the first chapter that the application of the Principle of Tolerance is facilitated by the formal nature of both logico-mathematical and philosophical sentences. So the broad choice we have in the selection of syntactical rules seems to rely upon our ability to treat certain domains as formal, but this surely requires an argument. The response to these

[31]Carnap offers an example in *Logical Foundations of Probability* (§5) of our quantitative concept 'Temperature', which he takes to be an explication of our pre-scientific comparative concept 'Warmer'. Carnap observes that 'Temperature' has been so fruitful that we now defer to this *explicatum* even when it disagrees with the original *explicandum*. Consider the case where two rooms are at only slightly different temperatures. A casual observation made by moving through each room would indicate that there is no difference between them, that they cannot be sequentially ordered according to warmness. Suppose however that the measuring procedures connected to the concept of 'Temperature' indicate that one is indeed higher in temperature, and so warmer, than the other. Reading a thermometer placed in each room, it would today be unreasonable under normal circumstances for someone not to acquiesce to this conclusion.

[32]Alan Richardson (1994, p. 73) puts the point this way: "if a strong meta language is required for the explicit presentation of the syntax of L, then certain features of the syntax language will, in this investigation, still be left implicit. But if acquiescence in nonprecise syntax language is admissible, then we seem to lose the point of the reconstructive project (why not rest content with object level imprecision also?) and the goal of the formulation of exact sentences about sentences."

criticisms involves understanding one last piece of Carnap's program, which are his motivations for considering logic and mathematics to be purely formal, and the place of that proposition in his overall meta-philosophy.

2.3.3 The Methodological Structure of Science

Recall from above that Carnap presupposes the "syntactical nature" of the axiom of choice—that it is logically valid in our unreconstructed, informal classical mathematics. This is what licenses it being a matter of choice whether to include it as a syntactical rule in LII. If our linguistic framework aims to explicate the methods of proof of classical mathematics, then it needs to recover the results of our mathematical reasoning done using the axiom of choice. Similarly, we have already discussed in chapter 1 why Carnap thought that philosophy should be treated as a formal science. He suggests this on the basis of its methodological similarities to logic and mathematics. But as we asked above, why should we consider mathematics purely formal in the first place? In answering this question it will be useful to re-establish that Carnap was not developing a traditional foundational program, even though some of the things he says could be read in that way.

Recall that the problem of empiricism is the necessity of explaining how logic and mathematics are possible given a robust empiricism. The Logical Empiricist solution to this problem was to count mathematics and logic as analytic. Carnap's conventionalist interpretation of logico-mathematical sentences as syntactical rules is a rigorization of this doctrine. Furthermore, recall that his reflections in his "Intellectual Autobiography" seem to position the analyticity of logic and mathematics as an alternative to a traditional empiricist or Kantian foundations, in the sense of being an alternative justificatory story, or an alternative explanation of our knowledge of mathematics, or of its genesis, or of its truth. However, careful consideration shows that Carnap's concerns actually tend to the methodological rather than the epistemic or logical. I repeat the excerpt:

> What was important in this conception from our point of view was the fact that it became possible for the first time to combine the basic tenet of empiricism with *a satisfactory explanation of the nature of logic and mathematics*. Previously, philosophers had only seen two alternative positions: either a non-empiricist conception, according to which knowledge in mathematics is based on pure intuition or pure reason, or the view held, e.g., by John Stuart Mill, that the theorems of logic and of mathematics

> are just as much of an empirical nature as knowledge about observed events, a view which, although it preserved empiricism, was certainly unsatisfactory. (Carnap, 1963, p. 47. My emphasis.)

By a "satisfactory explanation of the nature of logic and mathematics" here, I think Carnap has in mind nothing more than an explication of logic and mathematics that adequately describes its use and role in the practice of science. A traditional empiricist account of mathematics falls to Frege's devastating criticisms in *Die Grundlagen der Arithmetik* ([1884]1980). Whereas the empiricist tendencies of the Logical Empiricists are simply inconsistent with Kant's understanding of mathematical propositions as synthetic *a priori* knowledge. Thus, if one has independent reasons to hold an empiricist epistemology, an account of logic and mathematics that removes it from the realm of knowledge proper overcomes a major hurdle to that position.

One important question is whether such a position on the status of logic and mathematics can recover the methodological character of logico-mathematical sentences. If we chose to see logic and mathematics not as a domain of knowledge in its own right, but as a set of formal auxiliaries, can we still account for the role that these sentences are customarily taken to have in our system of knowledge? That this was Carnap's primary investigative motivation comports with Carnap's most detailed discussions of logic and mathematics.

In *Foundations of Logic and Mathematics* Carnap makes statements in his opening remarks that can again be construed as traditionally foundational:

> However, logic and mathematics not only supply rules for transformation of factual sentences but they themselves contain sentences of a different, non-factual kind. Therefore, we shall have to deal with the question of the nature of logical and mathematical theorems. It will become clear that they do not possess any factual content. If we call them true, then another kind of truth is meant, one not dependent upon facts. (Carnap, 1939, p. 2)

These seem to be questions and assertions of a traditional foundational sort about the nature of logic and mathematics. Moreover, they appear to be question-begging in just the way we saw Gödel suppose in §2.1. However, they are embedded in a discussion about the *applicability* of mathematics—its function in the language and practice of science. To continue the passage:

> A theorem of mathematics is not tested like a theorem of physics, by deriving more and more predictions with its help and then comparing

> them with the results of observations. But what else is the basis of their validity? *We shall try to answer these questions by examining how the theorems of logic and mathematics are used in the context of empirical science.* (Ibid. My emphasis.)

So again, Carnap's approach toward understanding the nature of logic and mathematics involves examining its use. In other words, he asks: What properties must an account of logic and mathematics recover if it is to adequately explain the role that logic and mathematics play in the sciences?

Foundations, Choice, and Applicability

In the first place, Carnap supposes that any reconstruction of mathematics should recover the methodological attitude that mathematicians themselves apply in their investigations. In §17 of *Logical Syntax*, after presenting the Principle of Tolerance, Carnap states:

> The tolerant attitude here suggested is, as far as special mathematical calculi are concerned, the attitude which is tacitly shared by the majority of mathematicians. (p. 52)[33]

So Carnap is of the opinion that most working mathematicians abide a tolerant practice—that they can investigate any mathematical structure whatever, investigate the consequences of any set of postulates, so long as their assumptions are stated clearly. So Carnap's reconstruction of mathematics as sets of formal auxiliaries, and so the applicability of the Principle of Tolerance in this domain, seems modeled at least in part upon actual mathematical practice.

As we have noted above, Carnap also considers recovering an account of the applicability of mathematics in the empirical sciences to be of key importance. This is further evidenced by Carnap's discussion of the foundations of mathematics in §84 of *Logical Syntax*. To open this section, Carnap asks a question: "*What should a logical foundation of mathematics achieve?*" (p. 325). The answer is that it must be able to explain how inferences and calculations can be made with the empirical sentences of the special sciences.

To this end Carnap introduces the foundational controversy between the logicism of Frege and Russell, and the "formalism" of Hilbert and Bernays (which

[33]Carnap here cites mathematician Karl Menger as an important promoter of the idea that mathematicians will vary their working assumptions, and that there is nothing wrong with this as long as those assumptions are clearly stated.

we have been calling 'finitism'). We have already briefly examined each of these schools in the previous interlude. For Carnap the primary difference between them is that the logicists were interested to offer an account of the meaning of the logico-mathematical symbols along with the development of a suitable logico-mathematical calculus. These meanings were established by reducing mathematical terms to logical terms via explicit definition, and deriving all mathematical theorems from purely logical axioms. Alternatively, the finitists were interested only to set up a logico-mathematical calculus and to determine its properties, especially its consistency.

Recall that Carnap's own program of logical syntax was strongly influenced by Hilbert's meta-mathematical techniques. However, Carnap argues that the construction of a logico-mathematical calculus is by itself insufficient as a foundation for mathematics. This is because:

> this calculus does not contain all the sentences which contain mathematical symbols and which are relevant for science, namely those sentences which are concerned with the *application of mathematics*, i.e. synthetic descriptive sentences with mathematical symbols. (Ibid., p. 326. Original emphasis.)

Carnap also argues that the logicists' focus on the purely technical *reduction* of mathematical to logical symbols as a way to ground the former's meaning turns out to be of little concern. Mathematical research has shown that we can construct a calculus either with logical and mathematical symbols together as primitive (as with LII), or by more traditional logicist means.

On the other hand, providing a logical interpretation of mathematical symbols within the context of a language including also descriptive, synthetic sentences is considered essential for Carnap, since by these means we can represent and explain the use of mathematical sentences in the sciences. A pure logico-mathematical calculus does not by itself allow for the derivation of 'Two people beamed down to the planet' from the sentence 'Kirk and Spock beamed down to the planet, and no one else did'. The Frege-Russell definition of the number '2' as the class of all pair-classes, included in a language along with the requisite descriptive expressions, explains how such inferences are possible.

So unsurprisingly, Carnap sees any controversy between finitists and logicists as a mistake. Both programs contribute something to our understanding of the application of mathematics in the sciences:

> Only in this way is the application of mathematics, i.e. calculation with

> numbers of empirical objects and with measures of empirical magnitudes, rendered possible and systematized. *A structure of this kind fulfils, simultaneously, the demands of both formalism and logicism.* For, on the one hand, the procedure is a purely formal one, and on the other, the meaning of the mathematical symbols is established and thereby the application of mathematics in actual science is made possible, namely, by *the inclusion of the mathematical calculus in the total language*. (Ibid., pp. 326–327. Original emphasis.)[34]

More important for our purposes is that in Carnap's mind the question of applicability is one of the most important with regard to foundations, and the philosophical problem is to recover applicability in a systematic way. His proposed answer to the question is an understanding of logico-mathematical sentences as conventionally chosen syntactical rules that act as purely formal auxiliaries. And he shows that account adequate in this regard.

Thus we see that Carnap's understanding of logic and mathematics as a set of conventionally chosen formal auxiliaries provides a solution to this problem of explication, which was to recover the role of mathematics in the sciences. His account is based upon: (i) An observance of the practice of working mathematicians; and (ii) A prior understanding of the distinct methodological roles played by mathematical and empirical sentences in the sciences, including their interaction in scientific practice (i.e., applicability). Traditional foundational concerns seem to have little or nothing to do with the motivations or aims of his philosophy of mathematics.

Formal and Factual Sciences

We can bolster the claim that Carnap's philosophy of mathematics is motivated by attempts to address primarily methodological rather than epistemic concerns. In this respect it will be worthwhile to examine Carnap's rarely-cited article, "Formal and Factual Science" ([1934]1953). The article aims to describe the project of the Logic of Science by means of an example. The example Carnap chooses is to clarify the relationship between the formal and the factual sciences. He begins with some

[34] Carnap offers the same assessment of this controversy, and the focus on applicability, in Carnap (1939, §20) and Carnap (1963, pp. 48–49). Quoting the latter: "Frege had already strongly emphasized that the foundation problems of mathematics can only be solved if we look not solely at pure mathematics but also at the use of mathematical concepts in factual sentences. He had found his explication of cardinal numbers by asking himself the question: What does "five" mean in contexts like "I have five fingers on my right hand"? Since Schlick and I came to philosophy from physics, we looked at mathematics always from the point of view of its application in empirical science."

remarks stressing that it is the logical, rather than the psychological, relationship which is of interest:

> Only the question concerning the *logical* relations between the two fields, that is, the difference in the syntactical character of their statements and statement-systems is our concern. While in their psychological character there is only a difference of degree and not kind between the two fields, from a logical point of view a precise and fundamental difference can be demonstrated. This is based upon the syntactical *difference between analytic and synthetic statements.* (p. 123. Original emphasis.)[35]

So Carnap plans to clarify the distinction between the formal and factual sciences by using the tools of logical syntax to analyze the characteristic logical properties of the sentences of each field. We could say that Carnap aims to provide an *explication* of this distinction, with the notions of 'analytic' and 'synthetic' sentences acting as *explicata.* As we have seen, this is Carnap's usual method and precisely what he does in *Logical Syntax.*

Of particular interest in the article is Carnap's description of the methodological role of each type of sentence in scientific practice: Of particular interest in the article is Carnap's description of the methodological role of each type of sentence in scientific practice:

> Science uses synthetic and analytic statements in the following manner. The factual sciences establish synthetic statements, e.g., singular statements for the description of observable facts or general statements which are introduced as hypotheses and used tentatively. From the statements thus established the scientists try to derive other synthetic statements, in order, for instance, to make predictions concerning the future. The analytic statements served in an auxiliary function for these inferential operations. (p. 127)[36]

This is similar to how we have been describing Carnap's understanding of science thus far, but the point here is that Carnap is *assuming* this structure at the outset.

[35]In the next chapter we will see that a Deflationary interpretation is required to deny that an unreconstructed, or pre-theoretic, division between analytic and synthetic sentences can be made sense of in the context of Carnap's program. This is essentially the Deflationary response to Gödel's circularity objection.

[36]Cf. Carnap (1939, p. 35): "The chief function of a logical calculus in its application to science is not to furnish logical theorems, i.e., L-true sentences, but to guide the deduction of factual conclusions from factual premisses. (In most presentations of logical systems the first point, the proofs, is overemphasized; the second, the derivations, neglected.)"

In other words, he takes this as an accurate, if informal, methodological characterization of scientific practice, and this both motivates and delimits the nature of his explication of the sentences and concepts of science. It is this characterization of the sentences of science that must be recovered by any methodological reconstruction of the language of science.

Indeed, Carnap often stresses that a clear distinction between the formal and factual components of science—between the logical and empirical, between the analytic and synthetic—is a requirement of any successful methodological analysis of science. Consider, for example, these remarks from his autobiography:

> To me it had always seemed to be one of the most important tasks to explicate this distinction [between formal and factual truth], in other words, to construct a definition of logical truth or analyticity. (Carnap, 1963, p. 63)

Carnap takes the division as vital because he considers it a genuine and significant insight into the nature of our knowledge and the character of our scientific theories. Consider one of Carnap's favourite examples of the fruitfulness of this distinction, Einstein's development of the theory of relativity:

> In my opinion, a sharp analytic-synthetic distinction is of supreme importance for the philosophy of science. The theory of relativity, for example, could not have been developed if Einstein had not realized that the structure of physical space and time cannot be determined without physical tests. He saw clearly the sharp dividing line that must always be kept in mind between pure mathematics, with its many types of logically consistent geometries, and physics, in which only experiment and observation can determine which geometries can be applied most usefully to the physical world. (Carnap, [1966]1974, p. 257)

Carnap takes examples like this as evidence that the perceived distinction between the formal and the factual components of our knowledge is a genuine insight into the methodological and epistemic structure of our scientific theories.

So Carnap's philosophy of mathematics has as its primary goal to recover an account of the applicability of mathematics in the empirical sciences. This goal is motivated by Carnap's concern to explain the nature of logic and mathematics through its use in the practice of science, and the methodological insight that distinct roles are played by the formal and factual components of our theories.[37] The Logical Empiricist account of logico-mathematical sentences seems to allow Carnap to recover these characteristics. Insofar as the circularity objections attack Carnap's program on the grounds that it has failed to offer an epistemological account of how we grasp or justify mathematical truths, or an ontological or semantic account as to how logico-mathematical truth is generated, this was simply not Carnap's goal. His concerns were of an entirely different, methodological sort.

2.4 Response: Explication as Foundations

As indicated at the outset of this chapter, it is worth noting Awodey & Carus' (2004) very astute observation that Gödel's particular circularity objection is unsound. Recall that Gödel's complaint was that Carnap must prove that a candidate syntactical rule is consistent in order that it can do the epistemic work of replacing Gödel's own appeal to mathematical intuition. But since such a proof requires mathematical notions stronger than the rule, Carnap is stuck using stronger mathematical principles than those he was attempting to show syntactical, and his account collapses. Awodey & Carus note that Carnap need not *prove* that a candidate syntactical rule be consistent; it need only *be the case that the rule actually is* consistent. Gödel's call for a proof of consistency goes too far, since all of our syntactical rules may very well be consistent even if this cannot be proven. The situation is analogous to our acceptance of the consistency of Peano Arithmetic. Thanks to the incompleteness theorems, almost any proof of the consistency of arithmetic will inevitably be uninformative. But this technical situation alone is not usually taken to put into jeopardy our confidence in the consistency of the theory.

However, there are actually two concerns with this reply. First off, this response

[37] Again from Carnap's autobiography: "From [Frege's] analysis I gained the conviction that knowledge in mathematics is analytic in the general sense that it has essentially the same nature as knowledge in logic. [...] Furthermore the following conception, which derives essentially from Frege, seemed to me of paramount importance: It is the task of logic and of mathematics within the total system of knowledge to supply the forms of concepts, statements, and inferences, forms which are then applicable everywhere, hence also to non-logical knowledge. It follows from these considerations that the nature of logic and mathematics can be clearly understood only if close attention is given to their application in non-logical fields, especially in empirical science. Although the greater part of my work belongs to the fields of pure logic and the logical foundations of mathematics, nevertheless great weight is given in my thinking to the application of logic to non-logical knowledge. This point of view is an important factor in the motivation for some of my philosophical positions, for example, for the choice of forms of languages, for my emphasis on the fundamental distinction between logical and non-logical knowledge." (Carnap, 1963, pp. 12–13)

does not respect the Principle of Tolerance, in the sense that it does not acknowledge that we may want to investigate the consequences of choosing a framework with inconsistent syntactical rules. This being the case, Gödel would argue that we have moved out of the realm of mathematics altogether, since our supposed "L-Rules" would imply ostensibly factual sentences. To the letter of Carnap's program however, this is not the case, since by definition every sentence of the language would be analytic, and so the framework would have no factual sentences. Certainly this is an odd framework, and it is certainly unworkable as a total language of science (since it would be wildly inadequate for explicating our empirical theories), but it should not be ruled out from the beginning, especially as the explication of some pure logico-mathematical theories (such as paraconsistent logics).

More importantly, this response is predicated upon the peculiarities of Gödel's criticism to the first thesis of the syntactic viewpoint. The more general concern that we have abstracted from the circularity objections is still to be addressed. The fundamental worry is that Carnap's account of mathematics as a purely formal set of syntactical rules appeals to informal mathematical concepts in its justification or explanation, thus undermining the thesis. But we have seen that Carnap's understanding of logico-mathematical sentences as formal auxiliaries is not a foundational thesis in the traditional sense. Rather, it is the result of a methodological analysis of science and the role that logic and mathematics seem to play in our total system of knowledge. In other words, Carnap's mathematical conventionalism is itself an explication of the distinction that he recognizes between the formal and factual aspects of science. In order to support this explication as successful, Carnap must show that his conventionalist account of logic and mathematics recovers our understanding of the role that mathematics plays in the total language of science. Carnap takes this to be an account of the applicability of logico-mathematical sentences, and certain characteristics that equate to those sentences being utilized as formal auxiliaries. He then argues that his treatment of logic and mathematics recovers these characteristics, and so is successful in this regard.

Why is this not just an appeal to verificationism? Recall in §2.1.2 above that Gödel's second criticism involves calling into question the Logical Empiricist notion of 'content'. Gödel objects that the Logical Empiricists rule out from the beginning any definition of the concept that is not limited to 'empirical content', thus begging the question.[38] I suggested that while traditional Logical Empiricists such as Hahn

[38] Note that given the response of Awodey & Carus, this objection is also unsound because it depends upon the success of Gödel's first objection. However, in the same way that the general

and Schlick do indeed assume verificationism, that Carnap in fact shows that mathematics involves nothing that ought to be construed as content. Carnap does this by *transforming* foundational questions about the nature of logic and mathematics into problems of explication. The question then is not to explain the *nature* of logico-mathematical truth in some metaphysical sense, or our epistemic access to it. Carnap *presumes* the veracity of logico-mathematical concepts and results, because these are matters of science, not philosophy. Rather, Carnap sees the philosophical problem as one of developing a more rigorous language that recovers the role that logic and mathematics play in our system of knowledge.

So Carnap does not just presume that logic and mathematics is analytic, but provides an informative analysis of logico-mathematical truth and related concepts which suggests that treating logic and mathematics as purely formal recovers its essential characteristics. This analysis does indeed presume the concepts and methods of mathematics itself, but this is part of the nature of providing an explication. The concept 'Warmer' does not disappear once we have offered a rigorization of it with the concept 'Temperature'. Moreover, in order to demonstrate that our *explicatum* is successful, we must rely at least in part upon our confidence in the *explicandum*.

It is important to recognize that Carnap's methodological characterization of the sciences does quite a bit of work in his program, and it is a point upon which his suggestion that philosophy be replaced by the Logic of Science (as the primarily formal explication of the concepts of science) depends. But insofar as Carnap's program is a recommendation to adopt a particular set of methods, it is resistant to foundational criticisms. And insofar as his mathematical conventionalism is an explication of a concept of logico-mathematical truth predicated upon this methodological characterization of the role of logic and mathematics in the sciences, he can virtuously appeal to the concepts and methods of mathematics to argue that his explication is successful. As we will see in the second interlude, any further metaphysical or epistemic questions Carnap regards as pseudo-questions.

In the next chapter we will revisit Gödel's criticisms as a means to understand the Deflationary reading in greater detail. As noted, the Deflationary response to these objections is quite different than the response developed herein. Examining this response will give us an opportunity to better understand the methodological role of the Principle of Tolerance in Carnap's program.

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idea of Gödel's first objection generalizes, the kernal of his second objection remains a difficulty outside of the context of Gödel's particular presentation.

Chapter 3

'Empirical Fact', Tolerance, and Conventionalism: A Reply to Goldfarb and Ricketts

It is impossible to argue against what professes to be insight, so long as it does not argue in its own favour.

BERTRAND RUSSELL ([1914]1952)

In this chapter we will examine the Deflationary interpretation in more detail, and I will argue that it is in several places either mistaken or uncharitable as an interpretation of Carnap's meta-philosophy. As mentioned in our first chapter, Goldfarb and Ricketts' reading seems partially motivated precisely because they believe it to furnish Carnap with a response to Gödel's criticisms—almost every paper in their series devotes a substantial amount of space to the treatment of this issue.[1] In any case, Gödel's understanding of Carnap's program affords an illuminating contrast for our own understanding of the Deflationary interpretation, since Goldfarb and Ricketts argue that he fundamentally misunderstands Carnap's aims and the radical nature of his project.

With regard to the other circularity objections, although Goldfarb and Ricketts do not treat them in a systematic way, the various concerns the authors do address over the course of their many papers advocating their reading strongly suggests how

[1]But see especially Goldfarb & Ricketts (1992), Ricketts (1994; 2007; 2009), and Goldfarb (1995*b*; 1996; 1997; 2009).

a deflationary Carnap would respond to these objections. One of these concerns is the intuitionist complaint noted in both our first and second chapters, that Carnap's meta-philosophy seems to beg the question against an intuitionist because the meta-logical investigation of various frameworks will almost necessarily presuppose mathematical notions stronger than any the intuitionist considers legitimate. We will see that the Deflationary interpretation and the interpretation we have developed thus far must approach this complaint in very different ways.

We will begin with a brief review of the main tenets of the Deflationary reading. As noted, Goldfarb and Ricketts' interpretation will become clearer through the discussion of their treatment of the circularity objections. Their responses lean heavily upon the Principle of Tolerance to deny that Carnap recognized any notions which "cut-across" distinct linguistic frameworks, notions such as 'empirical fact' and 'mathematical truth'. In §2 we will examine the Deflationary response to Gödel in detail and see that, contra the Deflationary reading, Carnap's program does indeed have a rather robust notion of 'empirical fact' that acts in exactly the way Goldfarb and Ricketts argue it must not. §3 argues that logocentrism is a rather bold interpretive thesis in the context of Carnap's program. This will lead us to §4, which discusses the Deflationary response to the other circularity objections. We will see that this response threatens to rob Carnap's program of much of its potential philosophical interest, fundamentally limiting its extensibility and denying Carnap the ability to offer any reason for its adoption. As suggested in our first chapter, Carnap's program becomes *philosophically inert* on the Deflationary reading. We will also observe that the Deflationary response to Gödel does not completely address his concerns. Finally, §5 will examine the role of the Principle of Tolerance in Carnap's meta-philosophy in more detail. We will conclude that the principle is licensed by Carnap's methodological analysis of the sciences rather than acting as the foundation of his program.

3.1 The Deflationary Interpretation

Recall from our first chapter that the Deflationary reading suggests that Carnap's program is grounded upon an insight into the primacy of our language as a precondition for inquiry and justification. Consider:

> Carnap's central notion is that of a language, or, in his later terminology, a linguistic framework. A linguistic framework provides the logical rela-

> tions of consequence and contradiction among propositions. *The fixing of these logical relations is a precondition for rational inquiry and discourse.* There are many alternative frameworks, many different logics of inference and inquiry. There can be no question of justifying one over another, since justification is an intra-framework notion; justification can proceed only given the logical relations that a framework provides. (Goldfarb, 1995*b*, pp. 326–327. My emphasis.)[2]

Now, I take no issue with the understanding of a linguistic framework as being a model—a rational reconstruction—of our methods of inquiry and discourse. As we have seen, this is presumably the point of Carnap's reconstructive program. My concern is the further interpretive claim that outside the auspices of some particular linguistic framework Carnap does not recognize any set of intuitive epistemic notions to which our formal reconstructions can be compared. According to the Deflationary reading, for Carnap concepts like 'justification', 'inference', 'empirical fact', or 'mathematical truth' are *constituted* by the choice of syntactical rules for one's framework, and so there is no notion of scientific inquiry or practice standing outside of this rigid formal structure.

We labeled this interpretive ascription made by the Deflationary reading Carnap's *logocentrism*, which I will repeat here:

> **Logocentrism:** Our choice of linguistic framework includes the choice of all epistemic standards for justification, truth (*viz.*, analyticity), and correctness, and therefore all such notions must be language-relative.

The consequence is that since each distinct framework will generate its own distinct set of epistemic notions, there can be no extra-framework notions which "cut-across" distinct linguistic frameworks, and so there can be no rational grounds for the categorical assertion or rejection of a linguistic framework, or for the ordering of a set of proposed frameworks according to some rational criteria. Any rational argument for the rejection of a framework would by definition ensue already from the confines of one's own particular framework, but this need-not impinge upon the very disparate notions of rationality and justification that may apply in some other language.

It follows on this reading that any choice between frameworks must be entirely a matter of our values and practical concerns—after all, the choice of a language is not

[2]In our discussion from the first chapter we quoted Goldfarb (1996, p. 225) making this same point. Cf. Goldfarb & Ricketts (1992, p. 65) and Ricketts (2007, p. 206).

something that can be "correct" or "incorrect"—and so the Principle of Tolerance has primacy of place in the Deflationary reading of Carnap's meta-philosophy. I will also repeat this principle here:

> [**Principle of Tolerance:**] *It is not our business to set up prohibitions, but to arrive at conventions.*
>
> [. . .]
>
> *In logic, there are no morals.* Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments. (*Logical Syntax*, pp. 51–52. Original emphasis.)

