CARNAP'S CONVENTIONALISM: LOGIC, SCIENCE, AND TOLERANCE

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December 16^{th}, 2013

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Abstract

In broadest terms, this thesis is concerned to answer the question of whether the view that arithmetic is analytic can be maintained consistently. Lest there be much suspense, I will conclude that it can. Those who disagree claim that accounts which defend the analyticity of arithmetic are either unable to give a satisfactory account of the foundations of mathematics due to the incompleteness theorems, or, if steps are taken to mitigate incompleteness, then the view loses the ability to account for the applicability of mathematics in the sciences. I will show that this criticism is not successful against every view whereby arithmetic is analytic by showing that the brand of "conventionalism" about mathematics that Rudolf Carnap advocated in the 1930s, especially in *Logical Syntax of Language*, does not suffer from these difficulties. There, Carnap develops an account of logic and mathematics that ensures the analyticity of both. It is based on his famous "Principle of Tolerance", and so the major focus of this thesis will to defend this principle from certain criticisms that have arisen in the 80 years since the book was published. I claim that these criticisms all share certain misunderstandings of the principle, and, because my diagnosis of the critiques is that they misunderstand Carnap, the defense I will give is of a primarily historical and exegetical nature.

Again speaking broadly, the defense will be split into two parts: one primarily historical and the other argumentative. The historical section concerns the development of Carnap's views on logic and mathematics, from their beginnings in Frege's lectures up through the publication of *Logical Syntax*. Though this material is well-trod ground, it is necessary background for the second part. In part two we shift gears, and leave aside the historical development of Carnap's views to examine a certain family of critiques of it. We focus on the version due to Kurt Gödel, but also explore four others found in the literature. In the final chapter, I develop a reading of Carnap's Principle – the 'wide' reading. It is one whereby there are no antecedent constraints on the construction of linguistic frameworks. I argue that this reading of the principle resolves the purported problems. Though this thesis is not a vindication of Carnap's view of logic and mathematics *tout court*, it does show that the view has more plausibility than is commonly thought.

For my parents, to whom I owe everything.

Analytic philosophy is ethically neutral *formally*; its professors do *not* indoctrinate their students with dogmas as to life, religion, race, or society. But analytic philosophy is the exercise of intelligence in a special field, and if the way of intelligence becomes part of the habitual nature of men, no doctrines and no institutions are safe from critical reappraisals. [...] Analytic philosophy has thus a double function: it provides quiet green pastures for intellectual analysis, wherein its practitioners can find refuge from a troubled world and cultivate their intellectual games with chess-like indifference to its course; and it is also a keen, shining sword helping to dispel irrational beliefs and to make evident the structure of ideas. It is at once the pastime of a recluse and a terribly serious adventure: it aims to make as clear as possible what it is we really know.

— Ernest Nagel, "Impressions and Appraisals of Analytic Philosophy in Europe, Part 1"

The scientific world conception is characterized not so much by theses of its own, but rather by its basic attitude, its points of view and direction of research. The goal ahead is *unified science*. [...] From this aim follows the emphasis on *collective efforts*, and also the emphasis on what can be grasped intersubjectively; from this springs the search for a neutral system of formulae, for a symbolism freed from the slag of historical languages; and also the search for a total system of concepts. Neatness and clarity are striven for, and dark distances and unfathomable depths rejected. In science there are no 'depths'; there is surface everywhere [...].

– Otto Neurath et. al., "Wissenschaftliche Weltauffassung: Der Wiener Kreis"

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Contents

Chapter 1

Introduction

The fundamental question this thesis aims to address is that of the status of arithmetic. Put bluntly, it will be our purpose to discover whether arithmetic is analytic or synthetic; and, lest the suspense become unbearable, we say up front that it will be our aim to defend the claim that it is analytic. This thought has been current in analytic philosophy for at least the last hundred-odd years, and one might be forgiven for thinking that the question has been settled once and for all with the publication of Gottlob Frege's *Die Grundlagen der Arithmetik* (*The Foundations of Arithmetic*) in 1884, or at least with the "rediscovery" of Frege by mainstream analytic philosophy in the 1950s. However, in the light of certain difficulties that arose in the study of the foundations of mathematics, namely of the paradoxes of set theory and of the First and Second Incompleteness Theorems of Kurt Gödel, there has been a steady, if quiet, undercurrent of dissent from this orthodox view. However, for reasons that I will make plain below, I believe the time is right to address these dissenters, and put at least one of their arguments, along with a few of its variants, to bed for good.

The argument that I will address can be summarized easily. Essentially, the dissenters claim that the orthodox view is either unable to give a satisfactory account of the foundations of mathematics due to the incompleteness of any mathematical theory rich enough to be of use in this endeavor, or if steps are taken to mitigate this incompleteness, then the orthodox view loses the ability to account for the applicability of the mathematical portion of our language in the natural sciences, or indeed in any synthetic discourse. The dissenters' argument falls short of showing that mathematics must therefore be taken to be synthetic – though some of them think this as well.[1] Our aim will be to show that this is not successful against every view whereby arithmetic is analytic, and in particular we will show that the brand of "conventionalism" about mathematics that Rudolf Carnap advocated in the 1930s, especially in his book *Logische Syntax der Sprache* (*Logical Syntax of Language*), does not suffer from the difficulties which may plague other orthodox views. Over the course of this defense it will become clear, however, that the sense in which

[1]Most famously Gödel argued for exactly this conclusion in his 1951 Gibbs Lectures ([Gödel, 1995c]). More recently Kolman has argued in a similar fashion in his [Kolman, 2006], though his ultimate aim is different from Gödel's.

Carnap's conventionalism is 'orthodox' is loose. I will say more about this below, and much more as this thesis progresses. Now, though, I think it worth pausing briefly to make a few remarks on scholarship in the history of analytical philosophy, in particular scholarship on the Vienna Circle, since my approach to the problem that concerns us here – of the status of mathematics – will be of a primarily historical and exegetical nature.