Tolerance suggests a logical pluralism: Our choice of logical language is not a matter of truth but instead a matter of convention. It also recommends that we state our methods clearly—in the form of syntactical rules—thus suggesting the formalized methods that Carnap favoured.

None of this is controversial with regard to Carnap interpretation. We have seen Carnap enact this process in *Logical Syntax* through the reconstruction of intuitionistic and classical mathematics. He does indeed suggest that our decision to utilize intuitionistic or classical reasoning in further scientific investigation is ultimately a matter of choice. The reconstructions of these positions in the form of linguistic frameworks LI and LII help to inform that choice by highlighting the formal properties of each language. However, the logocentric interpretation of Carnap's program takes this one step further by suggesting that this process is really all there is to his meta-philosophy. Because all of our epistemic notions are intra-framework, Carnap cannot provide rational criteria by which to determine that some particular framework acts as a more accurate reconstruction than some other, or give a rational justification for the adoption of some particular framework over another. Indeed, Carnap cannot argue for any kind of philosophical position at all. Conventionalism and empiricism are just particular frameworks that we might choose adopt on the Deflationary reading, since Carnap's program is "not based on any substantial theoretical commitments of its own." (Goldfarb, 1997, p. 61). Likewise, the methodological reforms encapsulated in the Principle of Tolerance are a suggestion, but not one for which we can provide rational arguments. "In a sense, it is not said, but only shown." (Ibid.)

As we will see below, on the Deflationary reading Carnap's program cannot be the explication of the concepts of science, since there can be no particular set which constitutes *the* concepts of science. Rather, Carnap's program amounts to the construction of independent, almost free-floating, linguistic frameworks that practicing scientists may choose to utilize or to ignore. Philosophical debates are still treated as a matter of choice between linguistic proposals, but because of the logocentric character of our concepts this procedure is extended to all inquiry and investigation. Since rationality and justification are intra-framework notions, the comparison of distinct frameworks, or the idea that distinct frameworks may be more or less adequate with regard to some informal scientific concept, fall completely under the methodological purview of Tolerance, that is, they are entirely matters of convention. So rather than trying to show that an explication is successful and offering reasons for the adoption of one *explicatum* over another, on the Deflationary interpretation such choices are a matter of our preferences and values, as in choosing between a set of Kuhnian paradigms.[3]

Furthermore, since a deflationary Carnap eschews the very idea that argument is appropriate in philosophy, he gives up the ability to recommend his position to any objector who does not immediately accept the Principle of Tolerance. The position also results in a troubling tension within Carnap's program, as Tolerance's call for clarity must ultimately remain unfulfilled due to circularity-style reasoning. But as we will see below, this tension is a consequence of the Deflationary interpretation.

3.2 The Deflationary Response to Gödel

3.2.1 Review: Gödel's Circularity Objection

Recall from the previous chapter that Gödel thinks of Carnap as attempting to *argue* for a philosophy of mathematics labeled the "syntactic viewpoint". This doctrine is comprised of two primary theses: (i) Mathematics is nothing other than the syntax of langauge; and (ii) Mathematical statements have no content. In other words, Gödel

[3]For Kuhn ([1962]1996) the standards of rationality are an important component of a scientific paradigm and so during a crisis of normal science, once we have abandoned the established paradigm and are groping for a replacement, the choice between paradigms must ultimately be a matter of preferences and values. Importantly, Kuhnian paradigms are *incommensurable*—they cannot be rationally compared because such standards as would be required for a rational comparison are intra-paradigm notions. The Deflationary interpretation of Carnap paints his meta-philosophy as striking similar to these Kuhnian doctrines. This comparison of a deflationary Carnap's program with Kuhn's derives from Norton's (1977, chp. 6) study of several earlier interpreters of Carnap.

takes Carnap to be arguing that we can justify mathematical concepts and truths by "reducing" them to purely formal syntactical rules. To this end, he characterizes Carnap's account of mathematics as consisting of our selecting some class of syntactical rules which act to define a consequence relation over the factual sentences that are representable in the language thus constituted. These factual sentences are then the synthetic sentences of the language, while the logico-mathematical sentences follow purely from the syntactical rules we have selected, and so are analytic.

Gödel's first criticism then follows from the observation that some choices of syntactical rules are not innocent, since presumably our logic and mathematics should be factually non-creative, that is, intuitively the truth-value of factual sentences should not be completely determined by a convention of language. Since an inconsistent rule entails every sentence, a minimum constraint on our choice of syntactical rules should be their demonstrable consistency. But since Carnap cannot prove the consistency of syntactical rules without presuming mathematical notions at least as strong, Carnap's attempted justification of mathematics is viciously circular. Gödel's argument against the second thesis of the syntactic viewpoint then follows from his first. Recall that Gödel argues the Logical Empiricsts are begging the question by presuming a notion of 'content' that excludes anything other than empirical content. But since Gödel takes himself to have shown that the first thesis of the syntactic viewpoint is incorrect, Carnap has no basis for restricting the notion of 'content' in this way; at least, he cannot presumptively rule out mathematical content as illegitimate.

Of course we have seen that Gödel's first argument here is unsound. The success of Carnap's program does not require that our syntactical rules be *demonstrably* consistent, only that they be consistent. More generally, I have argued that since Carnap is trying to furnish explications of various logico-mathematical concepts, and of the overall role of mathematics in the sciences, he is free to presume whatever mathematical concepts necessary. With regard to Gödel's second criticism, Carnap's explication of 'content' is supported by his methodological analysis of logic and mathematics and the recovery of its applicability in the sciences. Thus Carnap is not begging the question, but providing an informative analysis of the nature of logic and mathematics as purely formal.

3.2.2 Deflationary Response: No 'Empirical Facts'

Not surprisingly, the Deflationary response to Gödel instead appeals straightforwardly to Carnap's logocentrism. Goldfarb and Ricketts argue that Gödel fails to take account of the methodological priority of our linguistic choices. Notice that Gödel supposes Carnap to assume a language-independent realm of facts, which we then represent in a particular linguistic framework as a class of synthetic sentences. But according to the logocentric thesis of the Deflationary view, any appeal to a language-transcendent realm is a simple misunderstanding of Carnap's program.

Goldfarb and Ricketts correctly point out that a class of empirical (*viz.*, synthetic) sentences is determined within a particular framework *at the same time* that its logico-mathematical rules are selected. As discussed in the Logico-Mathematical Interlude, it is in virtue of laying down syntactical rules (including formation rules) that the classes of analytic and synthetic sentences of a formal language are determined in the first place. Hence there is no possibility of the consequences of an L-Rule intersecting with some set of synthetic sentences—those sentences could not then be synthetic. Where the Deflationary interpretation goes one step further is to suggest that given a framework wherein sentences that intuitively represent empirical facts are instead analytic, Carnap's logocentrism dictates that it makes no sense to argue that while those sentences are not synthetic within the framework, they *should* be because they are correlated with actual empirical facts.

Gödel's argument presumes this correlation between the statement of sentences representing empirical facts considered in our informal language of science and sentences within our formal reconstructions. According to Goldfarb and Ricketts this is a mistake, since Carnap's program simply does not allow for a notion of 'empirical fact' prior to the construction of the language:

> Gödel's argument, if applied in the setting of *Logical Syntax*, requires a domain of empirical fact conceived as transcending or cutting across different linguistic frameworks. However, as the Principle of Tolerance indicates, it is central to the metaphysics of *Logical Syntax* that any such language-transcendence be rejected. Rather, the notion of empirical fact is given by way of the distinction between what follows from the rules of a particular language and what does not, so that *different languages establish different domains of fact.* (Goldfarb, 1996, p. 227. My emphasis.)[4]

Again, what counts as an empirical fact is relative to and determined by the choice of syntactical rules for a language. To assume that there is some domain of facts

[4]Cf. Goldfarb & Ricketts (1992, p. 69) and Goldfarb (1995*b*, p. 328) for earlier iterations of this same argument.

apart from or prior to the establishment of the epistemic standards of a language is apparently to misconstrue Carnap's idea that we cannot make claims about the world directly and absolutely. This is what Tolerance tells us: All we can do is propose a set of syntactical rules from which a class of synthetic sentences may follow, these are then representative of the empirical facts as established by that linguistic framework. There is simply no further question to be asked about the "correctness" of our linguistic framework, that is, about whether our framework has accurately characterized some language-independent domain.

In summary, the Deflationary position concludes that Gödel's argument against thesis (i) of the syntactic viewpoint is unsound, but for a different reason than Awodey & Carus (2004). According to the Deflationary reading, the idea that there are language-transcendent facts, and so the idea that we must be careful to ensure that our *prima facie* logico-mathematical choices do not mistakenly attribute a truth-value to the sentences meant to represent those facts, are things which Carnap's meta-philosophy simply rules out from the beginning.

3.2.3 'Empirical Fact' in Carnap's Meta-Philosophy

I will now argue that insofar as the Deflationary reading takes Carnap's program as systematically rejecting all language-transcendent notions, it is simply incorrect. While Goldfarb and Ricketts argue that Gödel is mistaken in taking Carnap to assume a realm of pre-existing, language-transcendent facts, recall the article that Gödel cites in support of this reading:

> Science uses synthetic and analytic statements in the following manner. The factual sciences establish synthetic statements, e.g., singular statements for the description of observable facts or general statements which are introduced as hypotheses and used tentatively. From the statements thus established the scientists try to derive other synthetic statements, in order, for instance, to make predictions concerning the future. The analytic statements served in an auxiliary function for these inferential operations. (Carnap, [1934]1953, p. 127)

This is Carnap's "Formal and Factual Science", which we highlighted in the previous chapter during our discussion of Carnap's methodological analysis of the sciences. Carnap is here discussing the role of analytic and synthetic statements in the practice of science. Recall that this was in the context of Carnap's offering an example of an explication—he is suggesting that 'analytic' and 'synthetic' successfully characterize the informal distinction we find in practice between the formal and factual sciences. According to Carnap, then, we *begin* with the factual sciences independently establishing a realm of facts construed as synthetic sentences. What Carnap does not say is that science begins with the selection of specific syntactical rules for a formal framework from which the synthetic sentences are meant to flow, because that kind of a structure is a *reconstruction*—an artificial characterization or model of the informal practices and languages used by science.

Carnap makes just this point in his reply to Robert Cohen's contribution to the Carnap-Schilpp volume:

> To Cohen's criticism of *conventionalism* I should like to say that a pure conventionalism (like that of Hugo Dingler, for example) was never maintained by any adherent of logical empiricism, nor by Mach or Poincaré. [...] Cohen believes that my so-called principle of tolerance in the logical syntax contains a "doctrine of conventionality-chosen basic-truths". But this is not the case. *The principle referred only to the free choice of the structure of the language, and not to the content of synthetic sentences.* I emphasized the non-conventional, objective component in the knowledge of facts, e.g., in [Carnap (1949)]. There I also pointed out that *the first operation in the testing of synthetic statements is the confrontation of the statement with observed facts.* Thereby I took a position clearly opposed to a pure conventionalism and to any coherence theory of truth. (Carnap, 1963, p. 864. My emphasis.)

While the *structure* of our linguistic representation of empirical facts is amenable to Tolerance, the *content* of those sentences is not. This is the point that we made with regard to protocol-sentences in chapter 1.

Recall that in §82 of *Logical Syntax* Carnap presents a relatively simple model of scientific confirmation. Protocol-sentences in a given linguistic framework encode basic observations. These can then be checked for equivalence to other protocol-sentences derived from the P-Rules of that framework in what amounts to a basic hypothetico-deductive process. So far this can all be interpreted as occurring under the purview of a linguistic framework—say LII supplemented with certain P-Rules—and so presumably different frameworks will establish different sets of protocol-sentences, and there may well be no possibility of some pre-linguistic realm of 'observations' or 'facts' which constrain or otherwise limit our choice of framework. This is just Goldfarb and Ricketts' response to Gödel. However, this is not what Carnap suggests:

> Syntactical rules will have to be stated concerning the forms which the *protocol-sentences*, by means of which the results of observation are expressed, may take. (*Logical Syntax*, p. 317. Original emphasis.)

Carnap then adds in square brackets:

> On the other hand, *it is not the task of syntax to determine which sentences of the established protocol form are to be actually laid down as protocol-sentences*, for 'true' and 'false' are not syntactical terms; the statement of the protocol-sentences is the affair of the physicist who is observing and making protocols. (Ibid. My emphasis.)

In other words, we may choose to talk (i.e., set up our language) as if we are speaking about our own subjective phenomena, or as if we are speaking about physical objects,[5] but such formulations should have nothing to do with the factual content of the sentences representing our protocols. To extrapolate: If our formal language is to be an *adequate reconstruction* of an informal empirical theory, then the synthetic sentences of the language whose truth-value is supposed settled will need to correspond to the observational basis of the informal theory being subjected to reconstruction.

This extrapolation finds support in the 1949 article to which Carnap refers Cohen in his reply quoted above. The article in question is Carnap's "Truth and Confirmation", and he there aims to explain the distinction between the concepts in the title, arguing that distinguishing between a *definition* of truth and a *criterion* of confirmation is of vital importance to understanding science. The most interesting part of the article for our purposes is Carnap's analysis of the notion of the "confirmation of directly testable statements" (pp. 124–125). A "directly testable statement" is

[5] Cf. Carnap ([1932]1987). His discussion there is quite close to his later discussion in *Logical Syntax*, but he stresses (p. 469) that "absolutism" with regard to our protocols is methodologically unfavourable. In other words, the idea of an unquestionable "given" that is then represented by protocol-sentences should be rejected. Rather, we may discard certain protocols if they fail to comport with the bulk of our already established protocol-sentences (for example). In his "Intellectual Autobiography", Carnap acknowledges the influence of Karl Popper in helping him to recognize this: "With some of his conceptions we could not agree, but some positively influenced my thinking and that of others in the Circle, especially Feigl. This is the case, for example, with Popper's views on protocol sentences, i.e., those sentences which are confirmed by observations more directly than others and serve as confirmation basis for others. Popper emphasized that no sentence could be regarded as an "absolute" protocol sentence, but that every sentence might be revised under certain circumstances." (Carnap, 1963, pp. 31–32).

one that can be strongly enough confirmed or infirmed to outright accept or reject on the basis of only one or very few observations—he offers: "There is a key on my desk" as an example.

Now, the confirmation of such statements involves two procedures for Carnap: (i) "*Confrontation of a statement with observation*"; and (ii) "*Confrontation of a statement with previously accepted statements*" (Ibid. Original emphasis.). The details of these procedures are as expected. With respect to the first procedure, Carnap asserts that I accept the statement "There is a key on my desk" because "I acknowledge it as highly confirmed on the basis of my visual and, possibly, tactile observations." (Ibid.) Notice that Carnap is here acknowledging base observations outside the context of a formalized framework—they are not *linguistic* items, but genuine or tangible elements of our experience:

> Ordinarily no definite rules are expressly stipulated as to how a statement may or must be formulated when certain observations have been made. Children learn the use of common language, and thereby the correct performance of the operation described [i.e, the process of confronting a statement with observations], through practice, imitation, and usually without the benefit of rules. These rules, however, could be specified. (p. 125)

This point is vital to our rejection of Goldfarb and Ricketts interpretation. Carnap is using the notion of an 'observation' here in a similar way as in his discussion of protocol-sentences above, and in precisely the way that Goldfarb and Ricketts suppose Carnap cannot—as representing a language-transcendent notion of 'empirical fact'. This is made even clearer by Carnap's discussion of a then-ongoing controversy:

> There has been a good deal of dispute as to whether in the procedure of scientific testing *statements must be compared with facts* or as to whether such comparison be unnecessary, if not impossible. If 'comparison of statement with fact' means the procedure which we called the first operation [i.e., the key example above] then **it must be admitted that this procedure is not only possible, but even indispensable for scientific testing**. (Ibid. Original emphasis, my bold.)

The worry is exactly the point at issue with regard to the Deflationary interpretation: Whether there is something extra-linguistic and language-transcendent to which our sentences might be compared. We see Carnap here asserting that some such procedure is required by the practice of science.

There are, of course, several complications. Carnap notes that the phrase 'a *comparison* of statement and fact' is not completely accurate as a description of the relationship between statements and facts. He prefers the term 'confrontation' because this suggests the investigation of whether a statement properly "fits" a fact rather than the idea that two objects should be compared by some common property (e.g., being red, or being words, etc.). More important for Carnap, he expresses worry that the term 'comparison' with regard to facts "easily tempts one into the absolutistic view according to which we are said to search for an absolute reality whose nature is assumed as fixed independently of the language chosen for its description." (Carnap, 1949, p. 126)

Does this not imply the Deflationary reading? I would argue not. Carnap certainly does acknowledge the role of our language in shaping our description—and so our understanding—of the world. Here he is saying as much:

> The answer to a question concerning reality however depends *not only upon that 'reality', or upon the facts* but also upon the structure (and the set of concepts) of the language used for the description. In translating one language into another the factual content of an empirical statement cannot always be preserved unchanged. Such changes are inevitable if the structures of the two languages differ in essential points. (Ibid. My emphasis.)

But Carnap's point here is not that there is no world outside the context of a particular linguistic framework, that there cannot be any sense of 'empirical fact' apart from some choice in the form of our protocol-sentences. He acknowledges a notion of 'fact', it is simply that this is not the *only* relevant factor in our scientific understanding. That we can formulate adequate and complete analytic-synthetic distinctions in our reconstructions is evidence for this.[6] To think that Carnap can accept no notion of an external world apart from our linguistic choices would be to make Carnap into a relativist, into a coherentist about truth. But we saw him reject this attribution above, in his response to Cohen.

[6] In Carnap ([1966]1974, chps. 27–28) he presents his mature Ramsey-sentence reconstruction of science. The reconstruction is specifically tailored to capture an analytic-synthetic distinction in both the observation and the theoretical language. Cf. Demopoulos (2013, chps. 4, 6–7) for an extremely insightful analysis of Carnap's ideas here and their limitations.

The example Carnap offers in his discussion of 'reality' is the languages of classical and modern physics. According to Carnap, certain statements of modern physics (especially those concerning newly-introduced concepts like 'wave-function') cannot be translated into the language of classical physics. Similarly, as a result of our adoption of the language of modern physics, certain classical statements must be rejected. This is because the classical language lacks the means of expression necessary for the newly introduced concepts, and lacks or is incompatible with the required inferential structures. But, again, this is not to say that the matter is a mere choice of language. Carnap offers a qualification immediately after discussing the complications of using the term 'comparison' in describing the relation of statements and facts:

> The scruples here advanced regarding the assertion that statements are to be compared with facts (or reality) were directed not so much against its content but rather against its form. The assertion is not false—if only it is interpreted in the manner indicated—but formulated in a potentially misleading fashion. Hence, one must not, in repudiating the assertion, replace it by its denial: "Statements cannot be compared with facts (or with reality)"; for this negative formulation is as much open to objection as the original affirmative one. In repudiating the formulation one must take care not to reject the procedure which was presumably intended, viz., the confrontation with observation. (Carnap, 1949, p. 126)

So it seems that Gödel interpreted Carnap in the way he did because this is more or less what Carnap says.

In alternately suggesting that Carnap has no language-transcendent notion of 'empirical fact', Goldfarb and Ricketts cut any ties that a formal language can have to the unreconstructed world, to actual observation. Indeed, as we noted in our first chapter, in several places throughout their series Goldfarb and Ricketts must actively explain away instances of Carnap drawing distinctions and speaking informally, seemingly outside the context of any particular linguistic framework. For example, they quote a remark from §14 of *Logical Syntax* that seems to indicate Carnap making an analytic/synthetic distinction in an informal way. The passage comes immediately after Carnap defines the consequence relation for LI, and is naturally interpreted as him motivating the definitions he has just constructed. I repeat the passage here:

> In material interpretation, an analytic sentence is absolutely true whatever the empirical facts may be. Hence, it does not state anything about

> facts. [...] A synthetic sentence is sometimes true—namely, when certain facts exist—and sometimes false; hence it says something as to what facts exist. *Synthetic sentences* are the *genuine statements about reality.* (*Logical Syntax*, p. 41. Original emphasis.)

I will also quote Goldfarb and Ricketts' take on this passage:

> However, there is a fundamental problem with Carnap's remark if it is taken as an intuitive basis of the analytic-synthetic distinction. If put in that role, it must rely on a framework-transcendent notion of fact or possible fact, and thus, as we saw in connection with Gödel's criticism of conventionalism, it would bespeak a view that is inconsonant with the Principle of Tolerance. For Carnap, there can be no general way of conceiving sentences as answerable to facts. Hence, this intuitive way of drawing the distinction should be discarded. (Goldfarb & Ricketts, 1992, pp. 73–74)[7]

Alternatively, we argued in our previous chapter that Carnap takes such informal characterizations as the basis for his formal explications—he is doing his best to characterize the *explicandum*. Furthermore, we saw Carnap utilize a notion of 'facts' above, in his discussion of confirmation.

So there seems to be a *lacuna* in the Deflationary argument between the Principle of Tolerance as a methodological principle in application to choices between reconstructions, and the espousal of a total logocentrism. Certainly Tolerance asserts that we should be logical pluralists, but there is a long way to go between that assertion and the idea that there are no language-transcendent notions at all. Carnap's program is not just a matter of forwarding linguistic proposals—the actual informal practices of science need to be taken into account, and this is what serves as the language-transcendent basis for our formal reconstructions.

3.3 The Priority of Logocentrism

We have seen that Goldfarb and Ricketts take Tolerance to be the most important element of Carnap's program. Appeal to this principle allows Carnap to deflate or ignore traditional philosophical arguments, as well as transforming the practice of

[7] Cf. Ricketts (1994, pp. 179–180).

philosophy into the presumably more fruitful and rigorous act of proposing or investigating linguistic frameworks for use in science. In chapter 1 we saw—and we will elaborate on this point below—that even Carnap's mathematical conventionalism and empiricism are licensed entirely by an appeal to Tolerance on the Deflationary view. Of course, as a consequence of the all-encompassing nature of Tolerance, on the Deflationary interpretation Carnap's program should itself be read *not as an assertion about the correct way to practice philosophy*, but as an attitude toward languages which expresses these methods and goals.

Despite this fundamental place for Tolerance on their reading, it is perhaps more accurate to say that on the Deflationary interpretation the Principle of Tolerance is actually a *consequence* of Carnap's supposed recognition of the logocentric character of our epistemic methods and standards. In other words, I think Goldfarb and Ricketts take logocentrism to be methodologically prior to Tolerance.[8] Consider Goldfarb's statements quoted above: For Carnap the selection of a linguistic framework (*viz.*, formal language) is a *precondition* for inquiry and discourse. So according to the Deflationary interpretation, justification only makes sense in the context of a linguistic framework with explicit and well-defined rules of formation and consequence to which one can appeal during a dispute or investigation. The plurality of languages amongst which we might choose is thus a consequence of the fact that there are different sets of constitutive principles which we might select as the basis for our language, and no possibility of ordering our preferences toward one particular set of principles over another according to some rational criteria—any such criteria, according to Goldfarb, presupposes already a choice of language.

This dependency is more clearly displayed in §II of Ricketts' article "Frege, Carnap, and Quine: Continuities and Discontinuities" (2004), wherein Ricketts traces the influence of Frege's philosophy of logic on Carnap. What is at issue is the transition from Frege's universalist understanding of logical laws as *the* laws of thought to Carnap's pluralistic conception. Given that Frege thought of the axioms of his *Begriffsschrift* as having a regulative role,"articulat[ing] the most fundamental standards for validity and consistency in thinking" (Quoted in Ricketts, 2004, p. 190),[9]

[8]This point was recognized while in conversation with Steve Bland. As with my interpretation of Carnap, my understanding of the Deflationary reading owes a great deal to our conversations.

[9]Recall that we touched upon Frege's understanding of logic as distinct from Carnap's in the Logico-Mathematical Interlude. Notice that Frege's way of construing logic blocks objections like Quine's ([1935]1976) toward a conventionalist understanding of logic. Indeed, for Frege the best we can do in explaining a formal system which codifies the laws of logic is to provide informal "hints" as to how to correctly utilize the system.

Ricketts questions how a pluralism in logic might be possible:

> From the perspective of this understanding of logic's regulative role, it makes no sense to represent the adoption of a logic, as Carnap does, as a choice from "an open ocean of free possibilities." How is Carnap's attitude of tolerance towards logic itself supposed to make sense? (Ibid.)

Ricketts proposes that the first step for Carnap toward his own unique views was the rejection of Frege's universalist conception of logic. This rejection was motivated by the divergent views on fundamental logical methods that developed during the 1920s. While Frege could rest easy (before 1902) with a straightforward appeal to his axioms as clear and self-evident truths, in light of work by Brouwer, Weyl, Poincaré, etc. by the time of *Logical Syntax* such an appeal seemed naïve at best. But Carnap saw the ensuing debates in foundations as reminiscent of the fruitless metaphysical wrangling that characterizes the history of philosophy. So rather than argue in one direction or another, Carnap proposes the meta-logical investigation of calculi that capture each possible option.

This approach, according to Ricketts, requires Carnap to further give up Frege's "assumption of a common store of logically interrelated thoughts expressed by the sentences of colloquial language and perspicuously expressible by sentences couched in the framework of begriffsschrift." (Ibid., p. 191) In other words, Carnap *must* eschew any "overarching notion of content to give his syntactic investigations their application to actual or hypothetical languages for science." (Ibid.) Thus the immediate connection between formal logic and the actual, colloquial language of science—alway presupposed as the point of the enterprise by Frege—is lost to Carnap, since the purely formal sentences of a calculus and the colloquial language used by scientists are no longer seen to share any content. Instead, this connection is drawn by establishing a correlation between the expressions and rules of a formal language and the linguistic behaviour of a group of colloquial speakers. But of course there are many distinct formal languages for which such a coordination can be established with any given colloquial language. Ricketts concludes:

> We can now appreciate the depth of Carnap's rejection of Frege's conception of a thought as that for which the question of truth arises. Linguistic behavior is, so to speak, in itself logically amorphous. *We bring logic to it by coordinating a calculus with it. The scientific philosopher, the logician of science, describes various calculi. She then can freely pick any of*

> *these calculi, and envision her group to speak a language coordinated with it.* In this way she applies to her group's hypothetical utterances, the syntactic explications of epistemic notions that the syntactic description of the calculus makes available. Application of terms like "true," "false," "consequence" and "consistent" in the logic of science become tolerably precise only via such coordination, and their application is restricted to the sentences of a possible language in coordination with the calculus for which they are defined. *This is how choice and tolerance in logic is possible.* (Ricketts, 2004, p. 193. My emphasis.)[10]

One element retained by Carnap on this reading is Frege's understanding of the axioms of logic as having a regulative role. In other words, what are now thought of as the logico-mathematical syntactical rules of a linguistic framework continue to provide "standards for validity and consistency", and this is just an expression of logocentrism. The difference is that without Frege's universalist assumption, there is nothing to suggest that there is only one set of such standards, and hence Carnap's adoption of the Principle of Tolerance.

The figure below displays the methodological dependencies of Carnap's program on the Deflationary interpretation. The recognition that we are embedded in our language, and that language itself is the device which makes inquiry, justification, and judgment possible, means that we cannot antecedently survey different sets of methods and concepts—or some external world—in order to determine which language best captures those things. Even the very question of what "best captures" is to mean is up for grabs, and so we have no recourse but to allow for a pluralism, a tolerance amongst different schemes for making sense of these notions. Carnap's preferred scheme involves a sharp distinction between the formal and the factual, and so he adopts a conventionalism and empiricism. These preference are further refined by his specific understanding of logico-mathematical sentences as formal auxiliaries.

[10]Cf. Ricketts (1994, pp. 182–183, 187). My own interpretation allows Carnap the leeway to suggest that we are free to choose whatever logical language seems best—we can even construct logical languages arbitrarily if it suits us. However, what the Deflationary reading misses is that when the goal is to reconstruct a pre-existing language, say the colloquial language of practicing science, we must take account of the customs of that language (recall our discussion from chapter 1 about pragmatics). More importantly, they miss the need to stress that this was Carnap's purpose. So while certain choices will remain—e.g., regarding the reconstruction of constructive mathematics we can choose to reject the law of excluded middle or employ only limited quantification—other aspects will be determined either by the semantics or the pragmatics of our informal theories. Cf. Carnap (1939, chps. 11–12).

Strong Meta-Languages

Logic and mathematics as formal auxiliaries

Linguistic Proposals (verificationism, empiricism, conventionalism, etc.)

Principle of Tolerance (Logical Pluralism)

Logocentrism

Figure 3.1: Methodological Structure of *Logical Syntax*—Deflationary Reading

It is important to emphasize that on the Deflationary reading these are simply *preferences*. Even the adoption of Tolerance itself, as we have seen, is taken to be an attitude rather than a theoretical commitment. However, our analysis of the Deflationary interpretation shows that it is incorrect for Goldfarb and Ricketts to state that a deflationary Carnap's program is "not based on any substantial theoretical commitments of its own." (Goldfarb, 1997, p. 61). Logocentrism— apparently inherited from Frege and expanded into Carnap's pluralism—is just such a commitment.

3.4 Arguing for Carnap's Meta-Philosophy

As mentioned above, the basic idea behind this Deflationary response to Gödel can be extended to the other circularity objections. Recall that each objection charges Carnap with the need to presume certain mathematical notions informally in order to demonstrate the Logical Empiricist thesis that mathematics is purely formal. Therefore Carnap's thesis is viciously circular if its has as its aim any sort of traditional foundational goal.

We saw in the last chapter that Carnap is not offering his mathematical conventionalism as a foundation for mathematics in the traditional sense. His interests are in the main methodological: To explicate the notion of 'mathematical truth' and associated concepts by attempting to recover the characteristic features of mathematical sentences, and the role they play in the practice of science, within the context of a formalized framework for the total language of science. As we saw, this program can still provide insight into the nature of mathematics, but largely eschews any justificatory or metaphysical questions.

The Deflationary reading also suggests that Carnap cannot be offering his conventionalism in the sense of a traditional foundation, but for somewhat different reasons.

Each of the circularity objections assumes that Carnap is trying to do foundational or epistemic work: Quine supposes Carnap's account is meant to generate or mediately explain mathematics as arising from conventions; Potter assumes that Carnap is trying to explain how we are able to grasp infinite truths as finite beings; and Beth presumes that Carnap is offering an explanation or account of our understanding of logico-mathematical structures. All of these projects presume some extra-framework notion which guides or constrains our choice of syntactical rules, but as with the notion of 'empirical fact', Goldfarb and Ricketts argue that Carnap's logocentric perspective requires that there can be no notion of 'mathematical truth' prior to the establishment of a linguistic framework.[11] But as we will see, this response calls in to question the tenability of Carnap's program, and it becomes unclear who if anyone would be willing to adopt his recommendations.

3.4.1 Rejecting Philosophy of Mathematics

According to Goldfarb and Ricketts, Carnap's view of mathematics

> is that mathematical truths are framework-truths: they are statements to whose acceptance any user of the framework is automatically committed. [. . .] They are, rather, artifacts of the linguistic system. (Goldfarb & Ricketts, 1992, p. 64)

As we have seen, this is certainly Carnap's understanding of the methodological role of mathematics in the sciences. Thus his reconstructions are formulated as to comport with this analysis. However, Goldfarb and Ricketts' take this one step further by suggesting that for Carnap, the definition of 'Analytic' for a language L "yields mathematics" (Ibid., p. 70), and that is all there is to it. Different linguistic frameworks will yield entirely distinct mathematics, and the Principle of Tolerance tells us that we are entirely free in our choice of linguistic framework, full stop.

This characterization appears in Goldfarb and Ricketts original article promoting the Deflationary reading. The context is a discussion about whether Carnap was amenable to meta-languages more expressive than their requisite object-languages.

[11]Consider: "To think there is some foundational question that will concern relative strengths of metalanguage and object language, and that has the effect of imposing restrictions on the metalanguage, is to accept some kind of epistemic relation that applies across language. But that is precisely what Carnap's basic view disallows. The dependence of some truths on other truths, be they empirical or analytic, can be made sense of only within a linguistic framework." (Goldfarb & Ricketts, 1992, p. 69)

This point was in question because Friedman (1988) mistakenly suggests that Carnap's idea in *Logical Syntax* was to use LI as a kind of "neutral" meta-language for all syntactical investigations.[12] In their article Goldfarb and Ricketts rightly observe that Carnap is open to the use of meta-languages of any required strength for the syntactical analysis of whatever object-language happens to be of interest. What is interesting is that, according to Goldfarb and Ricketts, it is this amenability of Carnap's toward strong meta-languages which requires him to give up any kind of traditional foundational program. I quote at some length:

> It seems clear, then, that Carnap had no objection to the use of strong metalanguages: not just those stronger than primitive recursive arithmetic, but even those that outstrip the object language being described. This implies that he did not intend the sort of bootstrapping that Friedman imputes to him, that is, the use of a weaker system, more readily acceptable from some standpoint or other, in order to legitimize a stronger system. *In that case, it is a mistake to take Carnap to be trying to give an informative answer to the Kantian question "How is mathematics possible?" or indeed to any similar question.* What we are suggesting is that Carnap does not take the general clarification of the status of mathematics which *Logical Syntax* provides as being at all foundational, as addressing the issues that concerned not just Kant, but also his immediate predecessors Frege, Russell, and Hilbert, and possibly even the 1930 Carnap of "The Logicist Foundations of Mathematics"[cited here as Carnap ([1931]1983)]. Rather, for the Carnap of *Logical Syntax* (and later), questions of foundations, upon clarification, wind up being questions of what can be done inside various linguistic frameworks, or what sort of frameworks can be made usable." (Goldfarb & Ricketts, 1992, p. 68. My emphasis)[13]

According to Goldfarb and Ricketts, Carnap's acceptance of strong meta-languages in the syntactical analysis of linguistic frameworks by itself *entails* that he has given up any traditional foundational proclivities, including questions regarding the ap-

[12] Friedman no longer holds this view. See, e.g., Friedman (1999; 2009).

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

plicability of mathematics in the empirical sciences. Instead, his program is one of proposing and investigating linguistic frameworks.

Their reason for this is that Goldfarb and Ricketts consider an objection along the lines of the circularity criticisms, supposing that if Carnap were doing any kind of foundational work at all, the objections would be successful. Here is their characterization of the problem, which is similar to what we called in our previous chapter the crux of the circularity objections:

> On Carnap's view, essentially, *the definition of "mathematical truth" for an object language is to yield mathematics*, by which he means all mathematical truths of the object language. It can do this, however, only given all mathematical truths of the metalanguage. It should occasion no surprise, then, that *mathematics is obtained with the adoption of a framework*; for what counts as "obtained" is everything that follows from the specification of the framework together with mathematics. (Ibid., pp. 70–71. My emphasis.)

The idea is that a notion of 'mathematical truth' is only established once we have adopted a linguistic framework, that is, once we have selected a class of syntactical rules. However, as we saw in the Logico-Mathematical Interlude, in order to demonstrate that all of the analytic sentences of a given framework follow purely in virtue of the L-Rules of the framework (i.e., to prove that all logical sentences are L-Determinate) requires the use of a meta-language. But this use of a meta-language is tantamount to accepting that meta-language as our linguistic framework, and so on *ad infinitum.*

The question, of course, is whether this situation is viciously circular:

> Is Carnap's position infected with a vicious circularity here? We think not. To be sure, there is a regress, but it is not obviously circular or vicious unless one thinks that some foundational work must be done by the syntactical description of a language. If no such task is at issue, then the upshot is simply that we can never make the conventional nature of mathematics fully explicit in any framework. The structure of Carnap's view is then coherent. Given the distinction between issues within a linguistic framework and issues between linguistic frameworks—a distinction that is always central to Carnap's thought—then the position is not circular so much as self-supporting at each level. If the mathematical part of a

> framework is analytic, then it's analytic; and so invoking mathematical truths at the level of the metalanguage is perfectly acceptable, since they flow from the adoption of the metalanguage. (Ibid., p. 71)

This is why in the previous chapter I suggested that a deflationary Carnap confronts the circularity objections by embracing an infinite hierarchy of meta-languages. On our understanding of Carnap's program, he is happy to appeal to some tacit understanding of our informal mathematical notions in order to fix the interpretation of the mathematical concepts required by the meta-language within which our syntactical investigations proceed. This is to recognize that Carnap's program is ultimately about explication.

On the Deflationary interpretation, however, mathematics is identified with the analytic sentences of our linguistic framework—there is no extra-linguistic notion of mathematical practice to which we may appeal. Thus the acceptance of any one linguistic framework requires the acceptance of an infinite hierarchy of meta-frameworks in order to support the idea that mathematics is just what follows from our choice of syntactical rules.

That a choice of meta-language must be made in order to demonstrate Carnap's thesis is a crucial point, something that Beth's objection brings into sharp relief. Recall his worry is that Carnap's advocating for an entirely formal approach to mathematics is inadequate, since *Logical Syntax* must presuppose that readers grasp the correct informal understanding of the meta-language used in the book, and this is to tacitly import already all required mathematical concepts. Goldfarb and Ricketts explicitly note this objection, arguing that it does indeed expose a limitation in Carnap's program:

> Clearly, if the metalanguage is a rich one, and if our understanding of it cannot be exhaustively explicated in terms of rules, deductive procedures in axiomatic systems, or the like, then Carnap's "presupposition" is an admission that much can never be made explicit, but must simply be tacitly relied upon. This fits poorly with Carnap's proclaimed standards of exactitude and rigor. (Ibid., p. 72)

The primary difficulty motivating Goldfarb and Ricketts' entire discussion here is that the Principle of Tolerance is a call to make our ideas clear. This seems to be interpreted by them as a call for formalization—and indeed we have seen that Carnap's methodological recommendations almost exclusively involve formalization.

However, as we know, Gödel's theorems require that the investigation of the meta-logical properties of an object-language requires a more expressive meta-language, but if we leave this meta-language informal, then we have failed to make our ideas completely clear.

Here is a portion of Ricketts' discussion of this objection:

> For Carnap, the notion of *understanding* is far too imprecise and unformed to bear much weight here. Indeed, throughout his career, he seeks to replace appeals to 'understanding' with comparably more precise appeals to explicitly, if informally, stated rules. (Ricketts, 2004, p. 195. Original emphasis.)

We are supposed to take Carnap as eschewing notions such as 'informal understanding' with an appeal to the syntactical choices made at the meta-level. The recourse for a deflationary Carnap here seems to be nothing more than a flat rejection of the idea that the intuitive understanding that Beth is looking for really exists, precisely because any useful notion of 'mathematical truth' will be language relative, and so no one construal can be any better than another. The logocentric nature of our methods and standards means that there is no language-transcendent notion to which we can here appeal. If Beth is looking for something more, then he has missed the point.

The consequence of this situation for Carnap's program, according to Goldfarb & Ricketts (1992), is that there can be "no exhibition of the basic idea" that mathematics is a purely formal set of conventionally chosen formal auxiliaries, since in order to derive this result it needs to be assumed that mathematics at least as strong "comes with the framework of the metalanguage" (p. 71). This is why, on the Deflationary reading, Carnap must reject *all* traditional questions of the philosophy of mathematics. The dual calls for clarity and pluralism of the Principle of Tolerance place Carnap's program in a somewhat tenuous situation on the Deflationary reading.

As another example consider again Gödel's observation that Carnap's definition of 'Analytic' for LII must utilize the term "for all properties" in a way that goes beyond the expressive means available in LII itself. We discussed this observation in the previous chapter, here is Goldfarb's take on the situation:

> If the use of an unbridled universality operator over higher order objects does not bespeak a Platonistic commitment, it must be connected somehow to convention. But how does this convention get to be determined? There seems to be no way to do this, except to say that it's a matter of

> the meta-metalanguage. [...] This is not an incoherent position; it is, as I have written elsewhere, 'self-supporting at each level'. But it does have more than a whiff of circularity or at least vacuity, which, of course, Carnap's critics will exploit. (Goldfarb, 2009, p. 120)[14]

Their way out for Carnap is to double-down on the Principle of Tolerance, to suggest that its scope is so broad as to encompass Carnap's entire program. In this way Carnap need-not justify or in any sense argue that mathematics can be treated as conventional—this is merely the proposal he is offering. It is perfectly acceptable for critics to formulate their own counter-proposals: Beth may propose an intuitionisitic framework, or Gödel a framework wherein we speak as mathematical realists. Such frameworks will address the metaphysical and epistemic questions of concern to their advocates. The Principle of Tolerance allows for all such language forms.

3.4.2 Objection From Intuitionism

This situation strikes me as less than optimal. Again, Carnap's scheme for addressing the controversy between intuitionism and classical mathematics does indeed involve the transformation of the debate into a choice between object-languages—LI and LII in fact—each offering methods of proof of differing strengths. Our choice is in the end a function of our aims and the value we place in various formal characteristics: Do we prefer greater security, or do we require greater (or at least more convenient) expressibility? But notice that the intuitionist would never agree to this approach to the debate, since the meta-logical tools required to state and adequately investigate LI and LII are already illegitimate to the intuitionist. This is not really a circularity objection as I have been using that label, since it questions the very possibility of Carnap's program rather than its tenability as a foundation for mathematics. In other words, the intuitionist will not find this situation amenable because the intuitionist *does not see the question as a matter of language choice in the first place*—the intuitionist rejects the Principle of Tolerance.[15]

[14] Cf. Goldfarb (1995*b*, p. 330; 1996, pp. 228–230).

[15] Indeed, we might construe an intuitionist like Brouwer as holding a "Principle of Intolerance". Besides rejecting as non-constructive the methods and concepts required for a complete meta-logical comparison (in Carnap's sense) of the languages in question, Brouwer would also reject the very idea that mathematical practice can be captured or in any way construed as a formal language. The gulf between Carnap and Brouwer here is as clear an example of a fundamental divergence of attitude toward mathematics and methodology as one is likely to find. My purpose in these next few pages is to suggest only that we can interpret Carnap in such a way that he has an actual *argument* for his own program over Brouwer's perspective, not that it would convince Brouwer, as he has such

This sort of argumentative situation places a deflationary Carnap in what seems to me to be a rather awkward position. Presumably, such critics are looking for some *principled reasons* why such long-standing questions in the philosophy of mathematics no longer deserve our attention. But this is exactly the sort of demand that the Deflationary view takes Carnap to reject. On this reading, Carnap thinks philosophers should be in the business of making proposals rather than arguing for positions. So Carnap can propose a language expressing something like the syntactic view, while Gödel and Beth are free to propose their own languages. The reason for this is the logocentric nature of our concepts. Once recognized, there is no obstacle to accepting the Principle of Tolerance, and so any "global" questions about epistemology or foundations become merely questions about language choice.[16]

A deflationary Carnap thereby rejects his critics' questions, and so their criticisms, out of hand as bad questions. But should someone dispute a hard-line logocentrism, Carnap's only recourse is to again point to the Principle of Tolerance and suggest that his challengers pursue their own course. Of course this will not persuade an intuitionist, nor should it, since they begin with quite different methodological assumptions. But there is no other way for Carnap to advocate for his program, and he has reached a complete impasse with his interlocutors. What the position thus amounts to is that unless someone accepts logocentrism—in essence *already buying* the Principle of Tolerance—there is nothing to recommend the adoption of any of Carnap's ideas and methods, including Tolerance. Carnap's meta-philosophical program comes as a "sealed package" on the Deflationary reading.

Notice, however, that this argumentative situation is incongruous with Carnap's pronouncements in the Foreword of *Logical Syntax*, which are in fact quite bold: That the considerations in the book *will lead us to the conclusion* that philosophy be *replaced* with the logical analysis of the language of science (p. xiii). On the Deflationary reading this amounts to no more than a *recommendation* to ignore traditional epistemic and foundational questions in favour of a project of language construction. If this is not a complete failure to address his objectors' concerns, such a response at least completely stifles any possibility of progress between Carnap and his critics.

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strong foundational convictions. Other intuitionists may be more amenable to Carnap's methods of formalization and an understanding of logical reasoning that sees it as foundationally on par with fundamentally mathematical reasoning like the principle of induction. For example, Brouwer's student Heyting ([1956]1971) developed several formalizations of intuitionistic logic. Carnap mentions some of Heyting's earlier work along these lines in §16 of *Logical Syntax*, alluding to the idea that Heyting is an example of an intuitionist more sympathetic to Carnap's reconstructive program. Thanks to John Bell for bringing up this point and coining the label for Brouwer's attitude.

[16] Cf. Ricketts (1996, p. 236).

While Goldfarb and Ricketts acknowledge this impasse, they dismiss it as simply "a mark of how basic a philosophical difference there is between Carnap and his critics." (Goldfarb & Ricketts, 1992, p. 71) As we will see below, our reading alternately allows Carnap to present an *argument* in favour of his program and its logical pluralism—that a conventionalism can provide an account as to the nature of logico-mathematical truth while still adequately recovering certain essential characteristics of logico-mathematical concepts as used in the language of science. So instead of a simple deference to Tolerance, that principle comes as a feature of Carnap's program, with the program as a whole recommending itself as an adequate and informative explication of the epistemic character and methodological role of mathematics.

3.4.3 Gödel's Criticism—Again

As noted at the outset of this chapter, the argumentative situation that results when advocating for a deflationary Carnap's proposals seems almost akin to Kuhn's understanding of crisis-stage science. As with Kuhnian paradigms, each linguistic framework is found to be incommensurable with every other. This understanding of the Deflationary reading is actually reinforced by reflecting upon their interpretation of some of Carnap's meta-logical results discussed earlier. Recall that Ricketts supposes that Carnap "places no justificatory weight on consistency proofs." (Ricketts, 1994, p. 192). In fact, Ricketts supposes this is support for the Deflationary interpretation, since Carnap is here not interested in traditional foundational questions.

Ricketts thus raises the same question we addressed in our first interlude: *Just what was Carnap up to in §34i?* Ricketts supposes that the proof is provided as *evidence* that LII can include synthetic sentences, and so "show that the language may be used to observationally test hypotheses." (Ibid., p. 193). Such evidence can, in turn, be cited when advocating that LII be adopted as the form of language to use in the reconstruction of scientific theories. In other words, in a debate over which language to adopt for the reconstruction of science—say with an intuitionist—Carnap's proof here is supposed to act as evidence in favour of LII.

Ricketts asserts that there is no vicious circularity in mounting "whatever mathematical resources are required" (Ibid.) to this end. But given what we have seen above, this whole line of reasoning seems almost certainly incorrect. An intuitionist should not accept the proof as evidence, since the result relies on methods the intuitionist rejects. But let us leave this point to one side for a moment. By asserting that Carnap lacks a language-transcendent domain of empirical facts, Goldfarb and Ricketts claim that it only makes sense to evaluate whether or not a given syntactical rule has empirical content relative to the analytic-synthetic dichotomy of the language within which it is embedded. But this means that Carnap has actually failed to address Gödel's original concern: Whether or not that syntactical rule is synthetic *in that language!* Thanks to the second incompleteness theorem, from within a particular language there is no non-circular way to determine this. A consistency proof within the language will be completely uninformative, as Carnap recognizes. We can provide one from without, using a more expressive meta-language, as Carnap does, but in what possible way could this provide *evidence* that the language is useful for formulating empirical hypotheses? In order to act as evidence in this way, the proof would need to establish—for lack of a better term—a *mathematical fact* about LII. This can only be done by presupposing more powerful mathematics at the meta-level, but this is just to give Gödel his point. Ricketts presumably supposes that the situation does not immediately succumb to Gödel's first objection because Carnap is not here attempting to defend the syntactic viewpoint, but only to offer a practical motivation for the adoption of LII. Like it or not, however, there is no reason not to re-Gödel at the level of the meta-language, since mathematics is being utilized to prove a mathematical fact about the power and practical interest of LII.

Indeed, we can also run Quine's criticism stemming from the first incompleteness theorem on this line of reasoning, since this sort of recommendation of a language requires one to argue that certain sentences of the language are analytic. However, from *within* the language there is no way to guarantee this—we must formulate a definition of mathematical truth from without. This is exactly what Carnap does, but such a definition cannot provide evidence that the object-language is suitable unless we are already presupposing the suitability of the more powerful meta-language. Thus on the Deflationary reading the point of such technical exercises is entirely unclear. Either these meta-proofs invite Gödel and Quine's points, or they must be read as entirely superfluous to any debate about the merits of LII.

So we find a deflationary Carnap in the position of being unable to marshal even *practical* considerations in favour of his preferred linguistic frameworks *within the context of his own program*, at least insofar as those considerations purport to be formal. As seen above, Carnap conceives the debate between intuitionists and classical mathematicians as a choice between a more conservative (and therefore safer) or a more expressive (and therefore more useful) language. But on the Deflationary reading, this is not a choice we really have, because in order to *compare* these distinct languages in a suitable way they must be embedded in a yet stronger language that we have already adopted. In other words, we cannot appeal to any language-transcendent notion of 'mathematical truth' or 'empirical fact', and so cannot take extra-framework results like consistency, completeness, the ability for a language to represent empirical hypotheses, or even relative expressive power as points in favour of one language or another. It therefore becomes a complete mystery as to why Carnap would go through the trouble to prove theorems such as consistency or the L-Determinacy of all logical sentences of LII in *Logical Syntax*. And furthermore, it becomes unclear how to even enact Carnap's program, since we cannot compare linguistic frameworks without already having adopted a more powerful framework.

3.4.4 A Methodological Argument for Tolerance

Our interpretation of Carnap's program does not encounter these difficulties, because we suppose that Carnap is willing to accept mathematical practice and its results as something distinct from its formal reconstruction. As with Carnap's proofs for the analyticity of choice and induction in LII, a proof of consistency can indeed act as evidence for LII on our interpretation because it is evidence that an explication is adequate. This evidence is with respect to an extra-framework understanding of our informal mathematical practice. On the Deflationary reading however, any formal property of a linguistic framework must be proven relative to some intra-framework notion at the meta-level.

We saw in our last chapter that his appeal to informal mathematics—the assumption that mathematics will produce its own results without need for philosophical grounding, and that as philosophers we can and should appeal to those results—is an important part of Carnap's program. This also provides Carnap with a somewhat more robust response to the intuitionist's objection discussed above. Recall Carnap's reply to Beth from the previous chapter. He *agrees* with Beth's observation that a particular interpretation of the meta-language was presupposed in *Logical Syntax*. Indeed, this is essential for his program because it allows interlocutors to communicate and so come to an agreement about what concepts are to be explicated. However, Carnap does note in a section of his reply that I failed to quote earlier that if the interlocutors cannot come to an agreement on a common informal meta-language, then no communication is possible. I now quote the passage:

> It seems to me obvious that, if two men wish to find out whether or not their views on certain objects agree, they must first of all use a common language to make sure that they are talking about the same objects. It

> may be the case that one of them can express in his own language certain convictions which he cannot translate into the common language; in this case he cannot communicate these convictions to the other man. For example, a classical mathematician is in this situation with respect to an intuitionist or, to a still higher degree, with respect to a nominalist. (Carnap, 1963, pp. 929–930)

Carnap seems to have no qualms about disengaging from the intuitionist if they are not willing to assume—at least for the sake of argument—the *mathematical methods required* for a complete meta-logical analysis of the controversy.

This is something we need to keep in mind: Languages of a certain strength are *required* if one wants to carry out meta-logical investigations of linguistic frameworks—cardinality considerations and the incompleteness theorems simply dictate the technical situation here. From a purely mathematical perspective, this poses absolutely no difficulty. We state our assumptions and carry on with proving theorems. An intuitionist may disagree also with this sentiment, but Carnap would suggest that her concerns are philosophical, not mathematical. Recall that he makes a point along these lines in *Logical Syntax*:

> The tolerant attitude here suggested is, as far as special mathematical calculi are concerned, the attitude which is tacitly shared by the majority of mathematicians. (*Logical Syntax*, p. 52)

The specific concepts developed in general syntax require the assumption of certain (perhaps controversial) logical notions, but there is no fundamental objection to developing completely distinct sets of meta-logical concepts for the purpose of investigating linguistic frameworks. Carnap would of course suggest that we use whatever meta-language is best able to let us fully examine the frameworks we are interested to choose between, but this is not to dogmatically require those concepts. And, indeed, much contemporary research into non-classical logics, non-classical set theories, and their respective semantics could be interpreted from the perspective of *Logical Syntax* as just such distinct avenues of investigation. They utilize a different set of logical concepts while pursuing broadly the same methodology. The only concern is that it becomes difficult to compare these distinct sets of concepts (meta-frameworks?) to the same extent that LI and LII can be compared using the classical concepts of general syntax, but this is hardly insurmountable.

Of key importance here is that Carnap is advocating for a strong meta-language on the basis of methodological considerations. Indeed, he is arguing for his entire understanding of logic and mathematics as formal by demonstrating that it adequately recovers certain key characteristics of mathematical sentences and their use. Contra the Deflationary reading, this allows Carnap to mount an *argument* in favour of his approach to philosophy as explication and to his particular explication of mathematics: Carnap's program not only recovers the role of mathematics in the sciences and provides insight into logico-mathematical concepts, it *also* allows us to move past seemingly intractable philosophical debates. On the Deflationary interpretation, this first aspect is missing, and so the best Carnap can do in advocating for his program is to point to the Principle of Tolerance and leave it at that. While such methodological considerations may not convince Brouwer or Gödel, they are genuine reasons in favour of Carnap's program.