The history of the Vienna Circle, and the logical positivist movement with which it was associated, is largely known as a cautionary tale in contemporary philosophy. The Circle are seen as akin to the legendary Icarus – a movement that flew too high on the wings of their rhetoric, and whose wings were subsequently melted by the very logical tools they proclaimed as the saviors of philosophy. John Passmore put matters succinctly while summing up in his influential essay "Logical Positivism", and said "Logical positivism, then, is dead, or as dead as a philosophical movement ever becomes".[2] Building on this theme, in the introduction to his book *Philosophical Analysis in the Twentieth Century*, Scott Soames says,

> One movement – logical positivism – is widely regarded to have been refuted by its own proponents. As chronicled in volume 1, the logical positivists articulated their basic conception, formulated in terms clear and precise enough to be tested, and then found counter–arguments that in the end undermined it. Events like these, which constitute real progress, are unfortunately far too rare in the history of philosophy. For that reason, the rise and fall of logical positivism is viewed by many philosophers today as a proud chapter in the analytic tradition.[3]

Indeed, as Passmore and Soames suggest, if any view in philosophy can be decisively said to be dead, then logical positivism is it.[4] As George Reisch notes in indirect agreement with Soames,

> Nostalgia, of course, carries little philosophical weight. Most contemporary philosophers, however much they may appreciate logical empiricism as their profession's founding movement, agree that in the 1950s and '60s logical empiricism was revealed to be a catalog of mistakes, misjudgments, and oversimplifications about science and epistemology.[5]

However, as Michael Friedman notes in his paper "The Re-evaluation of Logical Positivism", in the years since Logical Positivism was an active philosophical

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[2] [Passmore, 1967].

[3] [Soames, 2003], p *xiii*.

[4] For our purposes here it is not necessary to put a precise date on the death of logical positivism. Suffice it to say that it is sometime between the publication of Quine's essay "On What There Is", which in some sense reinvigorated and legitimized the study of metaphysics, and Kuhn's book *The Structure of Scientific Revolutions*, which is thought to have picked apart the positivists' view of scientific practice. Of course, this picture has been substantially debated in the literature. See [Reisch, 1991] as a paradigm example.

[5] [Reisch, 2005], p 1.

movement, it has become increasingly possible to consider its positions at a dispassionate distance.[6] As more philosophers and historians of philosophy look at the movement in the light of later philosophical developments, a new understanding of logical positivism has emerged. It is one in which, contrary to their depiction in the philosophical folklore, they are not dogmatic foundationalists, or radical empiricists. In keeping with this new understanding, it will be the purpose of this thesis to examine a particular critique of the conventionalism about logic and mathematics of Rudolf Carnap that was initially made by the famous logician Kurt Gödel, but was later followed by the philosopher of science Hillary Putnam, and also by Gabriella Crocco. Finally, this thesis will show that critiques of this kind fail because they do not adequately appreciate the role of Carnap's famous 'Principle of Tolerance' in his philosophical position.

Though the version of the argument that is the main focus of this thesis, namely the one due to Gödel, has only been known since 1995, versions of it have lurked in many philosophers' thinking about logical positivism almost since the very beginning of the movement. For example, a certain version of it is found in Karl Popper's book *The Logic of Scientific Discovery*, originally published in a shortened form under the title *Logik der Forschung*, and another can be found in Eino Kalia's work.[7] These early criticisms are so well known that they have led the philosophical community over the past 50 or so years to think of them as decisive. Again, Soames' comments above are a paradigmatic example of the kind of blanket dismissal that the positivist's ideas now receive. Partly, no doubt, this is due to some poor, or even mistaken, formulations of their views that they published in the early days. No less importantly, as I alluded above, the Circle's strident rhetoric often ran ahead of their actual argumentation. Indeed, Carnap in particular is known in part for his overbold phrasing and optimism in characterizing the Circle's achievements. Finally, I think it is partly due to certain historical circumstances that prevented the Circle from arriving at a more unified expression of their view, namely the premature deaths of both Hans Hahn and Mortiz Schlick, and possibly most dramatically the outbreak of World War II and the Cold War that followed.[8] In any case, until recently, the logical positivists were almost universally regarded by current philosophers as some combination of benighted fools, intolerant empiricists, and dogmatic foundationalists. To put it simply, in the current philosophical folklore the Vienna Circle are the bogeymen of analytic philosophy's past.

Making the argument that this view of the logical positivists, or the Vienna Circle, or even just of Carnap individually, is completely mistaken would be far too large a task for a work of this size, assuming it is even possible. Indeed, for all I want to say here it may very well be the case that the current philosophical folk view of logical positivism is ultimately correct. However, it is my goal to show that at least Carnap's view of logic and mathematics as he states it in his book *LSL* is not wrong for every reason it is thought to be so. I intend this to be a very restricted claim, and to make sense of what follows it is worth

[6] See [Friedman, 1991], p 505.

[7] See [Popper, 1959], p 36, and [Carus, 2007], ch 7.

[8] On the effect of Hahn and Schlick's deaths, see [Feigl, 1969]. On the effect of WWII and the Cold War see the introduction to [Carus, 2007] and [Reisch, 2005].

spending some time spelling out all the restrictions. I am not attempting a blanket defense of Carnap's view. Though quite a lot of research has gone in to arguing that there is more continuity than it might at first appear in his thinking over the course of his philosophical career, I will treat it as if there can be some definitive boundaries drawn between different streams of his research. Firstly, I exclude from consideration all of Carnap's work that was originally published after 1940. Secondly, I also exclude from consideration Carnap's two early books: *Der Raum*, which is the somewhat modified book version of his Ph.D thesis, and his second book *Der Logische Aufbau der Welt* on the subject of the logical construction of the world via our perceptual experience. These are excluded partly in an effort to meet the critiques which are the subject of this thesis on their own terms, and partly because their relevance to his views in logic and mathematics is questionable. This leaves the work that Carnap produced during his time in Vienna (1925 – 1931) and in Prague (1931 – 1936) as our primary sources. We will be particularly, though by no means exclusively, concerned with his book *LSL*, originally published in 1934 but revised and translated into English in 1937. There are, as ever with studies of this kind, some issues regarding translation and also post-hoc edits; these will be flagged in cases where I think that they make a difference to the interpretation that I offer.