3.5 The Scope of Tolerance

In examining the role of Tolerance in Carnap's meta-philosophical program, it will be worthwhile to return to the textual evidence cited in favour of the Deflationary interpretation during our discussion in the first chapter.

3.5.1 Empiricism, Stein, and Quine

Recall from our first chapter that we noted that Carnap on occasion asserts that we should think of his empiricism not as a philosophical thesis, but as a *proposal*. Goldfarb and Ricketts often point to such passages as evidence of their reading, since it shows that the Principle of Tolerance should indeed be taken as having a very wide scope in Carnap's program.

Similarly, Stein's (1992) recollection of the discussion between Carnap and Quine seems to support a Deflationary interpretation. Recall that Carnap and Quine were discussing the merits of formalized reconstruction for the philosophy of science. I repeat the passage here:

> This is a difference of opinion which, *despite the fact that it does not concern (in my own terms) a matter with cognitive content*, is nonetheless in principle susceptible of a kind of rational resolution. In my view both programs—mine of formalized languages, Quine's of a more free-flowing and casual use of language—ought to be pursued; and I think that if Quine and I could live, say, for two hundred years, it would be possible

> at the end of that time for us to agree on which of the two programs had proved more successful. (Stein, 1992, p. 279. My emphasis.)

Goldfarb and Ricketts take the passage to suggest that Carnap thought of his own program as a single, all-encompassing proposal, as a linguistic framework concerning one out of an unlimited number of possible meta-philosophies that we might choose to adopt. Quine's naturalist program is simply an alternate framework that we might choose instead—the Principle of Tolerance allows the choice of either.

This is to construe the Principle of Tolerance as having a very broad scope in Carnap's program, since all questions are ultimately a matter of Tolerance. However, Goldfarb and Ricketts' reading is not the only plausible reading of these passages. In fact, I do not think that it is even the most plausible, given the other evidence presented so far against the Deflationary interpretation. Rather, if we consider Carnap's program as the explication of the concepts of science, we can read both passages as advocating for Carnap's particular explication of a concept over some other.

To take Carnap's discussion with Quine first, Carnap straightforwardly states that the controversy admits of "*rational resolution*" even though the controversy does not concern "a matter with cognitive content". Presumably this would be empirical evidence regarding the success of one or the other programs—perhaps by means utilizing a criterion of papers published or scientific advances made. More seriously, the reason that this dispute lacks cognitive content for Carnap is because it is a question of a methodological choice: Should we pursue philosophy in the mode of the Logic of Science, or in the mode of naturalized epistemology? Carnap supposes both, because they are not incompossible projects. There is no need to introduce the notion of a linguistic framework here.

With regard to Carnap's presentation of empiricism in "Testability and Meaning" (1937), this should be interpreted as a problem of explication. Carnap's goal is the explication of the methods of science, which we have seen Carnap takes to be either empirical methods (*viz.*, observation and experiment), or formal methods (*viz.*, axiomatics and tolerance). Carnap bases this upon his understanding of the actual practices of science. Therefore a reconstruction of the total language of science should be an empiricist language. So while Carnap's empiricism is indeed a proposal to adopt a particular linguistic framework, as with his advocating for a formal understanding of logic and mathematics, we can see Carnap as forwarding a methodological argument that given the actual practice of science, an empiricist language seems to best capture those practices.

3.5.2 Another Possible Deflationary Motivation

Another possible motivation for Goldfarb and Ricketts' interpretation are the various discussions throughout *Logical Syntax* of the role that geometry plays in our system of theoretical knowledge. We know that a major influence on Carnap was Einstein's development of general relativity and its impact upon how we should understand the structure of our knowledge. Speaking in Carnapian terms, general relativity requires a shift in our characterization of the epistemic status of geometric principles from logico-mathematical rules to a matter of empirical measurement.[17] A Deflationist can argue that it thus seems dependent upon our framework whether the axioms of geometry are counted as logical or empirical, and so this is a matter for Tolerance.

While this is true in a sense, it misses a key insight of Carnap's project. In §50 of *Logical Syntax* Carnap speaks to a related issue as an example regarding his definition distinguishing between logical and descriptive vocabulary. I quote at length:

> Is the metrical fundamental tensor '$g_{\mu\nu}$', by means of which the metrical structure of physical space is determined, a mathematical or a physical term? According to our formal criterion, there are here two cases to be distinguished. Let S_1 and S_2 be physical languages, each of them containing not only mathematics but also the physical laws as rules of transformation [...] In S_1 a homogeneous space may be assumed: '$g_{\mu\nu}$' has the same value everywhere, and at every point the measure of curvature is the same in all directions (in the simplest case, 0—Euclidean structure). In S_2, on the other hand, the Einsteinian non-homogeneous space may be assumed: then '$g_{\mu\nu}$' has various values, depending upon the distribution of matter in space. They are therefore—and this is an essential point for our differentiation—not determined by a general law. '$g_{\mu\nu}$' is thus a *logical symbol* in S_1, and a *descriptive symbol* in S_2. For the sentences which give the values of this tensor for the various space-time points are in S_1 all determinate; and on the other hand, in S_2 at least part of them are indeterminate. At a first glance, it may appear strange that the fundamental tensor should not have the same character in all languages. But on closer examination we must admit that there is here a fundamental difference between S_1 and S_2. The metrical calcu-

[17] Thanks to Robert DiSalle for highlighting this point. Part III of Carnap ([1966]1974) is devoted to very insightful discussions of the distinction between mathematical and physical geometry, the various possible representations of space-time, and the reasons for choosing a non-Euclidean over a Euclidean metric.

> lations [...] are made in S_1 by means of mathematical rules [...] But on the other hand, for such calculations in S_2 empirical data are regularly required, namely, data concerning the distribution of the values of the fundamental tensor (or of the density) in the space-time domain in question. (*Logical Syntax*, pp. 178–179. Original emphasis.)

There are two important points here. First, we should recognize that one can *of course* construct a language wherein we assume a homogeneous (or heterogeneous) space, and so a language wherein the fundamental tensor ends up logical (or descriptive).[18] Recognizing this does not mean that there can be no language-transcendent notion of evidence, of 'empirical fact', which informs our reconstructions.

For example, in chapter 16 of *An Introduction to the Philosophy of Science* ([1966]1974), Carnap recounts the time he and Reichenbach traveled to the Einstein Tower in Potsdam to meet Erwin Finlay-Freundlich, while the latter was analyzing his observations which helped to establish general relativity's prediction that the path of light rays from distant stars should be deflected around the sun due to its gravitational field. Carnap notes that both a hetero- and a homogeneous space-time can account for these observations. But what is important for our purposes is that neither language determines or changes the epistemic role of Finlay-Freundlich's observations for any theory couched in either language: They are observations, empirical data—how else could they be construed? Any reconstruction, be it in S_1 or S_2, must take count of those observations (*as* empirical observations) regardless as to the syntactical rules chosen for the language.

Secondly, notice that the question of whether the fundamental tensor gets counted as logical or descriptive does indeed depend in one sense upon our choice to *assume* a homo- or a heterogeneous space-time. Once that choice is made however, the further question as to whether or not '$g_{\mu\nu}$' counts as logical or descriptive is out of our hands. Instead, Carnap's concepts in general syntax *dictate* the term's status on the basis of an external and objective criterion—a standard that applies *across languages*. Recall Carnap saying as much:

> if we reflect that all the connections between logico-mathematical terms are independent of extra-linguistic factors, such as, for instance, empirical

[18]Cf. Carnap ([1966]1974, chpts. 15–16), where he invokes Poincaré's well-known example of a spherical world wherein measuring rods contract as an observer moves further from the origin of the sphere. The point is that, whatever the observations to be accounted for, we can always introduce auxiliary assumptions or modify the laws governing our measuring apparatus (including the laws of optics if necessary) in order to maintain the claim that space-time has a favoured geometry.

> observations, and that they must be solely and completely determined by the transformation rules of the language, we find *the formally expressible distinguishing peculiarity of logical symbols and expressions* to consist in the *fact* that each sentence constructed solely from them is determinate. (*Logical Syntax*, p. 177. My emphasis.)

Carnap is here reflecting on the character of logico-mathematical knowledge and distills the unique property which allows us to capture its character formally. It is this criterion which determines whether a term is logical or descriptive, it is not a choice about which we can be "Tolerant." If the values of '$g_{\mu\nu}$' depend upon observations, the term is descriptive, otherwise not.

The point here is along the same lines as our discussions of Carnap's proofs of the analyticity of choice and induction in LII, and of its consistency. While the Principle of Tolerance grants us the ability to choose the assumptions and principles built into a language, this is not to say that there are no external constraints placed upon those choices. Carnap's logocentrism is not total. Instead, it is clear that Carnap observes certain constraints on the applicability of Tolerance, dependent upon the type of question that is being considered. Questions regarding the admissibility of some particular axiom or rule of inference (like choice), or those determining the form of a language (e.g., whether or not to include impredicative definitions, or to assume a heterogeneous space-time) are taken to be amenable to Tolerance. On the other hand, epistemological questions concerning the character of logico-mathematical (or empirical) knowledge are not presented as proposals to be weighed and measured practically, instead they are answered in a categorical way by formally recovering those properties that are essential to the identification and implementation of such kinds of knowledge in our scientific theories. The Deflationary reading simply misses this, and so construes Tolerance far too broadly.

In the next interlude I will summarize the interpretation of Carnap's meta-philosophical program that may be extracted from our discussions thus far. We will then have occasion to address one further aspect of that program in more detail. Specifically, we will return to Carnap's treatment of philosophical controversies, comparing his discussion in *Logical Syntax* with his more mature approach. I argue that they are importantly similar, despite refinement and a change in terminology. These concepts will then be utilized in our final chapter.

Anti-Metaphysico Philosophical Interlude

God created the integers; all else is the work of man.

LEOPOLD KRONECKER

The natural-number symbols are primitive symbols; the fractional expressions and the real-number expressions are introduced by definition.

RUDOLF CARNAP, *Logical Syntax*, p. 305

The next chapter will significantly shift our focus, away from the interpretation of Carnap and toward the analysis of set- and category-theory as a foundation for mathematics. This section is meant to act as something of a transition, as a summary of the reading of Carnap's program developed in the previous chapters, and also as an introduction to the concepts we will utilize in the final chapter for our analysis of Hellman's criticisms of set- and category-theory.

At least one of Hellman's worries is ontological, that an adequate foundations for mathematics must guarantee (or otherwise explain why we need-not worry about) the existence of enough objects to serve as the elements in models of our mathematical theories. This seems exactly the kind of worry that Carnap would classify as a pseudo-problem. It will therefore be worthwhile to take a closer look at just how Carnap identifies and treats pseudo-questions and statements, especially those concerned with abstract entities like numbers. I will argue that his approach in *Logical Syntax* maps onto his better-known, later distinction between *internal* and *external* questions as presented in *ESO* ([1950]1956). But first to the summary of our understanding of Carnap's program thus far.

B.1 Summary of Carnap's Meta-Philosophy

We have seen in the previous chapters that the methodological reforms Carnap suggests for philosophy amount to philosophy as the explication of the concepts of science (including mathematics) through the use of suitably robust meta-logical languages, along with the construction of said languages. Of paramount importance for Carnap is the development of successful reconstructions of the language of science which recover a sharp distinction between the formal and the factual components of our scientific knowledge. Such reconstructions are meant to offer insight into the structure of our scientific theories by clearly delimiting those aspects that are a matter of our linguistic conventions from those that are dependent upon the world. Similarly, in the reconstructive process itself we are required to distinguish object-level from meta-level sentences, helping us to identify philosophical pseudo-problems as apart from those questions that are usefully treated as a matter of framework choice, or questions amenable to actual scientific methods, either mathematical (a matter of proof) or empirical (a matter of observation and experiment).

It is in this vein that Carnap suggests we take the foundational debate between intuitionism and classical mathematics as a matter of language choice. This approach is predicated on the idea that there is no fact of the matter at stake—that the methods of proof we count legitimate are a matter of choice because the question regards only the language we wish to use in our scientific investigations. To see the debate in this way requires that we can treat mathematics formally, as a set of syntactical rules. But the circularity objections point out that in order to *demonstrate* that mathematics can indeed be treated formally requires that, at some level, we not treat mathematics purely formally. Instead, we must presuppose an understanding of mathematical notions.

Is this position viciously circular? I have argued that it is not, because Carnap's philosophy of mathematics is not a doctrine aiming to justify mathematical truths, or to explain our knowledge thereof, by arguing that those truths are generated by means of linguistic conventions. Rather, Carnap's goal is the methodologically motivated explication of the role of mathematics in the practice of science. Carnap's mathematical conventionalism is a formalized reconstruction of the formal sciences, and 'Analytic' in LII his *explicatum* for classical logico-mathematical truth. An explication demonstrates its worth not by being *proven true*, but rather by being fruitful and successfully recovering the key characteristics of the *explicandum*. Carnap provides evidence that his reconstruction recovers the role of mathematics in the sciences, explains its applicability, and offers insight into mathematical concepts and their use. In so arguing that we should adopt these *explicata*, the use of mathematical notions of whatever strength is no more vicious than Russell's use of the definite article in explaining his theory of descriptions.

This approach to foundations is again instanced in Carnap's discussion of the controversy between logicism and formalism. Carnap does not favour logicism because it is true that mathematics can be reduced to logic,[1] but because it better accounts for the methodological role of mathematics in the sciences. Analogously, Carnap favours a mathematical conventionalism precisely because it allows for the dissolution of what he sees as fruitless philosophical controversies, while at the same time recovering the key methodological characteristics of mathematics. These are points that Carnap can offer in favour of his admittedly non-traditional position. Further questions regarding the fundamental ontological status of mathematical entities or truths, and questions regarding our grasp of mathematics or its justification which fall outside the realm of empirical psychology, Carnap takes to be pseudo-questions. They address matters for which there is seemingly no common criteria for coming to an answer, and so interlocutors remain locked in fruitless wrangling.

This however is not to suggest—as the Deflationary reading seems to—that Carnap has completely discarded all questions in the philosophy of mathematics. We have seen that a close reading of *Logical Syntax* and Carnap's other works evidences his engagement with certain traditional questions in the philosophy of mathematics, especially the question of applicability. Notice that Carnap is alway very careful to distinguish questions regarding the representation of various logico-mathematical proposals—stronger or weaker languages? predicative or impredicative definitions? axiom of choice?—from questions that have an epistemic or methodological significance, such as the nature of logico-mathematical truth, the essential characteristics of logical vocabulary, or the role of protocol-sentences in the language of science.[2] This latter class of questions receive categorical answers in Carnap's work, because he is attempting to capture the essential characteristics of an *explicandum*, an existent, if informal, colloquial concept of use in the sciences.

Our discussion thus suggests that the scope of the Principle of Tolerance is some-

[1] Carnap takes the reduction to be a mathematical question that Russell & Whitehead demonstrated to be extremely plausible, *modulo* a few points like the logical status of the axiom of infinity, which Carnap thinks have since been overcome (e.g., by using a co-ordinate language, cf. *Logical Syntax*, §15 and Carnap ([1954]1958, §40)).

[2] This important observation became clearer to me through conversations with Steve Bland. As noted in our previous chapter, my reading of Carnap owes much to our conversations.

what limited in Carnap's program. It is a principle licensed and justified by Carnap's prior methodological analysis of mathematical practice and its role in the language of science. Carnap's logical pluralism thus derives from his ability to recover the key characteristics of mathematics as a set of formal auxiliaries—as a result there are no questions of correctness with regard to the choice of one set of logico-mathematical principles over any other. Not only does Carnap take this approach to comport with actual mathematical practice, but he also sees it as the most fruitful way to proceed. Carnap summarizes these ideas nicely in *Foundations of Logic and Mathematics*:

> The result of our discussion is the following: logic or the rules of deduction (in our terminology, the syntactical rules of transformation) can be chosen arbitrarily and hence are conventional if they are taken as the basis of the construction of the language system and if the interpretation of the system is later superimposed. On the other hand, a system of logic is not a matter of choice, but either right or wrong, if an interpretation of the logical signs is given in advance. But even here, conventions are of fundamental importance: for the basis on which logic is constructed, namely, the interpretation of the logical signs (e.g., by a determination of truth conditions) can be freely chosen.
>
> It is important to be aware of the conventional components in the construction of a language system. This view leads to an unprejudiced investigation of the various forms of new logical systems which differ more or less from the customary form (e.g., the intuitionist logic constructed by Brouwer and Heyting, the systems of logic of modalities as constructed by Lewis and others, the systems of plurivalued logic as constructed by Lukasiewicz and Tarski, etc.), and it encourages the construction of further new forms. The task is not to decide which of the different systems is "the right logic" but to examine their formal properties and the possibilities for their interpretation and application in science. (Carnap, 1939, pp. 28–29)

In other words, our formal explications can be judged right or wrong insofar as we are working to capture some pre-existing scientific concept (including logico-mathematical concepts), but this should not limit us in the development and investigation of new logico-mathematical languages or in the explication of untraditional interpretations of our logico-mathematical concepts.

The figure below encapsulates the relationship between the various key principles and concepts at work in the logico-mathematical portion of Carnap's program on our interpretation. Compare this with the Deflationary interpretation as outlined in Figure 3.1 from our previous chapter.

Principle of Tolerance (Logical Pluralism)

Logic and Mathematics as Formal Auxiliaries

Formal–Factual Distinction

Methodological Analysis of Science

Figure B.2: Methodological Structure of *Logical Syntax*—Our Reading

On our reading, the Principle of Tolerance is not a basic attitude that one must adopt in order to engage with Carnap's program. Instead, Tolerance is supported by methodological insights into the nature and practice of science. It is a methodological maxim which falls out of Carnap's goal of bringing to philosophy a mathematical method, along with his treatment of philosophy as neither above nor below the sciences, but rather as a part of that same enterprise. Mathematics takes care of constructing and investigating mathematical structures and proving theorems about them. In this task the mathematician will develop a variety of methods. Carnap sees the philosopher's task not as one of justifying these concepts and practices, or of telling the mathematician which methods and concepts are legitimate. Instead, the philosopher's role is to explicate these concepts and methods in the hope of providing epistemic and methodological insights, as well as offering tools for logical analysis when interlocutors get bogged-down in seemingly intractable controversies.

One might reject the methodological observations that ground this program as missing some key aspect of the structure of science. Or one might argue that Carnap's understanding of mathematics as a formal science fails to adequately explicate the role of mathematics in the language of science. However, as noted in the previous chapter, on the reading developed here there is a least an argument to be made that Tolerance is the correct methodology for addressing foundational disputes because Carnap can marshal the evidence that his program recovers the features of mathematics and logic that are essential to their development and application in the empirical sciences. He thus shifts the burden of proof onto the intuitionist to show that Carnap's account is inadequate in this regard.

B.2 Pseudo-Questions and the Reconstruction of Philosophy

The majority of our attention thus far has been paid to Carnap's understanding of mathematics and his explicative scheme for the concepts of science. However, as observed at the beginning of this dissertation, another of Carnap's interests is the evaluation of philosophy itself. In this vein, recall his reflections from an appendix to his *Introduction to Semantics*:

> The *chief thesis* of Part V, if split up into two components, was like this:
>
> a. "(Theoretical) *philosophy* is the logic of science."
>
> b. "Logic of science is the syntax of the language of science."
>
> (Carnap, [1942]1975, p. 250. Original emphasis.)

Carnap's overall goal is to fold philosophy into the sciences as a fruitful and progressive inquiry by the replacement of traditional philosophical methods with more rigorous mathematical methods. In this way "*the logic of science takes the place of the inextricable tangle of problems which is known as philosophy.*" (*Logical Syntax*, p. 279. Original emphasis.) According to the partition above, this involves first arguing that philosophical inquiry is nothing other than the Logic of Science, that is, the logical analysis of the concepts and languages of the sciences; and second arguing that Carnap's formal, syntactical methods are adequate to this task, that all questions of logical analysis are meta-level questions of logic or language choice, and can be treated purely formally. We can see this situation as a problem of explication: To reconstruct that part of philosophy which is scientific, while at the same time arguing that the rest of traditional philosophy is not a properly cognitive enterprise.[3]

B.2.1 Pseudo-Sentences and the Material Mode

We have already seen many examples of this transformative scheme. Carnap's proposed resolution of foundational debates was to transform them into questions of logic or into a choice of syntactical rules. This was the case for the controversy between intuitionists and classical mathematicians regarding the methods of proof we

[3] This way of framing Carnap's approach with regard to Part V of *Logical Syntax* was suggested to me by Wagner (2009*b*). I follow his discussion closely in parts of this section.

should consider legitimate, as well as the question over the ultimate nature of numbers debated by finitists and logicists. We have also seen Carnap employ a similar strategy to resolve seemingly intractable metaphysical disputes such as the debate between realists and idealists or between platonists and nominalists. In each of these cases the theses in question are initially presented as object-level assertions about the world; but upon translation into a rigorous formal language are seen to be more fruitfully interpreted as meta-level assertions about our language or as a proposal for some possible linguistic framework we might adopt.

The question is whether or not this scheme is adequate to encompass all relevant philosophical inquiry. Recall from our discussion in the first chapter that by the time of his adoption of semantics, Carnap took part (b) of the thesis above to be inadequate because it failed to include the semantical analysis of language. We will leave this issue to one side for the moment, noting that in *Logical Syntax* Carnap took his work in general syntax to make plausible the idea that purely syntactical methods are adequate in logical investigations.[4]

Isolating Scientific Philosophy

Making plausible (a) involves first cleaving-off certain domains of inquiry that have traditionally been labeled 'philosophy' as either belonging to the empirical sciences, or as being without cognitive content. In the first case, Carnap makes a distinction between *object questions*, those about the actual objects of some domain of interest, and *logical questions*, which are meta-level questions about linguistic expressions or theories which themselves treat objects. Most object questions are treated by the empirical sciences; with regard to philosophical questions about beliefs, mental states, etc., Carnap notes that these should properly be investigated via the empirical methods of psychology. However, as traditionally conceived philosophy also includes some object questions, especially those about supposed objects not amenable to treatment by the empirical sciences. Carnap gives as examples questions of what we might call "hard" metaphysics—about the nature of the absolute, things-in-themselves, values and absolute norms, etc. He dismisses these almost out of hand as non-cognitive, and so not really object questions.[5]

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[4]Our discussion thus far should make clear that Carnap did not think any one formal language would be sufficient for the treatment of all philosophical or logico-mathematical inquiry. Earlier in *Logical Syntax* he remarks: "*everything mathematical can be formalized, but mathematics cannot be exhausted by one system*" (p. 222. Original emphasis).

[5]Carnap does not attempt to argue directly that metaphysics is without meaning in *Logical Syntax*: "For anyone who shares with us the anti-metaphysical standpoint it will thereby be shown that

This leaves questions of logic, epistemology, and philosophical foundations (of physics, history, etc.):

> The term '*logic of science*' will be understood by us in a very wide sense, namely, as meaning the domain of all the questions which are usually designated as pure and applied logic, as the logical analysis of the special sciences or of science as a whole, as epistemology, as problems of foundations, and the like (in so far as these questions are free from metaphysics and from all reference to norms, values, transcendentals, etc.). To give a concrete illustration we assign the following investigations (with very few exceptions) to the logic of science: the works of Russell, Hilbert, Brouwer, and their pupils, the works of the Warsaw logicians, of the Harvard logicians, of Reichenbach's Circle, of the Vienna Circle centring around Schlick [...] (*Logical Syntax*, pp. 280–281. Original emphasis.)

Carnap says that these investigations (including the Vienna Circle's own past work) have often been formulated as object questions or mixed-questions. In order to make plausible part (b) of his thesis, Carnap will have to show that these formulations are ultimately misleading:

> As usually formulated, these questions are in part logical questions, but in part also object-questions which refer to the objects of the special sciences. Philosophical questions, however, according to the view of philosophers, are supposed to examine such objects as are also investigated by the special sciences from quite a different standpoint, namely, from the purely philosophical one. As opposed to this, we shall here maintain that all these remaining philosophical questions are logical questions. Even the supposititious object-questions are logical questions in a misleading guise. The supposed peculiarly philosophical point of view from which

all philosophical problems which have any meaning belong to syntax. The following investigations concerning the logic of science as syntax are not, however, dependent upon an adherence to this view; those who do not subscribe to it can formulate our results simply as a statement that the problems of that part of philosophy which is neither metaphysical nor concerned with values and norms are syntactical." (p. 280). Carnap does argue against traditional metaphysics (recall our brief discussion in chapter 1) in, e.g., Carnap ([1932]1934; [1932]1960; [1935]1996). The first two sources are instances of Carnap's famous critique of metaphysics by appeal to a straightforward verificationism. Wagner (2009*b*, pp. 184–187) observes that by the time of *Logical Syntax* Carnap had given up the simple formulation of this doctrine, and so these critiques. Instead, Carnap's goal in *Logical Syntax* is not so much a critique of the meaningless parts of metaphysics, but a reconstruction which identifies that part of such theses which is meaningful.

> the objects of science are to be investigated proves to be illusory, just as, previously, the supposed peculiarly philosophical realm of objects proper to metaphysics disappeared under analysis. (*Logical Syntax*, p. 279)

Carnap assumes that there is no special insight that philosophers can provide to genuine object questions of empirical science. Beyond this, Carnap's systematic treatment of philosophy aims to show that all of these object questions remaining in philosophy are really *pseudo-object questions*. That is, they are meta-level questions disguised as object-level questions. Notice here once again Carnap's assumption of a prior, exclusive and exhaustive distinction between the formal and the factual.

Faults of the Material Mode: Ambiguity

This analysis of philosophical questions is facilitated by an analogous distinction between genuine *object sentences*, which are sentences of the empirical sciences that are legitimately about the elements of some extra-linguistic domain of interest, and *syntactical sentences*, which transparently regard only the structure and formal properties of linguistic expressions and theories.[6] As before, we also have a third category of *pseudo-object sentences*, which are ambiguous. These latter sentences are formulated in what Carnap calls the *material mode of speech*—which often occurs when we speak in our informal, colloquial language. This mode is identified specifically by its use of object-level discourse inappropriately, which masks that such statements are best interpreted as syntactical sentences, that is, as meta-level statements about language. Thus Carnap's approach to philosophical debates that we introduced in our first chapter was the *translation* of material mode assertions into what he calls the *formal mode of speech*—an exactly specified formal language which interprets our philosophical statements as syntactical sentences.