I hope that the above restrictions on the claim I am making help to make clear that what is intended in this thesis is a rigorous, historically based defense of a portion of Carnap's thought, namely the view of logic and mathematics that he held in the 1930s. Sometimes this period of his thought is called his 'Syntax Period', though I think this label is misleading for reasons that I will discuss below. In general, the thesis will be organized chronologically, following Carnap's development up to the writing of *LSL*. Chapters two and three will focus on how Carnap arrived at this view. I will focus on the three major influences on his thinking about logic, namely Gottlob Frege, Bertrand Russell, and Ludwig Wittgenstein. The main focus of these sections will be to show that portions of the views of each of these influences were assimilated into Carnap's own view, but that this resulting position is distinct from each of them individually. Chapter two will give an account of Carnap's logicism, from its beginnings in his student days in Frege's lectures, through his reading of Russell's work. I stop short of including his own version of the logicist account of mathematics, which will be taken up in subsequent chapters. Chapter three is an examination of Carnap's time in Vienna, focused primarily on the influence of Wittgenstein on Carnap's thinking at the time. I build up to his break with the Wittgenstinnian views of the Circle, paying special attention to the debate over the nature of protocol sentences, and the interaction between Carnap's thinking on this issue with his thinking about his philosophical views more generally.

There is a sense that there is not much novel in the first few chapters, and indeed the discussion of Carnap's intellectual development is relatively well–trod ground. However, the groundwork that these two chapters lay is essential to the case that I will present in chapter four. In that chapter, I will give a detailed account of Carnap's view on mathematics and logic, grounded primarily in my reading of *LSL*.[9] I will argue that the most famous section of that book, the 'Principle of Tolerance', is critical to understanding the position that he advocates in that book. Additionally, I will argue that the Principle is, in some substantial sense, not a new portion of his thought first articulated in *LSL*, but rather something akin to it was a part of his thinking from his childhood, though I will note that he seems to have gone away from 'Tolerant' thinking during his days in Vienna. As part of this latter argument, I will answer Karl Menger's priority challenge to Carnap's Tolerance. This section is the final portion of our extended development of Carnap's position.

In Chapter five, I change focus somewhat, and take up Gödel's critique of this view. The critique was found in his *Nachlass*, and so it only became publicly subject to discussion in 1995 when the third volume of the Gödel *Collected Works* was published. However, Gödel was not the only philosopher to notice that this line of criticism might work against Carnap. As originally pointed out by Thomas Ricketts, Hilary Putnam gave a version of it in his "Philosophers and Human Understanding".[10] This version of the critique is also discussed in chapter five, along with some contemporary secondary literature which make the case that these kinds of criticisms do devastating damage to Carnap's view of logic and mathematics. In particular, we focus on work by Gabriella Crocco, Patricia Blanchette, Hilary Putnam, and Michael Potter. However, this chapter is purely expository, and we hold off from a final evaluation of these criticisms until later.

In the final chapter, we confront the critiques directly. In order to do so, we return to considering the Principle, and I argue for a reading of it, which I dub the 'wide' reading, whereby there are no antecedent constraints on which languages can be proposed. After pausing to consider, but ultimately dismiss, a revenge type worry due to Michael Friedman, we examine the criticisms from chapter 5. I argue that they can be put into two groups, analogous to the twin lines of attack from the sketch of the criticisms I gave above. They are: (1) criticisms that focus on the question of the applicability of mathematics, and (2) those criticisms which argue that the Principle is either incoherent or self-refuting. I analyze both groups in the light of the wide reading of the Principle and conclude that they do not succeed in doing any damage to Carnap's position so understood. In the end, this thesis is not a vindication of Carnap, or of the Principle of Tolerance, *tout court*. What I do take it to establish, however, is that there is at least one family of strategies that will not succeed against Carnap's Tolerance-inspired philosophy of logic and mathematics. But, this aim is immediately confronted by a pressing worry, namely why this kind of limited defense of a philosophical position – a position which has not been current for at least seventy-five years – should be of interest to contemporary philosophers. Before moving on to the opening considerations of this thesis, I think it is worth pausing on this question at some length.

[9]Though I will follow the commonly accepted practice of calling his view 'Conventionalism', this must be done with caution. In section 1.2 I explain the ways in which some philosophers in the past have been misled by this title.

[10]See [Ricketts, 1994], [Putnam, 1983], and section 5.2 below.

1.1 On the Value of History of Philosophy

Analytic philosophy is famously a-historical, if not outright anti-historical.[11] However, since almost the very beginnings of this way of doing philosophy, there has been an interest in laying out its history. In the Vienna Circle's 1929 manifesto, for example, the authors are at great pains to link their group with various figures in the history of philosophy.[12] There are several potential motivations that could lie behind attempts such as the Circle's to set out the history of analytic philosophy, and one need only look to the introductions of the many books on this history to find them enumerated.[13] One important motivation, which I think is fundamentally misguided, is to determine what analytical philosophy *is*.[14] In some cases, these definitions are genealogical and analytic philosophy is defined as a train of influence that flows from certain philosophers – most often Frege, Russell, Moore, and Wittgenstein – to others.[15] In other cases, the definition given is methodological, and analytic philosophy is philosophy done in a particular way; usually a striving for clarity and rigor are mentioned in this capacity, alongside the use of the tools of formal logic, a focus on language, and a certain relationship to the sciences (either separate from them in the early period, or continuous with them after the rise of naturalism sometime in the 1950s). Finally, though far less often than the other two definitions thus far discussed, sometimes analytical philosophy is said to be a certain collection of philosophical problems; this definition occasionally appears in a negative form as the exclusion of certain problems. In each case, however, it seems that continued historical scrutiny reveals these attempts to be futile. Consider, for example, the genealogical approach. There does not seem to be an economically sized group of philosophers which we can cite as those to whom we owe the foundation of the our tradition.[16]

Returning to the topic of this section, if the usual reasons that people do history of analytic philosophy are doomed to failure, then why should we do

[11]It is at least plausible that this attitude is one of the lasting vestiges of the influence of the Vienna Circle over the way analytic philosophy is done.