In order to clarify these distinctions, we take an example from Carnap's *Philosophy and Logical Syntax* ([1935]1996).[7] Consider the following sentences:

1a: The rose is red.
1b: The rose is a thing.
1c: The word 'rose' is a thing-word.

[6] Carnap actually counts syntactical sentences as a sub-species of *logical sentences*, which include also semantical sentences about the meaning of logical expressions. At the time of writing *Logical Syntax*, Carnap believed that most semantical issues could be treated syntactically, with the remaining semantical sentences being pseudo-object sentences in the sense to be explained below.

[7] Along with Carnap's previously-cited article "Formal and Factual Science" ([1934]1953), these two sources make up a succinct overview of the main philosophical theses and results forwarded in *Logical Syntax*.

The first sentence is synthetic, and "really asserts some quality of the rose." (Ibid., p. 62). Thus it is a genuine object-sentence. Sentence 1c is obviously a syntactical sentence. It concerns linguistic expressions (the word 'rose'), and is analytic in a suitably constructed language. On the other hand, 1b is ambiguous between the two others. Grammatically its subject is the rose, and so it seems to be about that object. But Carnap asserts that the sentence gives us no information about an object, since "from the sentence 1b we cannot learn any quality of the rose, neither as to its colour, nor size, nor form, nor anything else." (Ibid.). As with 1c, the sentence is analytic, since we can determine its validity without ever consulting the rose itself, but only by understanding that the word 'rose' belongs to the category of 'thing-words'. That is, we need only consult the rules of our framework to recognize its truth.

What is important here is that the sentences 1b and 1c seem to be equivalent, although the former is phrased in a misleading way. This suggests that pseudo-object sentences can be translated into syntactical sentences within the context of a suitably expressive meta-language. Indeed, this is the strategy we saw Carnap suggest in our original example from chapter 1 regarding the controversy between realists and idealists over the fundamental constituents of reality, as well as in the debate between finitists and logicists over the correct type-level for numbers, and between realists and nominalists over the ontological status of abstract entities.

Faults of the Material Mode: Language-Relativity

Carnap also warns us that the material-mode is apt to confuse in another, related way. Statements made in the material mode are often incomplete in the sense that they fail to note the language(s) to which they apply. In other words, such statements obscure the *relativity* of those assertions to some particular language or set of languages:

> Further, the use of the material mode of speech gives rise to obscurity by employing absolute concepts in place of the syntactical concepts which are relative to language. With regard to every sentence of syntax, and consequently every philosophical sentence that it is desired to interpret as syntactical, the language or kind of language to which it is to be referred must be stated. If the language of reference is not given, the sentence is incomplete and ambiguous. (*Logical Syntax*, p. 299)

For example, this language relativity is another important aspect in the dissolution of the dispute between realists and idealists. Carnap suggests the transformation of the realist and idealist theses into syntactical sentences regarding the structure of our language, or alternately into syntactical sentences proposing a possible structure for a refined language of science. Interpreted in the former way, as claims about a particular language, the dispute is a matter of logic—whether our language contains certain expressions which are equipollent. Interpreted in the latter way, the theses become suggestions for the most fruitful way to structure a possible language. In fact, interpreted as syntactical sentences the assertions need-not conflict at all, since it is possible to have a single language expressive enough to incorporate both suggestions.

In this same section of *Logical Syntax*, Carnap actually offers a list of some possible ways in which one might intend a syntactical sentence to hold as an assertion: (1) For all languages; (2) For all languages of a certain kind; (3) For the current language of science or a sub-domain thereof (physics, biology, etc.); (4) For a particular linguistic framework with well-specified syntactical rules; (5) For at least one language of a certain kind; or (6) For at least one language in general. Finally, we might state a syntactical sentence not as an *assertion* with respect to some language at all, but as a *proposal* meant to hold: (7) For a previously unstated language proposed as a language of science (or a sub-domain thereof); or (8) For a previously unstated language which it is proposed we formulate and then investigate, with no attention as to whether it is to serve as a language of science.[8] Carnap stresses that especially in the first six cases, when expressing our theses in the material-mode, we are apt to ignore the need to specify the scope of our assertions:

> The use of *the material mode of speech* leads, on the other hand, to a *disregard of the relativity to language of philosophical sentences*; it is responsible for an *erroneous conception of philosophical sentences as absolute*. It is especially to be noted that the statement of a philosophical thesis sometimes (as in interpretation 7 or 8) represents not an *assertion* but a *suggestion*. Any dispute about the truth or falsehood of such a thesis is quite mistaken, a mere empty battle of words; we can at most discuss the utility of the proposal, or investigate its consequences. But even in cases where a philosophical thesis presents an assertion, obscurity and useless controversy are liable to arise through the possibility of several interpretations (for instance, 1 to 6). (Ibid. Original emphasis.)

[8] Recall that the Deflationary interpretation takes Carnap's program to transform all philosophical assertions into linguistic proposals. Carnap's suggestions here seem contrary to that idea. It will indeed be fruitful in many cases to formulate philosophical theses as proposals rather than assertions however, but this is not the only option.

Consider again the debate between finitists and logicists regarding the nature or ontological status of the natural numbers (*Logical Syntax*, p. 300). Whereas a finitist might assert

> [**1a:**] *Numbers* belong to a special primitive kind of objects. (Ibid.)

A logicist would insist, rather

> [**2a:**] *Numbers* are are classes of classes of things. (Ibid.)

Carnap suggests that translated into the formal-mode however, the finitist thesis becomes the assertion that

> [**1b:**] Numerical expressions are expressions of the zero-level. (Ibid.)

While the logicist thesis becomes

> [**2b:**] Numerical expressions are class-expressions of the second level. (Ibid.)

Interpreted in the sense of (1) above, both assertions are obviously false. Interpreted rather in the sense of (6), both assertions are true. The point is that once the scope of each assertion is made clear, there becomes in each case a specific method for resolving the controversy.

Acceptable Uses of the Material Mode

It is important to note that Carnap does not advocate abandoning the material mode of speech where there is little chance of confusion:

> *It is not by any means suggested that the material mode of speech should be entirely eliminated.* For since it is established in general use, and is thus more readily understood, and is, moreover, often shorter and more obvious than the formal mode, *its use is frequently expedient.* (*Logical Syntax*, p. 312. Original emphasis.)

It would also be a mistake to think that Carnap is suggesting that all object-level discourse is inappropriate. Much of the inquiry of the empirical sciences is formulated using object sentences because this inquiry is really about objects in the world. Instead, Carnap supposes that philosophical discourse does not contribute anything empirical over and above what can be said in the sciences—in that case you would just be doing empirical science. Therefore, the criterion for meaningful philosophical discourse is its translatability into the formal mode, into syntactical sentences:

> *Translatability into the formal mode of speech constitutes the touchstone for all philosophical sentences*, or, more generally, for all sentences which do not belong to the language of any one of the empirical sciences. (Ibid., p. 313. Original emphasis.)

Taken altogether we arrive very much in the same place we were at the end of section B.1. The method of logical syntax acts as a tool that we can use when our investigations stall because of some seemingly intractable controversy. This allows us to resolve philosophical debates, as we have seen. But the tools of philosophy also support science by acting as a sort of "logical microscope" to rigorize and investigate concepts, theories, and methods when and where that may be helpful to broaden our understanding. This attitude comports with our overall understanding of Carnap's meta-philosophy as the explication of the concepts of science.

B.2.2 A Formal Criterion to Identify Pseudo-Sentences

Carnap's notions of the material mode of speech and pseudo-object sentences still require a formal explication of course. What Carnap must do is provide a formal criterion for identifying pseudo-sentences, as well as a method for reliably translating them into the formal mode. To this end Carnap identifies pseudo-object sentences with what he earlier in the book calls *quasi-syntactical sentences of the material mode of speech*. These concepts are given formal definitions in §63–64 of *Logical Syntax*. However, the technical details are rather complex. This is due in part to the complexity of the languages Carnap treats, and in part to his aforementioned opaque notation and sometimes convoluted definitions in general syntax. For our purposes merely describing the intent of the definitions should be sufficient.

Given an object-language L_1 regarding a domain of extra-linguistic objects, consider some logical or descriptive predicate P of those objects. P is a *quasi-syntactical predicate* if there exists a language L_2, and a predicate Q of L_2, such that L_2 contains both L_1 and the syntax-language, ML_1, of L_1, and for any argument a of P, $P(a)$ is equipollent (*viz.*, equivalent in content) to $Q(\text{"}a\text{"})$ in L_2, where ' "a" ' is the name of a in ML_1. $P(a)$ is then said to be a *quasi-syntactical sentence* relative to L_2. While $Q(\text{"}a\text{"})$ is its *syntactical correlate*, or in other words, its translation into the formal mode in L_2.

To completely capture the notion of a pseudo-object sentence requires the further qualifier to the notion 'quasi-syntactical sentence' of being *in the material mode of speech*. This is because a quasi-syntactical sentence may also be in the *autonymous* *mode of speech.* Consider again the above example, but the result is that $P(\text{“}a\text{”})$ is also a well-formed formula by the formation rules of L_2. In this case the sentence $P(a)$ is in the autonymous mode of speech (because the expression ‘a’ is used as its own name). If, on the other hand, $P(\text{“}a\text{”})$ is not well-formed, then $P(a)$ is in the material mode.

The idea behind these definitions is just what we saw above in Carnap's example involving the sentences about the rose. Whereas 1a is a genuine object sentence, 1b only seems to be because grammatically the word ‘rose’ stands in the place of the subject. However, the predicate ‘thing’ has a syntactical correlate in the predicate ‘thing-word’, and so we identify 1b as a quasi-syntactical sentence of the material mode—it is actually a syntactical sentence, but this is disguised by its surface-grammar. We can thus translate 1b into the the formal mode as 1c, which is equivalent in meaning and is likewise analytic.

B.2.3 Universal Words and Predicates

By this point we have offered several examples of Carnap's translational scheme in action. Carnap tells us that one particularly common class of pseudo-object sentences are those that include *universal words*—including such words as ‘number’, ‘thing’, ‘property’, ‘object’, ‘fact’, ‘spatial point’, etc.

Carnap's definition of this notion relies on his previous definition of an expression's *syntactical genus.* Basically, Carnap uses his rules of substitution and the formation rules of the language to define equivalence-classes of expressions according to their mutual substitutivity. Two expressions are called *isogenous* if, when they can be mutually substituted in any sentence, those sentences remain well-formed according to the formation rules of the language. A class of expressions is then a *syntactical genus* if every two expressions in the class are isogenous, and no expression of the class is isogenous with any expression outside of the class. Carnap tells us that, informally, we can say that a word in the material mode is then a *universal word* if it expresses a property belonging analytically to all the objects of a *genus*, wherein any two objects belong to the same genus if their designations belong to the same syntactical genus. In the formal mode, we say it is a *universal predicate.*[9]

To take one of Carnap's favourite examples, in languages wherein numerals constitute a syntactical genus (as in LI and LII), in the sentence

[9]It seems to me that a many-sorted language would be particularly useful as a context for explicating Carnap's ideas here.

Five is a number.

the expression ‘number’ is a universal word. This is in contrast to a sentence such as ‘5 is odd’, since ‘odd’ is not here a universal word. The reason for this is that while the sentence is certainly analytic (e.g., in LII), substitution of ‘5’ with some other numerical expression (i.e., some other expression of the same syntactical genus) may result in a contradictory sentence. On the other hand, the sentence ‘Kirk is a number’ is considered completely meaningless (again, in LII), because according to the formation rules of LII descriptive terms are not legitimate arguments for the predicate-term ‘number’. However, the sentence ‘Kirk is a thing’ is analytic, and so shall it be for any legitimate substitution of ‘Kirk’. Thus the term ‘thing’ is again a universal word.

It is important to recognize that not every use of universal words is problematic, or even an instance of the material mode of speech. For example, in the sentence ‘The integer 7 is odd’, the expression ‘integer’, although a universal word, is innocently used as an auxiliary expression to point out the genus to which the term ‘7’ belongs. Similarly for the universal word ‘process’ in a sentence such as ‘The process of crystallization. . . ’.

Problems are encountered only when universal words appear as “independent expressions” (*Logical Syntax*, p. 297) in a sentence, for example as the primary predicate. Carnap tells us that such sentences belong to the material mode of speech, and are apt to confuse:

> Most ordinary formulations in the material mode of speech depend upon the use of universal words. *Universal words very easily lead to pseudo-problems*; they appear to designate kinds of objects, and thus make it natural to ask questions concerning the nature of objects of these kinds. For instance, philosophers from antiquity to the present day have associated with the universal word ‘number’ certain pseudo-problems which have led to the most abstruse inquiries and controversies. It has been asked, for example, whether numbers are real or ideal objects, whether they are extra-mental or only exist in the mind, whether they are the creation of thought or independent of it, whether they are potential or actual, whether real or fictitious. (*Logical Syntax*, p. 310. Original emphasis.)

The temptation is to reify the objects to which the universal word seems to refer, which leads to intractable philosophical disputes. In such cases the universal word is being used as a quasi-syntactical predicate, and so the sentence is a pseudo-object sentence. Thus these disputes rest ultimately on a confusion according to Carnap. Translation into the formal mode demonstrates this, and so dissolves the dispute.[10]

B.2.4 Problems: Application to Informal Discourse

How successful is Carnap's reconstruction of philosophical inquiry and disputes as a syntactical enterprise? Wagner (2009*b*, p. 195) observes that historically Carnap's scheme had "a limited impact, to say the least" on discussions in the philosophy of mathematics. And this is certainly true, even with regard to ontological questions. On the other hand, in his contribution to the Carnap-Schilpp volume Bar-Hillel takes at least the general idea of Carnap's method to be of the utmost importance:

> If I had to point out what I regard as the greatest single achievement of Logical Empiricism (and of Analytical Philosophy in general), I would not hesitate to declare that this greatest achievement consists in establishing and corroborating the thesis that many, if not most, philosophical controversies are not, as they are commonly regarded by participants and onlookers alike, theoretical disagreements on questions of fact (of a scientific, or ethical, or aesthetical, or ... nature) but rather disagreements [...] on the kind of linguistic framework to be preferably used in a certain context and for a certain purpose. (Bar-Hillel, 1963, p. 533)

What Bar-Hillel is endorsing here is really just a statement of Carnap's logical pluralism—that philosophical assertions should be formalized and relativized to some language or set of languages—rather than the particular formal procedures just described for identifying and translating material mode sentences into the formal mode.

Problem: Discourse Involving Reference

There are at least two significant problems with these formal procedures. The first regards the unnaturalness of its translations in certain cases. Although the translations we have encountered as examples so far may be quite plausible, other examples stretch credibility. This is especially the case with pseudo-object sentences involving

[10] As Carnap says: "All pseudo-questions of this kind disappear if the formal instead of the material mode of speech is used, that is, if in the formulation of questions, instead of universal words (such as 'number', 'space', 'universal'), we employ the corresponding syntactical words ('numerical expression', 'space-co-ordinate', 'predicate', etc.)" (*Logical Syntax*, p. 311)

reference or other semantic concepts.[11] Part (b) of Carnap's thesis reviewed above is that the logic of science is the *syntax* of the language of science. So Carnap can treat sentences including the notion of reference in one of three ways: (i) As nonsense like certain ill-formed metaphysical statements, which seems incorrect; (ii) As object-sentences, which strongly implies many of the ontological theses (or at least questions about them) that Carnap develops his program specifically to avoid; or (iii) As translatable into the formal mode, which thus seems the only reasonable option.

Carnap's favourite example is the sentence 'Yesterday's lecture was about Babylon' (*Logical Syntax*, p. 286). This is a pseudo-object sentence because, according to Carnap, it appears to assert something about Babylon the city. Carnap argues that this is not so, since our knowledge of the immediate properties of the actual town of Babylon cannot be affected by the truth-value of this sentence. The proposed formal mode translation of the sentence is:

> In yesterday's lecture either the word 'Babylon' or an expression synonymous with the word 'Babylon' occurred. (Ibid.)

This translation is striking in its inadequacy. It is simply not the case that the syntactical sentence captures anything like the intent of the original material mode sentence. For example, the word 'Babylon' may not have been uttered once in yesterday's lecture, although the lecture may yet have been about the city. Similarly, one may utter the word 'Babylon' simply to state that it is a topic not to be covered in the lecture.

As noted above, the problem is just that some formal mode translation is required so that semantical sentences are not considered nonsense. Translations such as these do seem the best possible given the constraints of Carnap's concepts in general syntax. But in the end they are inadequate, and so this speaks to a problem with naturalness of Carnap's reconstruction. We will see below that Carnap's eventual "semantic turn" is welcome in this regard.

[11] It is important to note that the concept of *truth* is a special exception. In *Logical Syntax* Carnap rejects the notion of reference in philosophical discourse because he thinks it leads to pseudo-questions and can be eliminated via translation into the formal mode. His rejection of truth is quite different. As mentioned in the Logico-Mathematical Interlude, Carnap develops all of the machinery necessary to construct an equivalent to Tarski's truth-definition, but he does not do so. This seems to be because he requires his meta-languages in *Logical Syntax* to be entirely analytic. Thus Carnap had no way to develop a criterion of adequacy for a truth-definition that could include descriptive expressions, and seems to have not seen how his meta-languages might be extended to incorporate a full truth-definition. This causes him to reject the concept truth as non-logical, and so an illegitimate syntactical concept. It is not that questions of truth are apt to mislead, it is rather that Carnap thinks the concept 'truth' has no syntactical correlate.

Problem: Pseudo-Object Sentences in Informal Discourse

The second problem with Carnap's procedure regards our ability to identify pseudo-object sentences. The machinery Carnap utilizes for this purpose—the notion of a quasi-syntactical sentence and the distinction between the material and formal modes—are concepts constructed in general syntax for a variety of purposes.[12] When they are applied in Part V of *Logical Syntax* to the identification and analysis of pseudo-object sentences however, we are then applying these concepts designed for a formal system to our regular, informal language—the language of discourse for most philosophy, even as practiced in the Logic of Science. Carnap is clear about this:

> Since the original sentence, in most cases, cannot be understood univocally, a particular translation into the formal mode of speech cannot univocally be given; it cannot even be stated with certainty that the sentence in question is a pseudo-object-sentence and, hence, a sentence of the material mode of speech. The translation given here is accordingly no more than a suggestion and is in no way binding. (Ibid., p. 302)

The problem is that our informal language does not have a well-defined set of formation rules. Thus there is no definite answer as to whether a sentence is quasi-syntactical, or as to whether a particular expression is a universal word.

This situation has the potential to limit the applicability of Carnap's reconstruction of philosophy. Again, Carnap is clear about this limitation:

> Since the rules of syntax of the word-language are not exactly established, and since linguistic usage varies considerably on just this point of the generic classification of words, our examples of universal words must always be given with the reservation that they are valid only for one particular use of language. (*Logical Syntax*, p. 293)

However, Carnap maintains his stance that translatability into the formal mode is the criterion for meaningful philosophical discourse. While our translations will not be completely determined, it is the responsibility of the philosopher forwarding a philosophical thesis to make that thesis as clear as possible:

[12] Carnap puts these concepts to use first in the analysis of intensional sentences, arguing that they can be treated extensionally (§68). This analysis is then extended to modal sentences (§69) in a way similar to Carnap's later treatment of modal operators in *Meaning and Necessity* ([1950]1956) using C-Terms ('S is logically impossible' is taken as equivalent to 'S is contradictory', 'S is physically necessary' is equivalent to 'S is P-Valid', etc.).

> It is the task of anyone who wishes to maintain the philosophical thesis in question to interpret it by translating it into an exact sentence. This latter may be sometimes be a genuine object-sentence (that is to say, not a quasi-syntactical sentence); and, in that case, no material mode of speech occurs. Otherwise it must be possible to give the interpretation by means of translation into a syntactical sentence. (Ibid., p. 302)

Carnap's stance here can be interpreted as an appeal to the Principle of Tolerance, which suggests that we make our theses clear by presenting them in the form of syntactical rules. Interpreted in this way, Carnap's definitions in Part V of *Logical Syntax* provide a schema for applying this methodology to philosophical debates, and showing us by example how we might transform philosophical theses into questions of logic or language choice.

Tolerance in Philosophy

How does this compare to the Deflationary reading? Goldfarb and Ricketts focus their attention on Carnap's philosophy of mathematics, saying very little about Carnap's project as applied to traditional philosophical statements (insofar as those statements are not about mathematics). Extending their reading somewhat, it is natural to suppose that Carnap is indeed appealing to Tolerance by suggesting the use of the translation schema we have just presented. Carnap's idea is to discard wearisome philosophical controversies by acquiescing to an attitude of Tolerance, and so suggesting we propose linguistic frameworks instead. However, as with Carnap's philosophy of mathematics, this is not the most persuasive interpretation of what Carnap is suggesting, especially to one not already on board with the project.

In lock-step with our understanding of Carnap's philosophy of mathematics from chapter 2, we can alternatively interpret Carnap's work here as a reconstruction of the practice of philosophy, or at least that part of philosophy which can be done in a scientific mode. The goal is the methodologically motivated explication of philosophically legitimate concepts and practices, which we saw Carnap construe rather narrowly both in our first chapter and directly above. Object questions, including those epistemic and semantic questions which can be treated by psychology and linguistics, are best handled by empirical science according to Carnap. The examples and concepts developed in Part V of *Logical Syntax* then aim to make plausible the idea that we might formally treat the remainder. Metaphysical questions are banished from *Logical Syntax* without much argument, as we saw. What is important is that the analysis of these questions occurs before any appeal to the Principle of Tolerance. Regarding the problem of determining whether some use of a universal word really is quasi-syntactical, on this reading Carnap would acknowledge that it is inevitable that we will encounter ambiguity and vagueness in our language at some level of our inquiry. Rather than being a problem however, on our interpretation confrontation of our informal discourse becomes an element of Carnap's program, since explications by definition work to rigorize some informal domain.

I thereby take Carnap's discussion of philosophy in Part V to serve three related purposes: (i) The formal concepts he develops for the identification and analysis of pseudo-object sentences act as his *explicata*; (ii) His examples and discussions serve to demonstrate the adequacy of this characterization of philosophical inquiry—that his scientific philosophy is suitable and translation of philosophical assertions into the formal mode capture the essence of these debates; and (iii) To *justify* the application of Tolerance in philosophical debates by making plausible the idea that such debates can indeed be treated syntactically rather than as object-level inquiries. So as with his philosophy of mathematics, we again find that Carnap's program neednot rest upon the Principle of Tolerance directly, but instead upon an appeal to the methodological structure of science—in this case philosophy. It is the plausibly formal nature of this domain which then licenses the use of Tolerance.

B.3 Linguistic Frameworks and Abstract Entities

As mentioned above, Carnap's embrace of semantics in the mid-thirties resolves several of the other issues reviewed. The awkward syntactical translations of sentences including semantical concepts are no longer necessary. Instead, sentences involving the notion of reference and other such semantic concepts, including 'truth', can now be counted legitimate and given a straightforward formal semantical analysis, as they should. The treatment of universal words also receives an explicit update in *ESO*.

This article introduces Carnap's famous distinction between *internal* and *external* questions, as well as the notion of a *linguistic framework* that we have been using throughout our discussion.[13] The purpose of the article is in a sense to confront the

[13]I have been following Goldfarb and Ricketts in using 'linguistic framework' as synonymous with Carnap's term 'language' from *Logical Syntax*. We will see below that this is not entirely correct, since in *ESO* Carnap uses the notion of a linguistic framework only with regard to the treatment of abstract entities. Still, it is easy enough to extend the notion to include the rest of the elements in Carnap's project of language construction—as we have implicitly done—since this extension does not conflict with Carnap's use of the term in *ESO*.

problem of empiricism, but with regard to the use of expressions that ostensibly refer to abstract entities—properties, numbers, classes, propositions, etc. Recall that the problem of empiricism is to account for our seemingly non-empirical knowledge of logic and mathematics within the context of a robust empiricist epistemology. The problem *ESO* confronts is just that since Carnap now admits semantical discourse that involves extra-linguistic reference to objects which are arguably not elements of our experience, reference to abstract entities in our theories suggests the non-material *existence* of such entities. This reference thereby raises the question of providing a philosophical account of abstract entities—be that a platonism, nominalism, intuitionism, modal structuralism, etc. In other words, the introduction of semantics seems to return us to the very ontological and foundational questions that Carnap's meta-philosophical program was meant to discard as pseudo-questions.

Notice that expressions which might raise ontological questions about abstract entities were addressed in *Logical Syntax* through the identification of material mode sentences involving universal words, and so by their transformation into syntactical sentences. Questions about the ontological status of the referents of universal words are thus taken to be pseudo-questions, because upon analysis sentences involving universal words are interpreted to be not about objects, but only about linguistic expressions. Adopting a semantical program requires Carnap to reject this strategy, since it is no longer a mistake to think that universal words are referring to objects—according to our semantic theory, the numerals, for example, do indeed refer straightforwardly to numbers. This means that many sentences identified in *Logical Syntax* as pseudo-object sentences and translated into the formal mode are now legitimate object-sentences. The problematic for Carnap is to develop another criterion which demarcates tractable philosophical questions from intractable pseudo-questions, and to show it plausible that the legitimate philosophical questions are amenable to a resolution on the basis of language choice.

In *ESO* Carnap suggests that if we wish to speak about a certain sort of entities, we must introduce "new ways of speaking" (p. 206) into our language to accommodate these new entities. This involves first the introduction of new rules into the language, governing the sentences we may form about such entities. This step will include the introduction of a new higher-level predicate for the entities, which allows us to say of a particular entity that it belongs to the new type. These higher-level predicates are analogous to the function of universal words in *Logical Syntax*—they obtain of the expressions in question. The difference of course is that they can indeed *refer*, so there is no need for a translation into the formal mode. The second step is the introduction of new variables the values of which are the entities in question.

Carnap calls this procedure the introduction of a new linguistic framework for the entities, and distinguishes between two sorts of existence questions we may ask regarding our newly introduced entities: (i) Questions regarding the existence of these new entities *within* the framework; and (ii) Questions regarding the existence of the *framework as a whole*. The first sort are internal questions and are amenable to usual mathematical or empirical methods. The latter are external questions, which will be analogous to Carnap's *syntactical questions* from *Logical Syntax*.