[12]Authorship of this manifesto is somewhat hard to determine. I have given it to Neurath. A fuller discussion of my reasons for this assignment is found in section 3.1.2 below.

[13]For example, the introduction to Glock's [Glock, 1997], and in particular pp *vii* – *ix*, is a clear example.

[14]There might be some distinction to be drawn between 'analytic' philosophy and 'analytical' philosophy. We might say that in the former case we are discussing a particular school of philosophy which has been dominant in the English speaking world for the majority of the last hundred years, while in the latter case we are naming a particular method of doing philosophy – philosophical analysis, which investigates the world by analyzing propositions into their component parts, and stands in contrast to a kind of monism or synthetic approach to philosophical system building. I, however, do not mean anything substantial by the difference in terminology here, and I trust that, having noted this possible confusion, it will not trouble us further.

[15]Michael Dummett's [Dummett, 1993] is perhaps the most famous example, though there certainly are others.

[16]A paradigmatic case is Richard Rorty, who is usually not included in the foundational group, but is not related genealogically to Frege, Russell, Moore, or Wittgenstein in any obvious way. Martinich and Sosa make a general version of this point in the introduction to their [Martinich and Sosa, 2001].

it at all? There are two reasons that I think warrant mention here. The first we have already seen in action. The attempts to do history as a kind of definition of analytic philosophy are, at root, a kind of boarder guarding. They are attempts to say who is 'in' and who is 'out' in one way or another. One of the most valuable contributions that history of analytic philosophy can make is to show that these attempts have failed – that is, to resist these attempts at systematic exclusion of particular philosophers, areas of investigation, or philosophical methods from what is considered legitimate philosophy in the majority of the English speaking world.

There are two other important reasons for doing history of philosophy that I want to touch on here. The first is the thought that exploring the works of philosophers from the past can shed light on philosophical positions that have been neglected in contemporary debates.[17] That is, one reason that we might be interested in studying history is to mine it for ideas. As I see it, this can be a very profitable enterprise, but comes with substantial risk. For example, since someone engaged in this kind of historical profiteering is only interested in finding ideas that lead them to positions relevant in contemporary debates, they may allow themselves a certain leeway in their scholarship – after all, they are only after the idea insofar as they can use it, and if the historical subject of their research did not actually hold the position they discover, then so much the worse for the historical philosopher.

The second motivation is the converse of this first one: we study historical philosophers thinking not for the benefits that it can provide to contemporary philosophy, but rather for the sake of 'getting things right'. That is, we might think that it is important to ensure that the historical record is correct; that, for example, when we say to our students that some philosopher believed that P, they did in fact believe that that P. This approach has risks as well. Those of us who are more interested in history than contemporary philosophy might allow ourselves too much leeway in the philosophical portion of our work because our main interest is historical.[18] It is my aim in this thesis to chart a course between these two extremes. Additionally, while it is primarily a project in this latter camp – namely that of setting the historical record straight – I hope that there is value for philosophers whose interests are more closely aligned with those in the former group.

1.2 A Short Note on Terminology

As with any longer written work in philosophy, in this thesis there will be a number of central terms whose meaning may not be entirely clear. For most of them, I provide clarificatory remarks at the place where they are first used. However, two terms, 'Vienna Circle' and 'logical positivism', do not fit neatly into this pattern, and so it will be beneficial to give rough glosses for them at the outset. The name 'logical positivism' was used by its adherents to de-

[17]There are plenty of examples of this in recent history, but the 'rediscovery' of virtue approaches to ethics in 20^{th} century philosophy is perhaps the most well known.

[18]Barnes makes a similar point at [Barnes, 1995], p *xviii*.

scribe a view that attempted to marry the empiricist claim that all knowledge is grounded in sense data with the claim that mathematical and logical knowledge are both *a priori* and necessary. In part because the movement began in Vienna, where Ernst Mach and Ludwig Boltzmann had held the chair for the Philosophy of the Inductive Sciences, the first attempts to spell out what 'beginning in sense data' might mean were positivistic.[19] That is, they were framed in such a way that all substantial knowledge of the world, all synthetic sentences, could be directly tied in some way to sense impressions. Later, however, this commitment to sense impressions was relaxed, and it sufficed for synthetic knowledge to be verifiable by some kind of scientific testing; more will be said about this transition in section 3.1.2 below. Concordant with this change, some of the philosophers who adhered to this view came to prefer the term 'logical empiricism' to the older 'logical positivism'. In this thesis, however, I use the names interchangeably and I trust that this will not cause any confusions.

While not exclusive to them, for a number of reasons the group most associated with logical positivism is the Vienna Circle. Though Hans Reichenbach's Berlin Circle also fell under the logical positivist banner, as did the Ernst Mach Society (which was the organization that the Vienna Circle used for public outreach) and the Society for Empirical Philosophy (which was the public arm of the Berlin Circle), in this thesis the term 'logical positivism' will be a stand-in for the Vienna Circle only, and not these other groups.[20] Additionally, the term 'Vienna Circle' itself will be used to refer only to those people who self-identified as members; a full list can be found at the end of their manifesto, [Neurath, 1973]. Additionally, I will use the terms 'Vienna Circle' and 'the Circle' to pick out the same group. With these short terminological preliminaries out of the way, we begin with the thesis proper.

[19] Moritz Schlick later came to occupy this same chair.

[20] There is a small discussion of the relationship between the Circle and the various other scientifically minded philosophical groups in [Kraft, 1952], pp 5 – 6.

Chapter 2

Carnap and the Logicist Tradition

In this chapter we will examine the influence that two of the people who most impacted Carnap's philosophy of mathematics and logic, namely Gottlob Frege and Bertrand Russell, had on him. These two philosophers represent the genesis of the logicist stream of Carnap's thought, though the types of logicism they espoused were substantially different.[1] The goal of part two of this chapter will be to trace out what of their views are included in Carnap's own view, and in what sense he can be called a logicist as a result of this influence. Of course, there is a way in which labels of this kind do not matter – indeed, nothing much hangs on whether or not the position that we will eventually lay out as Carnap's is a 'logicist' one in some ultimate sense. This is merely a way at getting at what I believe Carnap had in mind for his position, and I believe it to be an instructive lens through which to examine his views. Moreover, Carnap repeatedly called himself a logicist, and did so throughout several different stages in his life.[2] Presumably this label had some meaning for him, and it will be instructive, I think, to draw out what it was.

This chapter will be structured into two parts. In the first, we give a brief overview of Carnap's philosophy of logic and mathematics in the 1930s. The main concern in that section is simply to give a frame with which we can compare his view with those of Frege and Russell, as well as those of Carnap's other influences to be examined in later chapters. We will revisit and further develop this overview in section 4.2 below. The first section of the second part will detail Carnap's relationship with Frege. The aim will be to show in precisely what ways Frege's views influenced Carnap's thinking about logic and mathematics into the period in which he wrote *LSL*. The second section of the second part we examine the influence that Russell had on Carnap's logicism. In

[1] Russell also exerted a large influence on Carnap's thought beyond his logicism. For example, it is often said that the impetus for Carnap's second book, [Carnap, 1967], comes from Russell's external world program. However, this aspect of his influence is not of further relevance to the investigation we undertake here. The interested reader is commended to [Carus, 2007] (in particular chapter 5), [Friedman, 1999], [Pincock, 2002], [Reck, 2004], and [Richardson, 1998] for more on this part of Russell's influence on Carnap.

[2] For two instances see [Carnap, 1963], p 12, and p 47.

particular, we will be concerned to show the way in which Carnap took Russell and Whitehead's book *Principia Mathematica* to have settled the question of whether mathematics could be reduced to logic once and for all. As we will see, this conviction plays a critical role in shaping Carnap's attitudes towards the debate over the foundations of mathematics, and indeed towards foundational questions in general.

2.1 Carnap's Philosophy of Mathematics and Logic: An Overview

The view of logic, mathematics, and the relationship between them that Carnap is most known for is that found in his 1934 book *The Logical Syntax of Language*. Though we will revise our account of his view later, for now we will use this brief summary. Firstly, Carnap assumes that it is uncontroversial that mathematics can be reduced to logic in the manner of Russell and Whitehead's *Principia Mathematica*, of which more will be said in section 2.2.2 below.[3] The question that is the subject of the debate over the relationship between logic and mathematics is therefore not a technical question about whether the one (mathematics) can be represented in the other (logic), which Carnap believed had been answered, but rather it must be a metaphysical one. Put roughly, it is something like this: "Is the fundamental nature of mathematics such that (1) it discovers properties of independently existing things (be they logical objects or other abstracta), or is it (2) about mental entities that proceed from the 'form of inner sense', or (3) is it about finite strings of marks on a page that are given in empirical sensation?" These three possibilities approximate the positions that various historical mathematicians and philosophers of mathematics took on the debate. However, as with all such metaphysical arguments, Carnap thinks the issue is not one of the nature of certain things, but rather it is one of *language*. That is, he thinks that the metaphysical issue is vexed and the debate intractable, and so would rather us address the question "Which language is most perspicuous for the formal reconstruction of our existing mathematical practice?"[4] By language he means a constructed language that can be either formal, in the modern sense, or something like a natural language but with the ambiguities ironed out. Each language is composed of a vocabulary of uninterpreted signs, and some rules (of formation, which tell us which strings of the vocabulary constitute sentences, and of transformation, which tell us how we

[3] This interpretation of Carnap's relationship with the Russell and Whitehead version of the logicist program has been challenged by Michael Friedman in his [Friedman, 2001], where he claims that "Nevertheless, he recognizes that traditional logicism cannot succeed; we cannot reduce mathematics to logic in some antecedently understood sense, whether in the sense of Frege's *Begriffsschrift* or Whitehead and Russell's *Principia Mathematica*" ([Friedman, 2001], p 223). For reasons that will become clear below, I think he is incorrect on this point. Carnap did know that there were significant differences between his own version of logicism and the 'traditional' kinds, but he nonetheless saw his as continuous with theirs, and, crucially, thought that the reduction had been accomplished by Russell and Whitehead (not withstanding some debates about the genuine logicality of certain axioms they used).

[4] See, for example, [Carnap, 1937], pp *xiv* – *xv*.

can make inferences from one sentence to another). There are no constraints, as Carnap sees it, on which vocabulary and on which rules one may include in one's language. Carnap refers to this freedom frequently, and encapsulates it in his 'Principle of Tolerance'. The languages as conceived of in this manner come with a distinction between three groups of sentences: the 'analytic', which are consequences of the null class of sentences alone, the 'contradictory', which have every sentence as a consequence, and the 'synthetic', which are the rest.[5] On this view, logic is the analytic and the contradictory sentences, i.e. those whose validity does not depend on any other sentences.[6] Mathematics, then, is a part of logic in the sense that the validity of mathematical statements in no way depends on any other sentences, and in particular not the synthetic ones.

As I will show below, the development of Carnap's view is an interesting reflection of his intellectual history. Over the course of this chapter and the next, I will show how various parts of his view are assembled from the views of his main philosophical influences: Frege (section 2.2.1), Russell (section 2.2.2), and Wittgenstein (section 3.1). Some of this ground is already well known, and some has already been discussed in the literature, but many of what I see as the key bits are not as well known as they should be. It is my intention here to help correct this.