Carnap gives as an example the Thing-Language (*viz.*, the framework of things), which is akin to our informal, colloquial language when discussing our observations of everyday medium-sized objects, although it can be extended with the introduction of space-time points, etc.[14] We do not explicitly adopt this framework, it rather manifests naturally in the course of the development of our communication abilities. We do however have the choice to keep using this framework for addressing our basic experiences, or to otherwise adopt, say, a phenomenological framework toward the same ends. In the thing-language we can ask questions about whether a particular book has a certain property, or whether a particular captain of the Starship Enterprise exists, etc. Such cases can be resolved by typical empirical methods. In the case of the Number-Language constituting our manipulation and use of the natural numbers and basic cardinality concepts, etc., questions such as whether the number five is even, or whether there is a prime number greater than one million, can be answered by typical mathematical methods—as Carnap says, "by logical analysis based on the rules for the new expressions." (p. 209)

These are all internal questions. As noted, existence questions of this sort can be settled by established methods. In the case of the thing-language, our methods are the conscious or unconscious evaluation of our observations according to certain rules of confirmation—recall our discussion of Carnap's notions of observation and confirmation from our previous chapter. This is not to say that internal existence questions are necessarily trivial however. Even in the case of internal existence questions in an analytic domain, an answer may not be forthcoming. For example, whether there exists an even integer greater than 2 that cannot be expressed as the sum of two primes is still an open question. What is important is that there are well-known and established *methods* for answering internal questions, regardless as

[14] In Carnap ([1954]1958, §39) he provides a more rigorous axiomatic treatment of various possible thing languages and their applications.

to whether an answer is forthcoming or even possible.[15]

Carnap tells us that the concept of "reality" occurring in thing-language internal questions is a scientific, non-metaphysical one:

> To recognize something as a real thing or event means to succeed in incorporating it into the system of things at a particular space-time position so that it fits together with the other things recognized as real, according to the rules of the framework. (p. 207)

This is in distinction to external questions about the reality of *things* themselves—questions about the system of things as a whole:

> In contrast to the former questions, this question is raised neither by the man in the street nor by scientists, but only by philosophers. Realists give an affirmative answer, subjective idealists a negative one, and the controversy goes on for centuries without ever being solved. And it cannot be solved because it is framed in the wrong way. To be real in the scientific sense means to be an element of the system; hence this concept cannot be meaningfully applied to the system itself. (Ibid.)

Carnap suggests that external questions are rather a matter of practical decision, resolved by making choices as to the structure of our language. This decision is a matter of our goals and certain practical concerns: e.g., the fruitfulness, expedience, and simplicity of adopting the framework in question. As noted above, we may choose to abandon the thing-language as a means to describe our basic observational experiences if we found a phenomenological-language to be more fruitful.

Notice the similarity in method here to Carnap's earlier discussion of this same debate in *Logical Syntax*, which we introduced as an example in our first chapter. What is added in *ESO* is the idea that acceptance of some framework includes the tacit acceptance of the entities introduced by that framework. Recall that in *Logical Syntax*, the discussion remained at an entirely formal, syntactical level. But even this new acceptance of semantical reference for Carnap amounts to no more than the

[15]Notice that our discussion here entails that the internal/external distinction does not map onto the analytic/synthetic distinction. Internal existence questions may be analytic or synthetic depending upon the methods of investigation relevant to the question (*ESO*, p. 214). Furthermore, Carnap distinguishes between logical and factual systems (*ESO*, p. 208), with the number-language being logical, while the thing-language is factual as expected. Again, this distinction occurs *prior* to Carnap's analysis of questions as internal or external. Finally, note the expansion of object-level questions to those involving purely analytic domains—internal questions are what Carnap used to call object-level questions. This is a result of Carnap's adoption of semantics.

use of the expressions and rules which are involved in the investigation of internal questions as facilitated by our acceptance of the framework in question.[16]

B.3.1 A Non-Formal Criterion

What of the further ontological or metaphysical questions that one might ask? For Carnap, these are simply settled by the acceptance or rejection of certain ways of speaking. The question of demarcation is, however, more subtle. As mentioned, Carnap's adoption of semantics requires him to give up the formal machinery that we reviewed above. The purpose of Carnap's translational scheme in *Logical Syntax* is to make plausible the idea that traditional philosophical questions can be fruitfully construed as matters of language choice. This is because such questions can be shown to be syntactical, rather than object, questions. The legitimacy of semantical reference means that we cannot use the notion of a quasi-syntactical sentence to identify pseudo-object sentences—universal words really do refer to objects, so it is incorrect to interpret every pseudo-object sentence as really being a syntactical sentence. Instead, the question becomes one as to what Carnap's notion of 'reference' amounts. Ricketts (1996, pp. 247–248) argues that the situation here makes Carnap's distinction between semantics and metaphysics "delicate indeed"; presumably because Carnap effectively gives up the ability to construct a formal criterion for identifying pseudo-object sentences, and so for demarcating the metaphysical (in the pejorative sense) from the scientific-but-analytic.

Instead, in *ESO* Carnap simply asserts that we should interpret traditional philosophical questions as external. The reason for this is because, in contrast to internal questions, there seems to be no common criterion nor any possible evidence that would convince philosophers advocating for distinct frameworks to come to some agreement. For example, regarding the debate between realists and nominalists over the existence of numbers, Carnap says:

> I cannot think of any possible evidence that would be regarded as relevant

[16]Consider: "If someone decides to accept the thing language, there is no objection against saying that he has accepted the world of things. But this must not be interpreted as if it meant his acceptance of a *belief* in the reality of the thing world; there is no such belief or assertion or assumption, because it is not a theoretical question. To accept the thing world means nothing more than to accept a certain form of language, in other words, to accept rules for forming statements and for testing, accepting, or rejecting them. The acceptance of the thing language leads, on the basis of observations made, also to the acceptance, belief, and assertion of certain statements. But the thesis of the reality of the thing world cannot be among these statements, because it cannot be formulated in the thing language, or, it seems, in any other theoretical language." (*ESO*, p. 208. Original emphasis.)

> by both philosophers, and therefore, if actually found, would decide the controversy or at least make one of the opposite theses more probable than the other. (p. 219)

He thus suggests that the external question is in fact a pseudo-question, but not solely because it can be interpreted as being free of empirical content. Instead it is a function of the methods that we can bring to bear upon its investigation and resolution. So contra Ricketts, *ESO* retains a principled means for drawing a distinction between legitimate theoretical questions and questions that should be a matter of convention. This is no longer a completely *formal* criterion, but it is still a useful tool for the analysis of philosophical debates. And in fact, it comports well with our interpretation of Carnap's program, since the application of the notions developed in *ESO* rests fundamentally upon the methods that can be employed in the investigation of various questions.

Example: The Atomic Hypothesis

The idea that internal and external questions can be distinguished by the methods we might bring to bear upon attempting to answer them is highlighted by an example showing that we must not confuse the adoption of a new framework with the settling of an internal empirical or mathematical question. Penelope Maddy (2007) reads Carnap's discussion in *ESO* as entailing that it incorrectly reduces hard-won empirical discoveries like the existence of atoms to a conventional question of framework adoption—in this case whether to adopt an "atom framework". This calls into question the adequacy of Carnap's account, since history shows that it was clearly not a purely conventional matter that determined the outcome of the atomic hypothesis.

William Demopoulos (2013, chp. 3) convincingly argues that it was not Carnap's intent to reduce such questions to matters of convention, nor is this a consequence of his discussion in *ESO*. In the first place, the idea that Carnap requires an "atom-framework" to address questions as to the existence of atoms, or to even consider the evidentiary basis of the atomic hypothesis, is an extrapolation from *ESO*.[17] More importantly, Demopoulos argues that the question as to the existence of atoms is most

[17] Carnap does not address the atomic hypothesis at all, but he does seem to imply that the existence of sub-atomic particles (and so likely also atoms) is best interpreted as a non-trivial internal question when he says: "The critics of the use of abstract entities in semantics overlook the fundamental difference between the acceptance of a system of entities and an internal assertion, e.g., an assertion that there are elephants or electrons or prime numbers greater than a million. Whoever makes an internal assertion is certainly obliged to justify it by providing evidence, empirical evidence in the case of electrons, logical proof in the case of the prime numbers." (*ESO*, p. 218)

naturally interpreted as splitting into two distinct questions according to Carnap's method of analysis in *ESO*: (i) An internal question regarding the application of our ordinary empirical methods; and (ii) An external question regarding the nature of our scientific theories. Demopoulos stresses that during the 18th and 19th centuries it was indeed an open question as to whether it would be possible, using typical scientific methods, to make a convincing case for the existence of atoms as opposed to their being considered merely a useful fiction. After Einstein and Perrin's work on Brownian motion at the beginning of the 20th century however, those who had previously denied that this was possible were shown to be incorrect.

What is important here is that these scientists (along with those that argued that confirming empirical evidence would eventually be forthcoming) were making a speculative guess as to the future course of scientific investigation and the strength of typical scientific methods. While these sorts of speculations are candidates for external questions, they are importantly distinct from questions which "*fall altogether outside the purview*" (Demopoulos, 2013, p. 57) addressed by our typical scientific methods. In the case of the atomic hypothesis, such a tangential question is the further meta-theoretical question regarding the relationship between empirical adequacy and truth, or in other words a question regarding the philosophical controversy between realists and instrumentalists over the ontological import of our scientific theories. One can speculate as to the ability of typical scientific methods to eventually be able to discover evidence confirming or infirming the atomic hypothesis without thereby addressing this further controversy. Indeed, this further question seems completely intractable with regard to typical scientific methods—they do not bear upon the question at all, and it is for this reason that such a question is fruitfully deemed external by Carnap, and so not a matter for theoretical investigation.

That this was the way that Carnap intended to distinguish between properly scientific and "metaphysical" or traditional philosophical questions is evidenced by the passage quoted above regarding the debate between realists and nominalists. Carnap dismisses this controversy as a pseudo-debate because there seems no possible evidence relevant to both sides that might move the debate forward. Demopoulos suggests that it is not hard to extrapolate Carnap's explicit statement on this debate to the analogous realism-instrumentalism controversy. With regard to Maddy's concern, this methodological criterion for the demarcation of internal from external questions means that Carnap's program need-not make the existence of atoms a conventional matter. There is no need to explicitly adopt any sort of "atom framework". Indeed, Carnap's notion of a framework as determining a particular *type* or *sort* of entity is not meant to be so specific. The thing-language, supplemented with the usual empirical methods, is all we must adopt in order to to make the question of the existence of atoms an internal one—allowing us to acknowledge the significant empirical and theoretical work done since the time of Dalton to eventually answer the question in the affirmative.

B.3.2 Lingering Questions

Thus it is the methodological analysis of our investigations which serve as an informal criterion for distinguishing between internal and external questions. The explication of the theories and concepts of science thus remains of key importance in helping us to distinguish between questions amenable to typical scientific methods, and those that seem to be intractable. The concepts developed in *ESO* can then be utilized in the dissolution of philosophical debates, especially those concerning ontological questions about the referents of our theory. While the application of these concepts and this procedure is not as definite as those developed in *Logical Syntax*, the mature approach arguably results in a more natural analysis.

But what of the further, ontological question that one might be tempted to ask: Do atoms *really* exist? Should we choose not to adopt a suitably supplemented thing-language, it may so happen that we arrive at a different answer as to the existence of atoms. Similarly, recall that a linguistic framework (in Goldfarb and Ricketts' sense) includes the methods of justification and rules of evidence for our scientific practices. So is not the existence of atoms still, in the end, a conventional matter?

Regarding the ontological question, as noted above for Carnap this amounts to a question of the acceptance of a framework—there is simply no more to it than that. Considered internally to some framework, ontological questions are decided on the basis of the standard methods employed by our scientific practices. When considered externally, ontological questions are not well-specified. On the question of conventionality, as in our discussion of Carnap's philosophy of mathematics, we must remember that Carnap's overall meta-philosophical program is *reconstructive*. One of its primary goals is the adequate characterization of the actual methods and practices of science, including the evidentiary basis of our scientific theories. We may indeed choose as the language for our reconstruction a phenomenalistic rather than a physicalist language, but for Carnap, this changes little regarding the empirical content of the scientific theories we are reconstructing. Similarly, while we might choose a realist or an instrumentalist language, this decision should not skew too far the informal relations between evidence and justification characteristic of the actual scientific practices we are attempting to reconstruct. In other words, if our choice of language requires that we deviate grossly in the representation of the scientific story as it occurred, then it has fundamentally failed as a reconstruction, and will likely not provide the kinds of methodological and epistemic insights that Carnap foresees.[18] Thus we again find that our informal scientific practice constrains our choice of linguistic framework, bounding the scope of the Principle of Tolerance because of the overarching aims of Carnap's program.

[18]This is not to suggest that a rational reconstruction must slavishly describe intellectual history as it occurred, or characterize a concept in the most faithful way possible. As discussed in our chapter 2, Carnap's notion of explication is more flexible than this, since the point is to develop more precise concepts which can ultimately replace our informal ones. Similarly then, we may suggest that a rational reconstruction of some episode in the history of science should indeed skew things to the extent that the point of interest is brought into sharper relief than is the case given a flatter reading of the history. The appropriate amount of license to take will have to be determined on a case-by-case basis, as with the appropriate bounds upon an explication.

Chapter 4

The Boundless Ocean of Unlimited Possibilities: Tolerance in Foundations

The first attempts to cast the ship of logic off from the terra firma of the classical forms were certainly bold ones, considered from the historical point of view. But they were hampered by the striving after 'correctness'. Now, however, that impediment has been overcome, and before us lies the boundless ocean of unlimited possibilities.

Rudolf Carnap, *Logical Syntax*, p. xv

In this final chapter we take seriously Carnap's meta-philosophical program and the methodological insights upon which it rests, exploring how the program's application might be extended to contemporary issues in the philosophy of mathematics. I note that this chapter will be much more speculative than what has come before, and should be taken as no more than an investigation into the fruitfulness of extending Carnap's ideas. I should also note that besides a brief review of the definition of a category, the technical details of set- and category-theoretic foundations, as well as Hellman's own modal structuralism, will be largely omitted. This is because development of the required technical apparatuses would be lengthy and seems unnecessary for an analysis of the philosophical points at hand.

We begin with an overview of set and category theory as they have been employed in furnishing a foundation for mathematics. With regard to category theory, we focus especially upon Awodey's *Category-Theoretic Structuralist* (CTS) program, since this is Hellman's (2003) primary target. In the second section discussion moves to some historic ontological concerns with a traditional set-theoretic program. We also briefly review more recent proposals of so-called structuralist programs as a means to allay these concerns. The reason for this detour is that Hellman (2001) offers several pointed criticisms of these more recent programs. Our analysis will highlight that Hellman's primary concern is that a foundation for mathematics provide a suitable background ontology for our mathematical theories. From the Carnapian standpoint we have developed, these are the wrong sorts of questions to ask of a philosophical foundation for mathematics. We are also interested in structuralism insofar as Hellman and Awodey's programs fall under this banner. These discussions will thus act as a precursor to §3, which details Hellman's (2003) criticisms of Awodey's program specifically. Hellman levies three objections against this program, which he helpfully labels: (i) The problem of autonomy; (ii) The problem of the home-address; and (iii) The problem of extraordinary structures. I round out this section with a brief review of Hellman's modal structuralist program, to understand what he thinks a foundation for mathematics requires in order to successfully overcome these problems.

In the fourth section we leverage our discussion of Carnap's meta-philosophy to provide alternative responses to Hellman's problems. In essence I argue that, interpreted in a way concordant with Carnap's meta-philosophy, none of these issues need-be seen as problems. The problem of autonomy can be treated in a way analogous to our suggested Carnapian answer to the circularity objections. Our analysis of the problem of the home-address will show that Carnap's method for addressing ontological questions likewise proves fruitful. On the more general question of a dispute between category- and set-theoretic foundations, I suggest that there are sound methodological reasons to extend Carnap's approach to the debate between classical mathematics and intuitionism in the direction of this more recent controversy. Finally, in §5 I summarize and review the conclusions of our entire study.

4.1 Sets and Categories in Foundations

In the previous chapters we have had occasion to briefly review the "big three" traditional foundational programs of *logicism*, *finitism*, and *intuitionism*. What these programs have in common is that they are all attempts to provide some unifying framework for our informal mathematical practice. In other words, they attempt to reduce or otherwise explain mathematical truths and concepts by means of some fundamental collection of basic notions and principles. In the case of logicism this is either Russell's notion of class (or propositional function), or Frege's notion of the extensions of concepts, along with the axioms of a logical language that are taken by Frege and Russell to be indubitable and maximally general. Hilbert takes as fundamental our immediate perceptual intuition of concrete symbols, along with the meta-mathematical methods of finitary proof theory.[1] Intuitionism is a bit of a special case, since Brouwer does not aim to account for the entirety of our informal mathematical practice, instead arguing for a restriction of that practice to methods which are constructive. But again, the fragment of mathematics considered legitimate is accounted for with appeal to some fundamental notions and principles, in this case non-perceptual, intuited objects and constructions, which are taken to be introspectively self-evident.[2]

In this regard each program offers an epistemological and semantic account of our knowledge of mathematical truths and the objects to which they refer, thereby also explaining what makes mathematical theorems true. In the logicist and finitist cases, some privileged set of objects is posited as the subject of our mathematical sentences, and our recognition of their truth is partially explained with reference to our grasp or understanding of these objects. In the case of intuitionism—again the odd one out—mathematical truth seems more subjective, since it is dependent upon our faculty of intuition and what is constructible.[3]

[1]Consider: "[...]something must be given in conception, viz., certain extralogical concrete objects which are intuited as directly experienced prior to all thinking. For logical deduction to be certain, we must be able to see every aspect of these objects, and their properties, differences, sequences, and contiguities must be given, together with the objects themselves, as something which cannot be reduced to something else and which requires no reduction. [...] The subject matter of mathematics is, in accordance with this theory, the concrete symbols themselves whose structure is immediately clear and recognizable." (Hilbert, [1926]1983, p. 192)

[2]According to Brouwer, intuitionistic mathematics has "its origin in the perception of a move of time, i.e. of the falling apart of a life moment into two distinct things, one of which gives way to the other, but is retained by memory. If the two-ity thus born is divested of all quality, there remains the *empty form of the common substratum of all two-ities*. It is this common substratum, this empty form, which is the *basic intuition of mathematics*." (Brouwer, [1952]1975, p. 510. Original emphasis.) Placek (1999, pp. 27–28) insightfully observes that Brouwer's conception of *intuition* changed somewhat from the time of his doctoral thesis ([1907]1975) to his later writings. Rather than focusing upon the succession from one sensation to another, his earlier work places the focus upon continuity and discreteness as complementary, equally primitive notions. As a result, the continuum was also taken as primitive and indefinable. With the later discovery of *species* and *spreads* as a means to define the reals, the idea of the continuum as primitive was abandoned.

[3]Cf. Placek (1999) for an extended discussion of the extent to which mathematical truths can be considered intersubjective according to various intuitionist programs.

A related question concerns the metaphysical or ontological status of these privileged objects. We might be platonists or nominalists, take our objects of mathematical discourse to be mind-dependent or mind-independent, etc. The foundational programs just surveyed are often thought of as supplying answers to such questions as well. But, as discussed in the Logico-Mathematical Interlude, with Gödel's theorems generally considered to have undermined the traditional forms of both logicism and finitism, and many philosophers and mathematicians being unwilling to adhere to the restrictions of intuitionism, the search for some other foundational program has brought such questions to the fore. Hellman's concerns with set-theoretic versions of structuralism, and with category theory, as foundational programs are along these lines. Thus, in order to understand his criticisms we move now to a discussion of these more modern foundational programs.

4.1.1 Set-Theoretic Foundations

Before discussing the varieties of structuralism proposed as foundational programs, it will be useful to say a few words about set theory *simpliciter* as a foundation. In line with the traditional foundational programs, set theory is often taken to provide a foundation in at least two ways.[4] In the first place it serves an important *unifying* purpose—most of mathematics can be reduced to the notions of *set* and *membership*. This provides an important grounding for mathematics, as whatever abstract, higher-order structures the mathematician may choose to explore, it is known that *mathematics* is still being done in some sense. Consider:

> [. . .]whereas in the past it was thought that every branch of mathematics depended on its own particular intuitions which provided its concepts and primary truths, nowadays it is known to be possible, logically speaking, to derive practically the whole of mathematics from a single source, the theory of sets. (Bourbaki, 1968, p. 9)

If one thinks of the axioms of set theory as furthermore having some special epistemic status—self-evidence, analyticity, freedom from contradiction, etc.—then second, such a reduction also provides an epistemic or explanatory *justification* for mathematics, in the sense of Gödel, Quine, *et al.*, as discussed in our second chapter.

Hand-in-hand with this reductionist project is often taken to be the more metaphysical one—that the reason mathematicians can feel safe in retreating to set theory

[4]Suppose a first-order formulation of the ZFC axioms within a suitable ambient logical language, as can be found in, for example, Suppes (1972).

when pressed for rigourization or justification of their practices is because it is taken to provide the *ontological* grounding for all of mathematics:

> Among the many branches of modern mathematics set theory occupies a unique place: with a few rare exceptions the entities which are studied and analyzed in mathematics may be regarded as certain particular sets or classes of objects. This means that the various branches of mathematics may be formally defined within set theory. As a consequence, many fundamental questions about the nature of mathematics may be reduced to questions about set theory. (Suppes, 1972, p. 1)[5]

The idea is that the cumulative hierarchy, or some other set-theoretic universe, provides the "background ontology" for the rest of mathematics. Mathematical existence claims about the natural numbers, all groups, etc., are then *actually* about sets in the cumulative hierarchy, which *really exist* in some (not necessarily platonistic) sense. This idea is exemplified in modern model-theory, a part of set theory.

On some views this orientation entails that questions regarding, say, the truth of the continuum hypothesis have a definite—even if not yet determinate—answer, independent of our formal mathematical theories. As Gödel says:

> It is to be noted, however, that on the basis of the point of view here adopted, a proof of the undecidability of Cantor's conjecture from the accepted axioms of set theory (in contradistinction, e.g., to the proof of the transcendency of π) would by no means solve the problem. For if the meanings of the primitive terms of set theory [. . .] are accepted as sound, it follows that the set-theoretical concepts and theorems describe some well-determined reality, in which Cantor's conjecture must be either true or false. Hence its undecidability from the axioms being assumed today can only mean that these axioms do not contain a complete description of that reality. (Gödel, [1947]1983, p. 476)[6]

The continuum hypothesis must have a definite truth value in the same sense as the hypothesis that Bismarck wore a moustache on some particular day. Whether we can

[5]And again: "Whatever can be said in the old-fashioned way in terms of 'abstract forms' and 'universals' can be reformulated much more precisely and simply in terms of sets, structures and formal languages. In this way we are spared the difficulty of saying just what sort of *things* those abstract forms and universals are. [. . .] Set theory simply banishes the problem of universals from the foundations of mathematics as irrelevant." (Mayberry, 1977, pp. 23–24. Original emphasis.)

[6]For a far more subtle discussion of Gödel's realism than I can possibly provide here, I again point the reader to Parsons (1995) and Tait (2001).

know these facts is a separate question regarding our epistemic circumstances. In the case of empirical facts, it is a matter of experience and empirical records. In the case of mathematical facts, for Gödel it is a matter of our having completely intuited the concepts involved. Just as we can come to gain new empirical facts through further investigation, we can gain a more complete understanding of mathematical concepts through mathematical investigation. This results in new axioms about the set-theoretic universe forcing themselves upon us as self-evident, and consequently we are able to more fully describe that independent mathematical reality.

From this foundational perspective, one can think of sets as the "atoms" of which mathematical objects consist, or as the "things" which mathematical theorems are *about*. To borrow a phrase from Marquis (1995), from this perspective mathematics can be seen as the "science of sets."

4.1.2 Category-Theoretic Foundations

Category theory, on the other hand, is often taken to furnish foundations in a somewhat different sense than either set theory or the more traditional programs.[7] Since its inception by Samuel Eilenberg and Saunders MacLane in the late 1940s,[8] category theory has certainly been employed as a *unifying* language, but usually to the end of facilitating research into the relationships between different mathematical structures, often bridging disparate branches of mathematics rather than in a strictly reductive sense as we saw with set theory. Part of the reason that category theory plays this role in modern mathematical practice is that the definition of a category is extremely general and widely applicable.[9]

Briefly, a *Category* consists of *Objects* $A, B, C, \ldots$ and *Arrows* $f, g, h, \ldots$, such that for each arrow f there is associated two objects, called the *domain* and *codomain* of f. Where $dom(f) = A$ and $cod(f) = B$, we write $f : A \to B$. For each pair of arrows $f : A \to B$ and $g : B \to C$, there is an arrow, $g \circ f : A \to C$, called the *Composite* of f and g. For each object A, there is an unique arrow, $1_A : A \to A$, called the *Identity Arrow* of A. These primitives must satisfy two axioms:

[7]Again, suppose a first-order formulation of the necessary axioms, as found in McLarty (1992). Here however, the ambient language assumed is a simple type-theory.

[8]The most relevant paper seems to be Eilenberg & Mac Lane (1945). Landry & Marquis (2005) offers a concise history of the theory's development. I rely significantly upon their discussion in this section, as well as the discussions in Awodey (1996) and McDonald (2012).

[9]My presentation in the next paragraph follows Awodey ([2005]2010) in all essential respects.

1. **Associativity:** For all $f : A \to B$, $g : B \to C$, and $h : C \to D$,

$$h \circ (g \circ f) = (h \circ g) \circ f$$

2. **Unit:** For all $f : A \to B$,

$$f \circ 1_A = f = 1_B \circ f$$

Anything that satisfies this definition is a category. Thus sets and functions on sets make up a category, so do groups and group homomorphisms, or the natural numbers and all recursive functions $\mathbb{N} \to \mathbb{N}$. We can also define a category of *proofs* for a given system of deductive logic closed under a consequence relation: Take formulae as objects and arrows as deductions from A to B. While it is natural to think of objects as sets and arrows as functions, importantly this is not necessary.[10]

These categories can also be related to each other. A *Functor*, F, is a mapping between categories $\mathbf{C}$, $\mathbf{D}$ that takes objects to objects and arrows to arrows in such a way as to preserve the structure of $\mathbf{C}$ in $\mathbf{D}$.[11] So not only do collections of categories themselves satisfy the definition of a category, but we find that we can use category theory to investigate the relationships between different sorts of structures in a very general way. This has led to the discovery of interesting connections between quite disparate branches of mathematics.

These applications were broadened by F. William Lawvere in his doctoral dissertation (1963), and in a series of papers shortly thereafter. Lawvere was the first to develop the idea of category theory as an autonomous foundation for mathematics, thinking about the theory's unifying role much like set theory above:

> [...]by "foundation" we mean a single system of first-order axioms in which all usual mathematical objects can be defined and all their usual properties proved." (Lawvere, 1966, p. 1).