2.2 Logicisms: Frege, Russell, Carnap

A key piece of Carnap's view as presented in the last section is the claim that mathematics is a part of logic. Historically this has been one formulation of a position called 'logicism', and Carnap often referred to himself as a logicist.[7] In some respects this is not surprising. After all, as an undergraduate he was a student of Gottlob Frege, one of the founders and staunchest defenders of logicism, and later in his career he carried on a long correspondence with Bertrand Russell who was one of the other main proponents of logicism. The effect of these interactions remained throughout his life, and in the *IA*, written some 43 years after his last class with Frege, he says,

> Whereas Frege had the strongest influence on me in the field of logic and semantics, in my philosophical thinking in general I learned most from Bertrand Russell.[8]

Further on he adds a bit more and says,

[5]This classification appears to commit Carnap to the inference *ex falso quodlibet*, or explosion. I will argue in chapter 6 below that he need not be committed in this way, and that it is a matter of pragmatic considerations that he proceeded in the manner that he did on this issue.

[6]If we were being completely rigorous here references to 'validity' would have to be replaced with talk of 'being a consequence'. I take it that in each place I use the word 'validity' it could be replaced by a suitable phrase that omits it in favor of 'being a consequence'. I trust that allowing myself this minor convenience will not cause any confusions.

[7]Other versions include the view that mathematics *is* logic, or that, whatever mathematics is, it can be fully represented using only logical definitions and inferences. A standard overview of logicism can be found in [Shapiro, 2000], chapter 5.

[8][Carnap, 1963], p 13.

> For me personally, Wittgenstein was perhaps the philosopher who, besides Russell and Frege, had the greatest influence on my thinking.[9]

While Carnap was influenced immensely by these two thinkers, the logicism he adopts is quite different from both. Below we will trace out in exactly which ways this is so. Of the third philosopher mentioned, Wittgenstein, it would be problematic to include him in a section on logicism. Instead, we will examine his influence on Carnap in section 3.1 below.

2.2.1 Frege

Gottlob Frege was a member of the faculty of mathematics at the University of Jena for 44 years.[10] During that time, he offered three lecture courses in logic entitled *Begriffsschrift* I, *Begriffsschrift* II, and "Logic in Mathematics". Generally there were not enough students to actually hold the lectures for any of these courses other than *Begriffsschrift I* which ran with a kind of regularity; by tradition in German universities there must be three people for a lecture to be held: the lecturer and two others. Indeed, *Begriffsschrift II* was held only once. Frege also offered lectures in other areas of mathematics and presumably these were better attended. Our examination of Frege's work will be split into two parts. In the first, we will give a brief account of his logicism, and the paradox that felled it. In the second, we will look at his influence on Carnap. The idea that Frege was a major influence on Carnap is common, and originates with Carnap himself as we will show in section 2.2.1 below. Our account of this idea will spell out what exactly this influence amounts to, with an eye to showing in what ways this interaction influenced Carnap's mature view.

Frege Before the Paradox

Frege is justifiably famous in modern analytic philosophy for several things. In this section, we will focus on one part of his views on the nature of mathematics, namely his logicism. As mentioned above, logicism is the view that mathematics either is logic in some substantial sense, or that it can be derived from logic with the help of definitions for mathematical concepts given in a logical notation. This latter version finds its beginning in Frege's first book, *Begriffsschrift*, published in 1879.[11] In the book he has two main goals. The first is to lay out his 'concept script', one of the very first systematically developed formal notations. With it, he showed how every statement could be analyzed in terms of functions and arguments. In the most basic case this would consist of a predicate being applied to an object. However, it was also possible to have predicates of higher types applied to lower-level predicates. This also

[9] [Carnap, 1963], p 25.

[10] This overview of Frege's academic career draws heavily on material in both [Beaney, 1997] and [Awodey and Reck, 2004].

[11] In what follows, I will use the word *Begriffsschrift* both as the title of Frege's book and as the name of his notational system. I trust that it will be clear from context which use I intend in each particular case.

has the effect of vastly simplifying the universe of discourse, as it now needs to include only two sorts of things – namely objects and predicates (though these come in various levels). He argues that, though the need for such a notation may not be immediately obvious, it will be invaluable in our formal reasoning, especially in mathematics and science. In his introduction to the book, Frege says,

> I believe I can make the relationship of my *Begriffsschrift* to ordinary language clearest if I compare it to that of the microscope to the eye. The latter, due to the range of its applicability, due to the flexibility with which it is able to adapt to the most diverse circumstances, has a great superiority over the microscope. Considered as an optical instrument, it admittedly reveals many imperfections, which usually remain unnoticed only because of its intimate connection with mental life. *But as soon as scientific purposes place great demands on sharpness of resolution, the eye turns out to be inadequate. The microscope, on the other hand, is perfectly suited for just such purposes, but precisely because of this is useless for all others.*[12]

The second goal is the investigation of the foundations of arithmetic. Indeed, this is the 'scientific purpose' that he has in mind in the passage above. As we noted in the first chapter, with the advent of set theory, the need for secure foundations became increasingly obvious inside the mathematical community. Since, as Frege also argues in the introduction to the *Begriffsschrift*, the firmest proofs are purely logical ones, he sets out to see how much of arithmetic he can derive directly from logic.[13] If he could derive it all, then the doubts about the foundations would be alleviated. Sadly, he did not manage to derive all of it, but he did show that mathematical induction could be analyzed purely logically.[14] Armed with this early success, he announces in the conclusion to *Begriffsschrift* his intention to give logical analyses of the other fundamental notions for arithmetic, and says,

> Arithmetic, as I remarked at the beginning, was the starting point of the train of thought that led me to my *Begriffsschrift*. I therefore intend to apply it to this science first, seeking to provide further analysis of its concepts and a deeper foundation of its theorems. I announce in the third Part some preliminary results that move in this direction. Progression along the indicated path, the elucidation of the concepts of number, magnitude, etc., will form the object of further investigations, to which I shall turn immediately after this work.[15]

With the analyses Frege announces here, he would have all the tools necessary to derive the whole of arithmetic from logic, and therefore to place it on firm

[12] [Frege, 1997a], p 49. My emphasis.
[13] [Frege, 1997a], p 48.
[14] For a modern rendition of Frege's analysis, see [Beaney, 1997], pp 75 – 76.
[15] [Frege, 1997a], pp 51 – 52.

foundations. However, the task was perhaps harder than the success he had enjoyed in *Begriffsschrift* made it appear, and his next book did not appear for five more years.

In the meantime, reception of *Begriffsschrift* was poor, and the book fell almost entirely on deaf ears. Few knew of Frege's work, and those that did failed to appreciate what he had accomplished. There were some published reviews, but they were almost entirely negative. A paradigmatic example is the review written by the English mathematician John Venn who says,

> But, making all due allowances for these considerations, it does not seem to me that Dr. Frege's scheme can for a moment compare with that of Boole. [. . .] I have not made myself sufficiently familiar with Dr. Frege's system to attempt to work out problems by help of it, *but I must confess that it seems to me cumbrous and inconvenient.*[16]

As Beany notes in his [Beaney, 1997], even sympathetic readers thought that an exposition of his ideas in plain language, rather than in his formalism, might assist him in answering some of his critics.[17] His second book goes a fair ways towards this aim.

Frege's *Die Grundlagen der Arithmetik*, is now a classic in analytic philosophy, and as such needs no introduction. Suffice it to say that in it Frege powerfully answers many of the criticisms that were leveled at *Begriffsschrift*, and also advances his positive research program of giving logical analyses of mathematical concepts. The major result that Frege manages in *Grundlagen* is an analysis of the concept 'number'. Essentially, Frege held that numbers were objects that, though abstract, were nonetheless objective. He has three arguments for why, all based to a greater or lesser extent, in his philosophy of language.[18] The first argument that Frege offers is to note that sentences like "Jupiter has four moons" are really ascriptions about the concept ". . . being a moon of Jupiter". Namely, it ascribes the property of having four instances to that concept. We might be tempted on that basis to say that what numbers are is second level predicates, which is to say that they are predicates of predicates. However, in these cases the number word, like 'four' in our example above, is just a part of the predicate, not the whole of it. So, were we to think that numbers were predicates in this way, then we would also have to think that there were different numerical predicates for each concept. Moreover, in other sentences numbers can be the subject of the sentence (for example, in the sentence "The number 2 is an even number"). Given this, then, it would be misguided to think that numbers are predicates of any level. Instead, they should be objects. The second argument is that we use definite articles to refer to numbers, as in the sentence "*The* number 2 is prime", coupled with the observation that we generally use definite articles to refer to objects. Finally, Frege notes that a hallmark of objecthood is the ability to say *which* object we are referring to by

[16][Bynum, 1973], pp 234 – 235. My emphasis. Beany also notes this review in his [Beaney, 1997], p 4.

[17][Beaney, 1997], pp 4 – 5, and p 83 fn 18.

[18]Here I follow the presentation of this material in [Wright, 1983], pp 10 – 12. Frege's original can be found in [Frege, 1980], §57.

means of identity statements. So, if numbers are to be objects, then we need to see if there are contexts in which the right kind of identity claims for numbers occur. Of course, we need not look far since number theory gives us the kinds of claims we want. There, we make both existence and identity statements about numbers regularly. The net effect of all of these arguments is to proceed by analogy. Frege shows that there are significant similarities between our talk where the subject of the sentences are uncontroversially objects and our talk when the subject is a number. Wright puts the point this way,

> According to this reading, then, Frege is treating linguistic facts as decisive of whether or not a concept is genuinely sortal in the sense glossed earlier. Let us grant that the sort of considerations sketched go a good way towards establishing that the syntax of our numerical language can be very closely assimilated to that characteristic talk of less controversial kinds of object. [...] Frege's proposal, I suggest, is that the fact that our arithmetical language has these features is sufficient to set up natural number as a sortal concept, whose instances, if it has any, will thus be *objects*, furnishings of the world every bit as objective as mountains, rivers and trees. And, once again, that the concept does indeed have instances is settled by the truth of the appropriate arithmetical statements.[19]

What is important about Frege classifying numbers as objects on linguistic grounds in the way that he has done is that now number talk is located back in territory that he had established as logical in *Begriffsschrift*. So, now armed with the analysis of numbers in terms of logic, Frege is one very large step closer to his goal of giving a logical foundation to mathematics. There are only two more tasks to be accomplished: to set out the rules of inference (Frege's term is 'basic laws') that govern our arithmetical reasoning and argue that they too are purely logical, and finally to use these rules to actually derive the propositions of arithmetic on the basis of the analyses of the mathematical concepts.

The setting out of the basic laws of arithmetic and the demonstration of their logicality is the task that Frege sets himself in his last book *Grundgesetze der Arithmetik* (*Basic Laws of Arithmetic*), published in two volumes, the first in 1893 and the second in 1903. Despite the argumentative successes that he had enjoyed with the *Grundlagen*, his work remained mostly unread and so he had a great deal of difficulty finding a publisher who was willing to print *Grundgesetze*. It was finally published at Frege's own expense.[20] Though he had intended this to be his masterwork, summing up and improving on his previous results, and finally finishing the derivation he had announced 14 years earlier, as with his other works, publication of *Grundgesetze* went essentially unnoticed, until it came to the attention of Bertrand Russell. The result is extremely well known, but for reasons that will become clear in the next section it is worth pausing a bit on this episode.

[19] [Wright, 1983], p 13. Frege also has arguments for the objectivity of numbers separate from these linguistic considerations, though we leave them aside for reasons of space. The interested reader is commended to [Frege, 1980], §26.

[20] [Carnap, 1963], p 4.