In contrast to most proponents of set-theoretic foundations however, Lawvere did not

[10]For example, define a category which takes sets as objects and binary relations as arrows. In other words, an arrow $f : A \to B$ is an arbitrary subset $f \subseteq A \times B$. Identity arrows on an object are the identity relation, and we can define the composite of $R \subseteq A \times B$ and $S \subseteq B \times C$ as:

$$(a, c) \in S \circ R \quad \text{iff} \quad \exists b \text{ s.t. } (a, b) \in R \;\&\; (b, c) \in S$$

[11]Cf. Awodey ([2005]2010, pp. 8–9).

then think of foundational programs (and so *a fortiori* a category-theoretic foundations) as serving any sort of justificatory role:

> [...]Foundations in this sense cannot be identified with any 'starting point' or 'justification' for mathematics, though partial results in these directions may be among its fruits. But among the other fruits of Foundations so defined would presumably be guidelines for passing from one branch of mathematics to another and for gauging to some extent which directions of research are likely to be relevant. (Lawvere, 1969, p. 281)

One reason for this attitude toward foundations as a primarily methodological, as opposed to epistemological, project is that the axioms of category theory are *algebraic*, in contrast to the *assertory* axioms of set theory. So whereas the empty-set axiom or the axiom of infinity are usually taken as asserting or guaranteeing the existence of certain sets, the axioms specifying a category merely provide a *definition* of a certain kind of mathematical structure (a category), but do not guarantee the *existence* of anything satisfying the definition. In this respect the axioms of category theory are often compared to the axioms of group theory.

Category-Theoretic Structuralism

But what of the epistemic, semantic, and ontological questions for which traditional foundational programs are designed to provide answers? Ultimately, Awodey's CTS program approaches foundations from a very different direction—a direction suggested by Lawvere's comments quoted above. In Awodey (1996) his primary aim is to clarify the notion of 'mathematical structure'. The hope is that this may then be of use in developing a philosophical approach to mathematics more concordant with current mathematical practice. He suggests that category theory is likely a very useful framework in this philosophical task, but cautions that he is not promoting CTS as a comprehensive philosophical position, only suggesting that pursuing a philosophical foundations "using a technical apparatus other than that developed by logical atomists since Frege" (Awodey, 1996, p. 235) may prove fruitful.

The perspective that Awodey develops with CTS he characterizes as "top-down", as opposed to the traditional foundationalist's "bottom-up" approach. According to Awodey, mathematical theorems should be read as both *schematic* and *conditional* in form. So rather than begin with some privileged objects which serve as the fixed background ontology for our mathematical theories,

> [...]the 'categorical-structural' [view] we advocate is based instead on the idea of specifying, for a given theorem or theory only the required relevant degree of information or structure, the essential features of a given situation, for the purpose at hand, without assuming some ultimate knowledge, specification, or determination of the 'objects' involved. [...]
>
> Thus according to our view, there is neither a once-and-for-all universe of all mathematical objects, nor a once-and-for-all system of all mathematical inferences. (Awodey, 2004, p. 56)

So Awodey suggests that mathematics in fact needs no foundation with regard to such questions, that they are in a sense beside the point.

As an example Awodey asks how we might understand that $i^5 = i$ in the complex numbers on both a top-down and bottom-up approach. The foundationalist, working from the bottom-up, needs to first construct the complex plane, define multiplication on it, and single out the required entities. She must then prove that the entities thus constructed do in fact have the necessary properties. On the other hand, working from a top-down perspective, Awodey suggests we need-only consider that:

> (i) in any ring, if $x^2 = -1$ then $x^5 = x$, and
>
> (ii) the complex numbers are by definition a ring with an element i such that $i^2 = -1$, and having a couple of other distinctive properties. (Ibid., pp. 56–57)

From this $i^5 = i$ follows. More explicitly, the "foundationalist" as labeled by Awodey is required by her preconceptions of the subject matter of mathematics to universally quantify over a specific range of *objects* (usually sets in the cumulative hierarchy, as discussed above). These objects are fixed, comprising the absolute mathematical universe. We may prove the statement via (i) and (ii) from this perspective, but this "involves consideration of a possibly huge but fixed range of specific rings, as well as of a particular ring consisting of equivalence classes of pairs of Dedekind cuts of ..." (Ibid.) From the CTS perspective by comparison, one takes (i) and (ii) as providing a specification of the relevant context of the situation, which is used to focus down upon the particular structures of interest. Statement (i) is certainly interpreted as being about *any ring*, but this is not with regard to some fixed number of rings that exist in some *real* or *absolute* domain; rather, it is like an open formula.

As noted, the CTS perspective also takes mathematical theorems to be conditional in form, with an antecedent condition specifying the relevant context as we just saw. Awodey suggests that this is the general form of all mathematical theorems:

> Every mathematical theorem is of the form 'if such-and-such is the case, then so-and-so holds'. That is, the 'things' referred to are assumed to have certain properties, and then it is shown, using the tacitly assumed methods of reasoning, that they also have some other properties. [...] Of course, many theorems do not literally have this form, but every theorem has some conditions under which it obtains. (Ibid., p. 58)

This raises the question of a mathematical theorem's being *vacuously* true. Consider again the theorem above. Taking the theorem in the form: *if* (i), (ii), *then* $i^5 = i$, what if there are not *actually* any rings? This is possible, recall, because like the category axioms themselves the axioms defining a ring do not assert that any rings *exist*. It seems in this case that the statement would thus be true, but for the wrong reasons. The statement is true simply because nothing satisfies the antecedent, and so the conditional is automatically true. In this case $i^5 = -i$ would also be true by similar reasoning, and $i^5 = 1$, etc.[12]

In response Awodey asserts that, again, from the CTS perspective this is the wrong way to understand the content of mathematical theorems. Explaining the schematic nature of the CTS interpretation, we observed that (i) above is like an open formula rather than a proposition, but this is not to suggest that such antecedent conditions of a theorem need-be *satisfied* in the usual, Tarskian sense before we can consider the proof of the theorem. Instead, antecedent conditions act to specify a *range of application* for the consequent. In other words, the theorem "*applies* only to those cases specified by the antecedent description." (Ibid. Original emphasis.) The consistency of our antecedent conditions—definitions, axioms, etc.—is the *desideratum* on this perspective.[13] Where this consistency is in question, Awodey suggests

[12] Notice the similarity to Russell's if-then-ism here. Awodey explains the problem thus: "The argument against if-then-ism in this form is that it makes all theorems hypothetical; they can never really be known, because the antecedent conditions will always remain in doubt. We may never know whether the axioms of ZFC are true, or they may even be inconsistent, and so it will not do to carry them along as conditions on every theorem." (Ibid., p. 60)

[13] Landry & Marquis (2005) helpfully compare the divergent perspectives between traditional set-theoretic "foudationalists" and the CTS program to the Frege-Hilbert controversy in geometry. With regard to Hilbert's finitist program for arithmetic, it is important to recognize that his attitude was not entirely concordant with the CTS program. This is not just because Hilbert was unaware of category theory, but because his "deductivism" still bottomed-out with the assumption of primitive objects (see n. 1 above). So this is still in contrast to the top-down categorical perspective: "We see, then, that the category-theoretic meaning of a mathematical concept is determined only in relation to a 'category of discourse' which can itself *vary*. Thus the effect of casting a mathematical concept in category-theoretic terms is to confer a degree of *ambiguity of reference* on the concept." (Bell, [1988]2008, p. 237. Original emphasis.)

that our only recourse is to investigate the further consequences of those conditions.

So category theory offers an understanding of mathematics that is quite different from either the traditional foundational programs or more modern set-theoretic foundations. Advocates of this approach argue that the language of category theory is especially suited to treat and develop the notion of *structure*, something they argue is an essential characteristic of modern mathematical practice, but has often been ignored by the traditional programs. Consider:

> This difference arises more generally in the respective accounts that set theory and category theory provide of mathematical structure. Both set theory and category theory transcend the *particularity* of mathematical structures. Set theory *strips away* structure from the ontology of mathematics leaving pluralities of structureless individuals open to the imposition of new structure. Category theory, by contrast, transcends particular structure not by doing away with it, but by taking it as given and *generalizing* it. It may be said that the success of category theory as a unifying language for mathematics is due to the fact that it, and it alone, gives direct expression to the centrality of form and structure in mathematics. (Bell, [1988]2008, pp. 236–237. Original emphasis.)

We might suggest, then, that the CTS perspective takes mathematics to be the "science of structures", to borrow this time from Shapiro (2005). However, this may still be misleading, since it raises the question of whether the structures themselves exist, as independent, reified objects. But as we have seen, a top-down category-theoretic foundation attempts to avoid such questions completely:

> Whether abstract entities are admitted into one's ontology or not, mathematical theorems apply in either case—mathematics is not the arbiter of existence. If one considers a cube and is willing to speak of the symmetries of the cube as objects, those symmetries constitute a group of 24 elements. If one is willing to speak of the squares of a chessboard as objects, and the possible moves of a knight on the chessboard as arrows, the collection of such objects and arrows yields a category. Whether or not such objects are admitted is not a question that one's mathematical program should settle. (McDonald, 2012, p. 24)

So perhaps better, we might say that mathematics is seen from a CTS perspective as the science or investigation of structure *simpliciter*.

4.2 Structuralist Foundations

Many philosophers have recently recognized that mathematicians regard some notion of 'structure' as essential, and so have begun to develop foundational accounts which provide some philosophical interpretation for that notion.[14] In this regard Awodey (1996) distinguishes between *mathematical* and *philosophical* structuralism. The former is a methodological approach to the practice of mathematics which is exemplified by, but not unique to, category theory—what Awodey (2004, p. 54) characterizes as a "certain, now typical, 'abstract' way of practicing mathematics", in which mathematical objects are determined by their admissible transformations rather than by their specific features as viewed in isolation.[15]

The CTS program that we have just reviewed supposes category theory to thus far be the culmination of the methods employed by mathematical structuralism, and suggests that a philosophical account of mathematics would benefit substantially from drawing on these same methods and technical apparatus (i.e., category theory). So philosophical structuralism attempts to provide a philosophical account of mathematics on the basis of some notion of 'structure'. Excepting the CTS program, the aim of a philosophical structuralism is often to supplement the set-theoretic foundational program outlined previously by embedding it in some broader context or interpretation that systematically develops the notion of 'structure' in some way. Hellman calls these approaches *Set-Theoretic Structuralism* (STS). Alternative structuralist programs aim to furnish a completely novel framework for answering the traditional

[14]See, e.g., Resnik (1997), Shapiro (1997), Parsons (2009), and, of course, Hellman (1989) and Awodey (1996). §3 of Landry & Marquis (2005) covers the lay of the land in this domain very well, describing and comparing the various positions. Parsons (1990) offer some pointed criticisms of the general structuralist approach, while Hale (1996) forwards objections to Hellman's modal structuralism in particular. The latter paper derives from a special issue of *Philosophia Mathematica* (vol. 4, no. 2, May 1996) devoted to structuralism.

[15]Consider: "Few modern mathematicians are interested in any properties of the objects they study which do not respect a given, well defined notion of isomorphism. That is to say, the topologist does not care to distinguish among homeomorphic spaces by examining the set-theoretic structure of the points of a space, nor does the geometer wonder about the ordinal rank of a given smooth manifold." (Awodey, 1996, p. 212). And again, specifically referring to the virtues of approaching mathematics category-theoretically: "In modern mathematics, one studies abstract structures of various kinds; groups, rings, modules, topological spaces, topological vector spaces, and the like [...] what often happens is that interesting properties of a given species of structure can be recovered solely from the known properties of the operation of functional composition within the system of functions associated with the collection of structures of the given species. In this way, one can pay less attention to the individual sets and their elements once some of the basic properties of the system of functions are established. Moreover, it can be observed that almost all interesting systems of functions satisfy certain basic and simple properties, such as the associative property of the composition of functions." (Hatcher, 1968, p. 262).

ontological or epistemological questions about mathematical objects and structures, which Hellman calls *Sui Generis Structuralism* (SGS). In either case, the programs are motivated not only to account for this mathematical notion of 'structure', but also to overcome certain problems with a straightforward set-theoretic foundation.

4.2.1 Benacerraf's Problem

One of the most well-worn of these problems has been most famously presented by Paul Benacerraf ([1965]1983). Define the natural numbers in a usual, set-theoretic way. The number 3 is then constructed as a particular set in the cumulative hierarchy, which, following von Neumann, we can represent as $3 = \{\varnothing, \{\varnothing\}, \{\varnothing, \{\varnothing\}\}\}$. This construction captures the essential properties of the number 3; or in Fregean terms, our definition—once spelled-out—provides a criterion of identity for recognizing the number 3, and for its application in mathematical and empirical contexts.

But as it turns out this is not the only construction of the number 3 possible, even if we limit ourselves to Zermelo-Frankel set theory. Given a difference in how we decide to define the successor relation, we might equally well follow Zermelo and construct $3 = \{\{\{\varnothing\}\}\}$. The problem is this: Which 3 is then the *real* 3, in whatever sense of "real" is taken to be most appropriate? Benacerraf expands:

> For if the number 3 is in fact some particular set b, it cannot be that two correct accounts of the meaning of "3"—and therefore also of its reference—assign two different sets to 3. For if it is true that for some set b, $3 = b$, then it cannot be true that for some set c, different from b, $3 = c$. (Benacerraf, [1965]1983, p. 279)

Benacerraf concludes that the question is in a sense ill-formed, that the number 3 should not *really* be taken to be any particular object at all. Rather than focus on the particular construction of this or that mathematical object, or even the particulars of this or that mathematical structure, what are instead important are the structural properties which relate this object to the others in an ω-progression. What is thus "uniquely 3" is its place in a progression which has the internal relations necessary to instantiate a natural number object (i.e., any ω-progression), rather than the mathematical object which we designate to act as the term's referent in a particular context. Similarly for the natural number structure itself.

Benacerraf's problem is an ontological worry for a set-theoretic foundational program, especially one that approaches mathematics with a platonist attitude. The cumulative hierarchy is in a sense too rich, and so we are unable to offer principled reasons for supposing that our singular terms refer to some object rather than some other. But since our mathematical theories are supposed to be about specific sets in the cumulative hierarchy, this indeterminacy of reference is taken as an embarrassment. Benacerraf allays the problem with a shift in perspective not unlike that made by the CTS program: He suggests that the subject matter of mathematics is form or structure, rather than some distinguished set of objects whose nature must be completely characterized prior to the development of higher mathematics. While Benacerraf's sense of 'structure' here is not entirely clear, the general idea has been developed in a variety of different directions.

4.2.2 Philosophical Structuralism

In perhaps the most straightforward sense of the term, we can take a *structure* to be a set with certain relations or operations defined on it. Then STS is just our typical use of model theory to describe and investigate the mathematical structures which satisfy various sentences and axiom systems, as well as the relationships between various structures. The idea is that our mathematical theories are *about* classes (or sets) of models rather than individual sets. For example, the second-order Peano axioms provide a categorical characterization of the natural numbers—all models are isomorphic, and so interchangeable. Thus the subject matter of arithmetic is ω-progressions, rather than some *particular* ω-progression we call the natural numbers. So this understanding of mathematical theories approaches Benacerraf's problem by providing grounds for justifying Benacerraf's assertion that "any ω-progression will do", since they are all structurally identical.

Hellman (2001) outlines several problems for this version of philosophical structuralism, all arising from *ontological* considerations regarding the continued need to assume a fixed background of sets to supply the elements of our models. To take just one of his criticisms:

> There are also puzzles at the 'in-between' levels [of the cumulative hierarchy] that arise on the straightforward, realist-platonist STS. On that picture, what sets exist is a matter of objective fact, analogous to the matter of physical existence. Our stipulations can no more determine the richness of the real world of sets than they can the richness of the physical world. As Shapiro [1997, p. 131] points out, there is always the possibility, on the traditional platonist picture, that we might be system-

atically and radically mistaken in our beliefs about abstract mathematical objects. (Hellman, 2001, p. 187)

The worry is whether we can be sure that our notion of 'full power-set' *really is* maximally wide, whether it really captures all of the mathematical objects within the cumulative hierarchy that are there to be described at each level.

We also encounter another version of Benacerraf's problem, but with regard to entire set-theoretic universes rather than individual sets. The question is: Which cumulative hierarchy—and specified by which particular axioms—shall we take as our absolute domain? On the STS program, Hellman suggests, there is simply no way to arbitrate the situation.[16] There is also the problem of just how to incorporate set theory itself into our structuralist treatment. On pain of contradiction, the structure we call 'the cumulative hierarchy' cannot itself be a set, so we have failed to provide a comprehensive foundation. Fundamentally, Hellman supposes that positing some *fixed* universe of sets just seems to miss an important element of mathematical practice: the indefinite extensibility of the mathematical universe.[17]

Hellman presents the various SGS programs as improving upon the aim of providing a foundation by solving or avoiding these problems. A *Sui Generis* Structuralism replaces set theory with some completely novel framework specifically tailored to developing a philosophical interpretation of the mathematical notion of 'structure'. We can think of such programs as "abstracting again" from the model-theoretic level:

> [...]the structures with which mathematics is concerned are entities in their own right, akin in some respects to model-structures, but distinguished from them by the fact that their elements have no non-structural properties, but are to be conceived as no more than 'bare positions' in the structure [...] On this approach, an abstract-structure is just what is left when, beginning with a model-structure, we abstract away from all that is inessential, leaving behind only what is common to all other model-structures isomorphic to it. (Hale, 1996, p. 125)

Hellman focuses upon Shapiro's (1997) so-called *ante rem* structuralism. To take a specific example in this framework, the basic idea is to speak of *the* natural number structure, but interpreting *places* within the structure as themselves the objects to

[16]One possibility is to adopt something like Gödel's perspective, as discussed in the first section of this chapter. I will not however pursue this suggestion further here.

[17]As Hellman says: "How can any fixed ontology be adequate to a structural interpretation of all the set theories—or *collection* theories, if you prefer—we can concoct?" (Ibid.)

which the theory refers. Particular ω-progressions (what Shapiro calls "systems") will have those places "occupied" by some definite objects given by a particular background ontology (some particular sets, Julius Caesar, etc.), but the theory itself treats the unoccupied structure as an abstraction from all similar systems.

Formally, these structures are implicitly defined in a second-order logical language. Shapiro supplements this language with "structure existence" axioms that assert the existence of structures with enough "places" for whatever mathematical theory is of interest. These axioms amount to translations of the usual ZF axioms, but include a novel explicit existence axiom:

> **Coherence:** If Φ is a coherent formula in a second-order language, then there is a structure that satisfies Φ. (Shapiro, 1997, p. 95)

The notion of *coherence* is taken as a new primitive by Shapiro. It is akin to *satisfiability*, but extended past the point where the collections determined by our theories become too large for set theory. Hellman comments:

> The Coherence Axiom enters naturally into SGS, for this framework does not appeal to a fixed background ontology in making sense of structure-existence. Indeed, were it to do so, it could hardly improve on STS with regard to the puzzles and problems afflicting it as described above. (Hellman, 2001, p. 189)

Note that consistency does not entail coherence since the framework is second-order. Shapiro recognizes that the notion remains somewhat problematic.

Leaving aside the problem of how to determine whether formulae and axioms are coherent, Hellman notes that Shapiro's version of SGS addresses the problems observed for the STS program. For example, the above-raised extension of Benacerraf's problem is addressed because there is, strictly speaking, no presupposed fixed universe of sets. Instead, we speak of independently constituted structures, determined by our mathematical investigations. We can likewise admit ever-larger structures unproblematically—as long as the axioms describing them are coherent.

The platonist worry of our being systematically mistaken about the contents of the mathematical universe is addressed by the same considerations:

> In particular, there is no problem of the possibility of less-than-full power sets in set-theoretic structures. Of course, less-than-full structures exist in abundance, but so long as we can coherently speak of the full ones, we can investigate them. (Ibid., p. 190)

This also addresses the question of the objective truth of "so-far indeterminate" mathematical statements, like the continuum hypothesis. Hellman suggests that the question: *Continuum hypothesis?* is properly directed at just the maximally wide structures, and "these determine a unique correct answer, whether we can learn what it is or not." (Ibid.)

Hellman of course raises problems with the SGS program. What is interesting is that many of the problems he raises are just iterations of the same problems that plague STS and basic set-theoretic foundational programs. For example, while the *ante rem* structuralist does not need to worry about questions pertaining some maximal universe of sets, and she need-make no commitment to the totality of all possible structures, Hellman argues that such a structuralist is committed to some totality of all possible *places in structures*:

> Such a collection of all places exists by second-order logical comprehension (as the union of the place collections of structures), although it cannot be a *structure* on pain of contradiction. [...] And then it seems that such a collection ought to be able to occupy places in structures; after all, informally, structuralism says that anything whatever can fill a structural position. (Ibid., p. 191. Original emphasis.)

Since SGS is committed to an ontology of *places as objects*, we must still worry about how to treat collections of all such things. So SGS still runs afoul of the idea that our mathematical universe should be indefinitely extensible. What we find, according to Hellman, is that no contemporary foundational program has thus far been able to generate "enough" objects to account for all possible mathematical practice, and so each of these programs fails to adequately service the semantic and ontological concerns that a philosophical foundation should supposedly treat.

There is also yet another recurrence of Benacerraf's problem which arises partially for the same reasons as the problem just discussed, and partially because of the way a SGS program takes structures as abstractions from models. We can think of the natural number structure on the SGS program as some archetypal ω-progression with just those properties common to all ω-progressions. But because the SGS program takes places-as-objects, Hellman observes that the numerals then denote definite places in some *unique* structure, which are determined by the successor function defined for that particular ω-progression serving as archetype.

There are actually two problems with this situation. First, a circularity: Since these "places" are supposed to have none-but-structural properties, how can we understand the notion of 'successor' here when the *relata* to be placed in an order are determined entirely by that ordering. In other words, without some independent constitution as objects *with essential, but non-structural, properties*, places cannot do the work typically done by sets in order to understand the relations we are defining.

Second is Benacerraf's problem:

> Suppose, however, contrary to my contention, that there is some *bona fide* 'structuralist' way of introducing a privileged successor-type relation on an *ante rem* structure [...] if we suppose, for the sake of argument, that an *ante rem* progression, $\langle N, \phi, 1\rangle$ is somehow attained, we immediately see that indefinitely many others, explicitly definable in terms of this one, qualify equally well as candidates to serve as the referents of our numerals. (Ibid., p. 195. Original emphasis.)

The conclusion is that places cannot be numbers, and so the SGS program fails to offer an adequate semantic account that explains our use of number-terms. It also fails to give us an ontological account of what numbers are, since the analysis of 'structure' that it provides proves to be inadequate.

What is important in all of this is that Hellman's worries all engage considerations from the perspective of the platonist-nominalist debate, presupposing that questions regarding such ontological worries must be settled *before* further investigation or development can proceed; or at least supposing such worries to stand conceptually prior to mathematical investigation. This is clear from another of the worries Hellman considers for SGS:

> On the SGS approach, positions are, of course, structurally interdependent, i.e., it makes no sense to speak of a single number in isolation. Still, should there not be a fact of the matter, for instance, whether natural numbers are or are not identical to the real numbers of the substructure mathematically identical to $\mathbb{N}$. Shapiro says, no, this is a matter of convention. This is puzzling, since SGS is non-fictionalist. (Ibid., p. 192)

The worry here is one of identity across structures—whether some collection of places determined by one structure is the *same collection* of places identified by another. Such concerns derive from a strongly "foundationalist" attitude toward mathematics, as Awodey would say. It is a matter of interpretation whether Hellman is advancing such concerns seriously or only hypothetically, i.e., *if* one adopts a platonist attitude, these are the concerns that must be addressed (so we should not be platonists, or we should approach foundations from another perspective). He makes statements in Hellman (1989) which seem as though one of his goals in forwarding modal structuralism is to bypass such debates altogether.[18] Yet one of Hellman's concerns for Awodey's CTS program—the home address—lies along exactly such ontological lines.

4.3 CTS from an Ontological Standpoint

Hellman (2003) is primarily an objection to category theory as a suitable framework for a philosophical structuralism. As mentioned in our introduction to this chapter he levies three main criticisms against CTS, but we will only really address the first two in much detail, for reasons that will become clear below. As with his discussions of set-theoretic and *sui generis* structuralisms, almost every one of his criticisms can be read as derived from a program's failure to provide a suitable ontological grounding for mathematics. He suggests that these inadequate programs can be replaced by, or supplemented with, his own modal structuralism.

It is important to recognize that Hellman is not attacking the interest or utility of category theory *qua mathematics* (as a mathematical structuralism), but only the idea that category theory is *adequate* as an autonomous framework for the pursuit of foundational research (as a philosophical structuralism). This is important because Awodey's response rests upon his stated goal, which was to enunciate a mathematical notion of structure which can then be of use in the development of a robust philosophical structuralism. As he says:

> Surely it is the rise of the structural approach in modern mathematics that has sparked philosophical interest in structuralism. And yet the actual methods of mathematical structuralism seem to have been largely ignored; philosophical accounts often proceed instead either from model theory or from scratch. This neglect seems unfortunate; a view based instead on the methods of mathematical structuralism would at least be more consonant with current mathematical practice, and could benefit

[18] Consider: "Conundrums associated with a special realm of mathematical objects, emphasized by a number of contemporary philosophers such as Nelson Goodman and Paul Benacerraf in terms strikingly reminiscent of Dedekind—how to reconcile talk of such objects with the multiplicity of "ways of taking them", however we ever manage to refer to such objects, and the like—such questions would be seen *not even to arise* on the modal logical eliminative interpretation." (Hellman, 1989, p. viii)

> from this substantial tradition by drawing on a now well developed technical apparatus. (Awodey, 1996, p. 210)

He suggests that category theory is likely a very useful framework in this philosophical task, but cautions that he is not promoting a well thought-out philosophical position, merely motioning in that direction. Our Carnapian replies to Hellman's worries will thus act to complement Awodey's cautious optimism by diffusing those objections from a more neutral standpoint. We need-not assume that category theory serves as a philosophical foundation in order to argue that Hellman's objections are beside the point on that question.

4.3.1 Problem: Autonomous Foundations

The first problem finds its roots in a debate between Solomon Feferman (1977) and Saunders Mac Lane. Observe that category theory presupposes the notions of *collection* and *operation*, evident from its primitive of *composition*, which as we saw is often an operation on functions. However, Feferman argues that a foundation for mathematics should provide an *analysis* of these notions, not simply *assume* them. A traditional foundational framework based on set theory provides the required analysis. Thus, the argument suggests that category theory is simply inadequate as a foundational scheme—it is dependent upon some prior notion of *set* and *membership* in order to recover a story about notions which are cognitively more basic than the fundamental notions that category theory provides.

An advocate of category-theoretic foundations might argue that we do not really need to start with a general theory of collections. Rather, we can start with the notion of a *topos*,[19] or the notion of *structure-preserving mappings related by composition*, or some such. After all, every theory must start with some basic, undefined notions. Hellman identifies this attitude as in this case just the problem however—there is a seemingly essential tension in the category-theoretic program between a broad structuralism and a robust foundation:

> [...]somehow, we need to make sense of talk of *structures satisfying* the axioms of category theory, *i.e, being categories or topoi*, in a general sense, and it is at this level that an appeal to 'collection' and 'operation'

[19] A topos is a specific kind of category that is particularly well suited to foundational research because it recovers analogs of many common set-theoretic constructions and arguments. Cf. Bell ([1988]2008) for one development of topos theory in a foundational mode. Linnebo & Pettigrew (2011) provide a brief outline of topos theory in the context of evaluating Hellman's criticisms.

> in *some* form seem unavoidable. Indeed, one can subsume both these notions under a logic or theory of *relations* (with collections as unary relations): that is what is missing from category and topos theory, *both* as first-order theories *and, crucially, as informal mathematics,* but this is provided by set theory. (Hellman, 2003, p. 135. Original emphasis.)

The root of the problem is that there is no intended interpretation of the category-theoretic axioms. This is by design, but it raises the question of how we might go about determining when a structure instantiates our axioms, or more generally, how we might come to identify categories. We usually rely on model theory in such cases, but this is to assume a background set theory. Thus category theory does not seem to be able to provide an autonomous foundation after all.

Put another way, Hellman's complaint here simply points out that in order to understand the notion of *being a category* in the right sense, we need to fall back on some *external*, prior theory of relations in order to provide witnesses for our formulae. So Hellman is here supposing that a robust Tarskian notion of satisfaction is required for understanding and fully investigating our axiomatic theories.

4.3.2 Problem: A Home Address?

Hellman's second objection also concerns the notion of satisfaction, but this time with regard to a background ontology. Considering category theory as an autonomous foundation for mathematics, Hellman suggests that we must at some point ask: "where do categories come from and where do they live?" (Hellman, 2003, p. 136). He observes that category theory simply fails to engage this question. As we have already seen, this is because the axioms of category theory are traditionally taken to be read *algebraically*, as defining conditions, rather than *assertively*, as specifying truths. But then we must ask, while set theory has the cumulative hierarchy of pure sets as those which *really exist* (ignoring the lingering questions discussed above), what categories or topoi *really exist* in an analogous sense?

Structures such as the natural numbers lead to such questions, Hellman argues, precisely because our intuitions seem to drive us to find some *objects* which we can define as *the* natural numbers in a Fregean sense. But as we saw above, the category theorist would not want her axioms read in this way since she would certainly not want to say that any particular topos is *the correct one* for doing mathematics, or that any particular set of objects need satisfy the Peano axioms in order for us to prove theorems from those axioms—such statements are entirely contrary to the spirit of category theory. Hellman suggests that we might here have recourse to a meta-logic, since first-order completeness guarantees us the existence of models. But we should recall that the traditional framework for constructing such models is of course set theory, and so we arrive back at the problem of autonomy.

Trying to reconstruct meta-logic categorically is also of little help, since our problem is just that we do not take the axioms of category theory to guarantee the existence of anything, they merely serve as a definition of a type of mathematical structure. Taking this route, we end up in the old Russellian difficulty of *if-then-ism*, but in a sense much stronger than what we saw Awodey advocate above:

> [...]what we thought we were establishing as determinate truths turn out to be merely hypothetical, dependent on the mathematical existence of the very structures we thought we were investigating, and threatening to strip mathematics of any distinctive content. (Hellman, 2003, p. 138)

It is the schematic character of category theory that is the root of Hellman's difficulties with such an account, since we cannot then furnish the background theory he deems necessary to provide a grounding for even our most fundamental constructions. We can see this attitude as encompassing both of Hellman's complaints: Because the axioms of category theory are not taken to make *assertions*, they are not properly *about* things, but it seems that this is exactly what foundational work is supposed to do—give us an account of the peculiarly *mathematical things*.

4.3.3 Problem: Extraordinary Structures

Hellman's "Problem of Extraordinary Structures" seems to me both more subjective and technical than the others, and so not requiring a philosophical treatment. Briefly, the complaint concerns the "naturalness" of attempting to recover very large set-theoretic structures categorically. Certain sets constructed using the axiom of replacement are one example Hellman offers, sets requiring large-cardinal axioms are another. Such large sets can be recovered in category theory using contextually similar axioms; the worry is whether or not these axioms are *ad hoc* in such a setting (i.e., motivated simply by the need to recover what set-theory can build).

This question seems to me either a matter for more mathematical research—e.g., via what axioms and in what contexts can we construct such sets? what do we learn about these structures in the context of category theory that is not obvious in set theory as a result?—or a subjective matter about what one considers a "natural" motivation. To take one of Hellman's examples, large-cardinal axioms in set theory are often motivated simply by the fact that we want to study large cardinals! Why this ceases to be a reasonable motivation in moving to category theory is lost on me.

4.3.4 Hellman's Modal Structuralism

As mentioned at the outset of this chapter, it will be worthwhile to pause briefly to examine Hellman's positive program before moving on to our own analysis. *Modal Structuralism* combines the most promising features of both STS and SGS:

> STS, you will recall, is an eliminative structuralism with respect to number theory, analysis, and theories other than set-theory itself that appear to quantify over mathematical objects other than sets. In contrast, SGS countenances, for each coherent theory, an *ante rem* structure of places and relations thereon as prescribed by the theory. The background ontology is that of structure theory: collections, places, relations and functions, interrelated by the axioms of structure theory. This is intended to apply to set theory (theories) as well as any other mathematical theory, and so is, in this sense, a more thoroughgoing structuralism than STS. Modal-structuralism ('MS') both resembles and differs from both. Like SGS it applies to set theories as to other mathematical theories, but like STS it is eliminative. But it does 'one better' in this regard, eliminating officially any reference to any (purely) mathematical objects at all, including structures. (Hellman, 2001, p. 198)

The basic idea is to eliminate reference to mathematical objects by developing mathematical theories in a second-order logical language supplemented with primitive modal operators. With this apparatus Hellman argues that we can talk about *possible* structures without the need to confront the troubling ontological and semantic worries that plague other versions of philosophical structuralism.

Following the ideas in Putnam ([1967]1983), Hellman (1989) develops a translation scheme for mathematical theorems into his second-order modal language. Taking a sentence S of arithmetic, for example, we can represent it as a universally quantified conditional claim which holds of all ω-progressions, as follows:

$$\forall X(X \text{ is an } \omega\text{-progression} \rightarrow S \text{ holds in } X)$$

'Mathematical structure' is thus expressed by quantifying over all ω-progressions in the antecedent of the conditional, with our theorem satisfied by any of them.

This formulation raises two problems we have already encountered, however. In the first place we are not yet sure that there *are* any ω-progressions, and so the statement may be vacuously true. Recall this problem was encountered by CTS as well, and it is really another way of expressing Hellman's "Home Address" objection. Second, Hellman needs to provide some analysis of 'holds in' if he is to claim that the modal structuralist framework might provide an adequate semantic account of our mathematical statements as distinct from set theory. This is akin to Hellman's problem of "Autonomous Foundations" for CTS, since that program is considered parasitic upon set theory for such an account.

The problem of vacuity is where the modal aspects of modal structuralism become important. The translation above is not complete. Instead, the above sentence should be read as a strict conditional, as follows:

$$\Box\forall X(X \text{ is an } \omega\text{-progression} \rightarrow S \text{ holds in } X)$$

This removes the need for the modal structuralist to somehow confirm the actual existence of an ω-progression (concrete or abstract). However, this is not the entire story either, since the problem reappears if ω-progressions are not possible.[20] Thus Hellman asserts that we require a modal existence assumption, as follows:

$$\Diamond\exists X(X \text{ is an } \omega\text{-progression})$$

This asserts the *possibility* of the required structure. The modal structuralist framework requires a similar axiom for each structure of interest.

At this point Hellman still needs to explicitly address the semantic aspects of his framework. The goal is to recover an account of mathematics that provides mathematical statements with a definite truth value in a Tarskian sense. In other words, he "needs to express the notion of 'ω-sequence' and, it seems, 'holding' or 'satisfaction'." (Hellman, 1989, p. 18) Hellman observes that he cannot simply use the usual notion of satisfaction from model theory, since this is to presuppose set theory, leading into a situation analogous to the problem of autonomy for CTS. Similarly, Hellman does not want to rely upon quantification over possible worlds

[20] As Hellman says: "Now, the very same situation would obtain in the case of modal conditionals if ω-sequences are *not possible*, i.e., if there could (logically) be no standard realization of the PA^2 axioms." (Hellman, 1989, p. 27. Original emphasis.)

(i.e., modal comprehension), since this is most naturally explicated in set-theoretic terms and leads to the same sort of ontological worries that plague other forms of structuralism:

> It should also be stressed that, in our unwillingness to quantify over possibilia, we avoid such extravagances as "the totality of all possible ω-sequences". From our point of view, such totalities are illegitimate, much as "the totality of all possible sets" is illegitimate. While it may be possible to treat such totalities without literal contradiction, countenancing them runs counter to the open-ended character of mathematical construction, which the [modal structuralist] seeks to respect. (Hellman, 1989, pp. 17–18)

What Hellman suggests instead is an interpretation of the background logic that uses plural quantifiers and mereology, which grants us enough of the conceptual apparatus of set theory without a commitment to a fixed background ontology:

> As it turns out, the conceptual resources for positing sufficiently rich domains—and they need be posited only as logical possibilities, not as actually existing—are available even to a nominalist, employing mereology and plural quantification; set-theoretic notions of membership, class, singleton, etc. are dispensable. Put in other words, the combination of mereology and plural quantification already incorporates just enough of the content of set theory to do the job. In effect, this combination gives the expressive power of full second-order logic, once the possibility of infinitely many individuals is postulated [...] and this suffices to express conditions guaranteeing even inaccessibly many objects (in the sense of strongly inaccessible cardinals). (Hellman, 2003, pp. 147–148)[21]

So we must be committed to the logical possibility of an infinite set, and then the devices of plural quantification and mereology take care of any seeming reference to objects made by our theories. In other words, the framework utilizes a (somewhat non-standard) background ontology in the usual way, but rather than requiring us to assert the existence of this fixed background domain, we need only assert its logical possibility. In this way, Hellman supposes, we avoid ontological worries.

[21] I suppress the technical details here. For a brief overview see Hellman (2001, §4) and Hellman (2003, §6). The details are motivated and worked out in §6 of Hellman (1989, chp. 1).

4.4 A Carnapian Response to Hellman

We come, finally, to the analysis of Hellman's objections to CTS. In the previous chapters we have developed an interpretation of Carnap's meta-philosophy that has him *arguing* for a particular understanding of logico-mathematical sentences on the basis of certain methodological considerations. More broadly, we have seen Carnap recommend and argue for a particular approach to philosophical investigations—suggesting the replacement of traditional philosophical methods with the Logic of Science. This is a methodology of *explication*: To systematize, analyze, and clarify the concepts, methods, and languages of the sciences. Regarding a system of foundations for mathematics, we have seen that this method entails the eschewal of some traditional foundational questions as intractable and inconsequential. In contrast, our brief survey in this chapter of philosophical developments in the foundations and philosophy of mathematics has shown that much of this work is rooted in the context and methods of traditional philosophy. Hellman's program, and his criticisms of other foundational programs especially, seems motivated by the sorts of metaphysical and ontological questions Carnap's program was developed to dissolve.

In applying Carnap's program to the foundational investigations discussed above, it is important to be clear about the bounds of my argument. In the discussion to follow we will take Carnap's attitude toward mathematics, and to philosophy more generally, as *tentatively plausible*, and evaluate Hellman's criticisms through this Carnapian lens. So I will argue that *given* a Carnapian understanding of logic and mathematics as conventional, and the methodology of philosophy as the explication of science, natural responses to Hellman's worries are available; and furthermore that the apparent controversy between category- and set-theoretic foundations can be dissolved fruitfully and in accord with a mathematical method via the Principle of Tolerance. The Carnapian perspective we have developed is of course not the only approach to these issues. My claim is only that this perspective is potentially fruitful and so worthy of more attention. We will discuss each criticism of Hellman's in turn, and then move to the more general question of set- versus category-theory as a foundational system.

4.4.1 The Problem of Autonomy

Recall that Awodey's primary suggestion is that we look to the *mathematical* notion of 'structure' in order to develop a philosophical account that better-respects actual mathematical practice. This ends up being a recommendation to utilize the language of category theory in the development of our philosophical foundations. But as we have seen, category theory engenders a very different approach to foundations than a set-theoretic perspective, with emphasis placed upon the methodological usefulness of a categorical language, eschewing the justificatory questions that we have seen are so common in a set-theoretic setting. But must we still answer these questions?

In this regard it will be useful to distinguish three notions of 'autonomy': *logical*, *conceptual*, and *justificatory*.[22] A theory has logical autonomy in relation to another if it is possible to formulate the first without appeal to notions of the second. A theory has conceptual autonomy in relation to another if we can understand the one without reliance upon the other. And a theory has justificatory autonomy in relation another if we can justify the first without appeal to the other, or to justifications of the other. Hellman's first objection thus suggests that category theory cannot act as an autonomous foundation for mathematics in any of these senses, since an analysis of *satisfaction*—the development of some theory of relations using the notions of *operation* and *collection*—is logically and conceptually prior to the notions of *object*, *morphism*, and *composition* that category theory takes as primitive. Thus neither does category theory have justificatory autonomy, since it relies upon some theory of relations to justify these notions, and it is to this theory and its ontology that we must appeal in order to justify the truth of category-theoretic statements.

In proposing his positive program, Hellman in a sense argues that modal structuralism has justificatory autonomy with respect to set theory by showing that it has logical and conceptual autonomy. He essentially argues that we can cleverly *get around* the need to appeal to some fixed background ontology in the justification of our mathematical statements. This is to acquiesce to the need for a philosophical justification of our mathematical practice, and then to show some approach other than the standard one is up to the task. Each of the foundational schemes we have investigated in this chapter take this attitude—excepting CTS. Rather than attempting to get around an ontological appeal, the CTS program rejects the demand, arguing based upon methodological considerations that mathematical theorems have a fundamentally different character than is supposed by "foundationalists".[23]

[22]This distinction is suggested by Linnebo & Pettigrew (2011).

[23]Consider Awodey (2004, p. 55): "Hellman sympathizes with the structural viewpoint, and even appreciates and accepts many of the details of the categorical approach to it, but he thinks that there is still something missing in the overall category-theoretical position. The something is to be provided by his modal structuralism. But I contend that what is missing is only a correct understanding of the categorical approach."

What of the conceptual and logical autonomy of category-theoretic foundations? Recall from the Logico-Mathematical Interlude that one of Carnap's goals in *Logical Syntax* is to develop a complete characterization of mathematical truth in the face of Gödel's incompleteness theorems. In our second chapter we recognized that Carnap does this by assuming a variety of strong mathematical notions in his meta-language. So while his formal characterization accomplishes its goal, there are questions as to whether that account is viciously circular, and as to whether it is really informative with regard to our informal mathematical understanding.

I argued that Carnap's account is not viciously circular, and furthermore that it does indeed offer a philosophically non-trivial analysis of our mathematical concepts and practices. We saw that Carnap's analysis is premised upon a certain methodological understanding of mathematical practice and its role in the total language of science. Carnap's goal, then, was to formally *explicate* the notion of mathematical truth in a way that adequately recovers these methodological insights. This can be done while presupposing informally those very same notions. The analysis proceeds by suitably characterizing the supposed essential aspects of our concepts in a formal context. That we can recover these essential aspects in a completely formal mode then supports a particular attitude toward those concepts in more general contexts, supporting the idea that we can in many cases *replace* our informal concepts with the formalized *explicata*. This procedure can thus provide insight into the epistemic role of an *explicandum*, even though we may have relied upon those very pre-theoretic concepts in their statement and analysis.

It seems to me that Awodey's CTS program is structured in a similar way. This is not to say that the CTS program is a modern analogue of Carnap's philosophy of mathematics, but they do share many similarities.[24] Both attempt to import the modern methods of mathematics into our philosophical inquiry, and both take these methods as reason to discard certain traditional philosophical questions. What we learn from the Carnapian approach is that analysis of a set of concepts can be infor-

[24]Indeed, there are authors who forward category theory as a foundational system who seem to hold a philosophical position almost identical to our interpretation of Carnap. Consider: "The aim of Foundational studies is to produce a rigorous explication of the nature of mathematical reality. This involves a precise and formal definition, or representation of mathematical concepts, so that their interrelationships can be clarified and their properties better understood. [...] A Foundational system serves not so much to prop up the house of mathematics as to clarify the principles and methods by which the house was built in the first place." (Goldblatt, [1984]2006, pp. 13–14). The further exploration of this connection, and the development of a modern Carnapian meta-philosophical program using category theory, seems to me an interesting area for future research.

mative without requiring the *construction* of those concepts from first principles.[25] Insofar as Carnap is providing an analysis of logico-mathematical notions he is not barred from utilizing those very same notions. Similarly, the notions of *operation* and *collection* are ubiquitous in our basic comprehension of mathematics and the use of language. That these concepts may be utilized informally in some respect means no more than that mathematical discourse must occur via language. Indeed, Awodey observes that the notion of a *relation*, and many others besides, can be recovered as an instance of *morphism*, and so the more familiar set-theoretic notions do not have a unique claim as a reductive basis. The idea of multiple logical and conceptual reductive paths obtains further support from the recognition that the CTS interpretation of mathematical sentences takes them as schematic and conditional, thus eschewing the demand for a notion of *satisfaction* as primitive.

So category theory, when taken in a foundational mode, can very well provide an informative perspective on our logico-mathematical knowledge without being required to construct that knowledge. For example, the discovery that all toposes are equivalent to intuitionistic type theories, and so categorical logic is really just a form of algebraic logic, has greatly expanded our understanding of type theory, intuitionism, and their relation to other areas of mathematics.[26] This is not just new mathematics, but a deepening of our understanding of the relationship between mathematics and logic. This sounds like foundational work if anything is.

4.4.2 The Problem of the Home Address

As regards Hellman's second criticism, it is here that the distinction in perspectives between a "foundationalist" and a CTS approach is most evident. Hellman is posing a question about the metaphysical status of the "entities" underlying our mathematical theories. As noted, much of Hellman's work and analysis is motivated by such concerns. On the top-down approach to understanding mathematical theorems that Awodey suggests, such questions do not arise because theorems are presented schematically—*given* objects with such-and-such properties, they also have these other properties, but we need say nothing about whether there are such objects. If we must say *something*, it might be suggested that our schemata are filled-in by general mathematical practice (presupposing groups with the required properties, objects that make up an ω-progression, etc.), or through application to the world.

[25] McDonald (2012) makes a similar point in the context of categorical foundations.

[26] See Lambek & Scott (1986) and Bell ([1988]2008) for technical developments. The latter especially focuses on the foundational significance of these developments.

From a Carnapian standpoint, this preoccupation with the ontological status of the mathematical entities to which our theories refer must be interpreted as external—as a set of questions that can have no definite answers because they are not well-posed. Recall from the Anti-Metaphysico Philosophical Interlude Carnap's suggestion that the acceptance of a kind of entities into our discourse is a matter of adopting a linguistic framework. Importantly however, the mark of an external question—and so a matter to be determined by our linguistic choices—is not simply a matter of asking an existence question, but instead depends upon the sorts of methods that can be brought to bear in attempting to answer that question. For example, in the case of the existence of atoms, our answer depended not upon the adoption of an "atom framework", but instead upon standard empirical methods. Atoms are a kind of 'thing', and so the standard methods available in the thing-language are considered to be our best tools to address the problem.[27] Whether atoms exist was thus shown to be an internal, rather than an external question. It is the further question as to the existence of things themselves and the related questions surrounding the realism-instrumentalism controversy that are considered external. Should we refuse a thing-language altogether, then those same informal, empirical methods that give us an affirmative answer on the point of atoms should be reconstructed in whatever terminology we choose to adopt instead.

These latter questions are deemed external because existence questions require some well-established criteria for their investigation and resolution. Moving to an analytic domain is much the same, but our "standard methods" are deductive rather than empirical. Internally we can pose and investigate existence questions about, e.g., the natural numbers because we have adopted a certain logico-mathematical framework. If a particular question turns out to be independent from the mathematical system we are investigating, we can choose to add additional axioms or move to an entirely different system. For Carnap, while there is a "fact of the matter" about the consequences that follow from some set of assumptions, there is no fact of the matter regarding an absolute or correct choice of logico-mathematical principles divorced from some particular set of ends. Insofar as the platonist or nominalist is interested in existence questions that transcend a practical choice, they have moved beyond the bounds of mathematical methods. In this regard there is a close

[27]This is not to say that the question will necessarily have an answer. The answer to certain empirical and analytic questions, even to those that are well-posed, may simply lie beyond the grasp of our investigative methods. Recall that we could interpret certain 19th century scientists as speculating on just this point.

connection between CTS as a *deductivism* and Carnap's own logico-mathematical program as expressed in the Principle of Tolerance. Mathematicians investigate the consequences of presumed-consistent axiom systems, and models thereof, which are ultimately also specified by presumed-consistent axiom systems, or more usually, by informal mathematical theories.

So we can understand the "Problem of the Home Address" as simply misguided insofar as it is a demand to specify a definite background ontology that our theorems must be about. What provides this answer to Hellman force, from the Carnapian standpoint, is Carnap's argument that Tolerance is the correct methodological principle to employ in mathematical investigations. Because mathematics is a formal science, our investigations should be constrained by consistency and mathematical interest, but little else. Understood from the perspective of Awodey or Carnap, then, the investigation of mathematics as developed from a categorical perspective is licensed to stand on its own without prior ontological grounding.

4.4.3 Applying Tolerance

This brings us to the second, more general level from which Carnap's program offers insight. The Principle of Tolerance allows us to eschew questions of "correctness" as applied to logic and mathematics. As we have seen, the debate between a "foundationalist" and category-theoretic foundations for mathematics rests on deep differences of perspective from which to understand mathematics and on the purpose of foundational programs. There is no question that both set theory and category theory act as fruitful frameworks for mathematical and foundational investigations, and so it seems best that both continue to be pursued.

Similarly, Hellman's modal structuralism remains of interest insofar as it highlights the possibility that modal notions are tacitly embedded in our mathematical discourse. Hellman (2001, §5) argues that our regular mathematical discourse involves tacit appeals to modal notions. The common set-theoretic notion of *all arbitrary subsets* is given as an example—what we really mean is *all logically possible subsets*. Whether this is correct will not be evaluated here, the point is that it is interesting. From the standpoint of Carnap's meta-philosophy, we can take modal structuralism as an explication of this aspect of our mathematical discourse.

This, however, is to treat these programs *as mathematics first*, with the understanding that deeper, "metaphysical" questions are ill-posed. As we have seen, Carnap's proposed replacement of traditional philosophical inquiry with properly scientific methods does not leave philosophy unscathed. I submit that Carnap would see much of the argumentative back-and-forth surveyed in this chapter as just the kind of "wearisome controversies" that his program suggests we abandon. With this barrier removed, there is no reason not to purse all three programs equally, recognizing that insight into our scientific methods, knowledge, and practices may arrive from unexpected shores.

A Deflationary Carnap

Notice that on the Deflationary reading of Carnap's program there is absolutely no motivation for this attitude, because there is no impetus for the application of Tolerance to the debate. If Hellman accepts the Principle of Tolerance, then he will presumably give up his criticisms, otherwise not. This is analogous to the response Carnap must give to a critic like the intuitionist on the Deflationary reading. Recall our discussion in chapter 3 of the intuitionist objection to Carnap's program: The intuitionist disagrees that formalization and a comparison of the respective consequences of each language is an adequate way to address the disagreement between intuitionism and classical mathematics. We saw that all a deflationary Carnap can do in response is throw up his hands and withdraw from debate. But even leaving to one side Brouwer's rejection of formalization as an adequate method for mathematics, *of course* the intuitionist disagrees with the attitude of Tolerance, because it requires the acceptance of a meta-language stronger than the mathematics the intuitionist considers to be valid. So Carnap is simply begging the question in a rather straightforward way. On the reading of the Principle of Tolerance we have developed, alternatively, there is a least an argument to be made that this is the correct methodology for settling the dispute because Carnap can marshal the evidence that his program recovers the features of mathematics and logic that are essential to their development and application in the empirical sciences. He thus shifts the burden of proof onto his critic to show that his account is inadequate.

So Hellman and Awodey at least have reason to accept the attitude of Tolerance—that mathematical discourse displays certain essential characteristics which are recoverable without appeal to further, metaphysical questions. Accepting this, they can each put aside their preconceptions and recognize the foundational value of the other's program, otherwise not. But Tolerance requires an *argument* if it is to have even this amount of force, and so be applicable in the dissolution of foundational debates as Carnap intended. If it acts to ground his entire program then that program—along with Tolerance—can just be dismissed as not expressing the right attitude for me. If alternatively Tolerance is justified by an appeal to the conventionality of formal knowledge, and this appeal can be shown to recover the important features of that knowledge, then its force as a methodological principle cannot simply be dismissed out of hand. The Principle of Tolerance then becomes a fruitful tool for resolving foundational debates.

4.5 Conclusions

Overall, what I have attempted to show in this dissertation is that the reconsideration of Logical Empiricism currently ongoing in the literature is extremely apt. Once properly understood, Carnap's meta-philosophical program offers an insightful and subtle approach to the analysis and understanding of the concepts of mathematics, the sciences, and their interaction—an approach which deserves further consideration and application. The discussion in this chapter represents only the most superficial application of this approach to contemporary foundational issues, but hopefully points the way toward more fruitful use of these ideas.

I take my primary contribution to be showing that Carnap's philosophy of mathematics is not to be as straightforwardly dismissed as is sometimes thought. As observed in the second chapter, we saw that the various circularity objections, which many have taken as undermining his program, do not when that program is considered in detail. Essentially, Carnap's responses to Gödel, Quine, *et al.* need-not weaken his program to the point that it is philosophically inert, as I have argued occurs on a Deflationary reading of Carnap. That interpretation suggests a straightforward deference to the Principle of Tolerance given almost any challenge to the mathematical or methodological import of Carnap's program. This was most clearly displayed in the deflationary response to Gödel's criticism, which invokes strong relativist tendencies that I argued are entirely incongruous with Carnap's work.

At the outset I suggested that Carnap's primary aim was to introduce a properly *scientific philosophy*. The methods proposed to that end treat both logico-mathematical and philosophical statements as conventional, and so are guided by the Principle of Tolerance; they are not, however, grounded by it. Instead Carnap offers methodological considerations for suggesting that philosophy, logic, and mathematics be explicated as formal sciences. It is these considerations that suggest a place for philosophers amongst the sciences as the investigators of its concepts.

Ultimately, this is not the only fruitful method in philosophy, nor will Carnap's arguments be completely persuasive to one who insists on pursuing more metaphysical considerations. But Carnap's meta-philosophical program does offer considerations in its own favour, and is certainly worthy of reconsideration.

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