The principle that was the undoing of Frege's version of the logicist program was, notoriously, his 'Basic Law V'. When combined with his commitment to unrestricted comprehension, Russell was able to use it to derive a contradiction in Frege's system. Initially, Frege was confident a solution could be found. In the appendix to *Grundgesetze* he famously says,

> The prime problem of arithmetic may be taken to be the problem: How do we apprehend logical objects, in particular numbers? What justifies us in recognizing numbers as objects? Even if this problem is not yet solved to the extent that I believed it was when I wrote this volume, nevertheless I do not doubt that the way to a solution has been found.[21]

However, it seems that despite the work Frege put in, no satisfactory fix could be found, and at some point Frege gave up. Based on research in Frege's *Nachlass*, Michael Dummett gives the date that Frege realized that his search for a solution was futile as sometime between April and August 1906.[22] With that, Frege tacitly acknowledged that his logicicist project was over, and that it had failed. With this overview of his position in hand, we can turn to the question of Frege's influence on Carnap.

What Carnap Learned from Frege

Carnap's family was from Wuppertal in western Germany near the Dutch border. However, after his father died in 1898, Carnap's mother decided to move the family first to the nearby town of Barmen, and then in 1909 across to the country to live with her older brother Wilhelm Dörpfeld, a noted figure in the history of archeology, who at that time was living in Jena.[23] This latter move was fateful for Carnap in many ways, but for our purposes it will suffice to remark on the sheer luck that he happened to be living in Jena when Frege was still active there, and that he decided to attend the University there.[24] Carnap was enrolled as a student there from the fall term of 1910 until the spring term of 1914. During that time, he studied with the neo-Kantian philosopher Bruno Bauch.[25] Bauch's influence on Carnap was strong at the time, but did not last. Exactly when Carnap stopped being interested in neo-Kantian themes is the subject of some debate, but suffice it to say that it did not survive the 1920s.[26]

[21] [Frege, 1997b], p 289.

[22] See [Dummett, 1991], p 6.

[23] [Carus, 2007], pp 45 – 46. Why Dörpfeld was living in Jena is unclear. Indeed, even more so since we know that by 1913 he had moved to the Greek island of Lefkas to pursue his archeological research. See [Bittlestone, 2005] for an overview of his work there.

[24] In the *IA*, Carnap notes that he also took classes at the University of Freiburg, located in Freiburg im Breisgau.

[25] Carnap's involvement with various parts of the neo-Kantian program has been the subject of quite a lot of research. Some excellent examples include [Friedman, 1999], [Friedman, 2000], [Stone, 2006], and [Carus, 2007].

[26] In his [Carus, 2007], Carus argues that Carnap was shaken loose from his neo-Kantian phase by his engagement with Bertrand Russell, which began in 1918 or 1919. Friedman, in his [Friedman, 1999], thinks that it lasted longer, up until at least the time that Carnap spent writing the *Aufbau*.

However, the impact that Frege had on Carnap was to last his entire life.

In the *IA* Carnap recalls Frege fondly. There is a relatively long passage describing his lecture style and personality. In addition, he mentions Frege as one of his influences several times throughout the *IA*. For example, he says,

> Whereas Frege had the strongest influence on me in the field of logic and semantics, in my philosophical thinking in general I learned most from Bertrand Russell.[27]

Further on in the *IA* he continues in the same vein by saying,

> For me personally, Wittgenstein was perhaps the philosopher who, besides Russell and Frege, had the greatest influence on my thinking.[28]

In total Carnap took five classes from Frege: the three that Frege offered on mathematical logic and two others entitled "Analytical Mechanics I" and "Analytical Mechanics II".[29] We know from the *IA* that he took *Begriffsschrift I* in the fall semester of 1910 more or less on a whim; a friend, who goes unnamed, told him that someone else had found the class interesting.[30] It is worth remarking on both how fortuitous and simultaneously how unsurprising it is that Carnap ended up in Frege's orbit. Prior to his arrival at university, he was actively involved with the German Youth Movement (*Jugendbewgung*), and even attended the first national meeting of the (then disparate) youth movement groups which had started to appear throughout Germany. These groups were intimately connected with a general trend towards *Lebensphilosophie*, that is, a kind of wholistic philosophy of life that was popular in Weimar Germany.[31] During his time enrolled in the University, he was associated with the youth movement group there, which called itself the Sera Circle (*Serakreis*). This group was rather eclectic, and it actively read and distributed pamphlets written by all of the competing factions within wider youth movement circles.[32] Given Carnap's association with the Youth Movement, in particular the 'Sera Circle' at the University of Jena, it is perhaps less surprising that he fell in with a somewhat offbeat crowd academically.[33] However, it is also worth remarking that his willingness to study mathematics, physics, and especially mathematical logic is, in fact, quite surprising. It is remarkable that someone who was so influenced by the *Jugendbewgung* in general and *Lebensphilosophie*, both of which often expressed broadly anti-scientific sentiments, chose to take up theoretical sciences. However, once this choice was made, Carnap's natural affinity for the subjects seems to have taken over. In the case of *Begriffsschrift II*, which was not offered until the summer semester of 1913, Carnap had to strong-arm a

[27] [Carnap, 1963], p 13

[28] [Carnap, 1963], p 25.

[29] See [Gabriel, 2004b], p 11.

[30] [Carnap, 1963], p 5.

[31] See [Forman, 1971] for a discussion of the interesting connections between *Lebensphilosophie* and positivism.

[32] [Carus, 2007], p 54 – 55.

[33] See [Carus, 2007], pp 50 – 56, as well as [Gabriel, 2004a].

friend, who Gottfried Gabriel identifies as Kurt Frankenberger, into attending with him so that the class would be held.[34] This fact already shows the extent to which Carnap was impressed by what he had seen from Frege in the first course, especially in the context of the two year gap between the two *Begriffsschrift* lectures. But, I contend that Carnap was not merely impressed. Rather, the experience of Frege's lectures was formative for key parts of Carnap's later view. In what follows, and by making use of Carnap's student notes from Frege's lectures, I will argue that what Carnap learned from Frege was not his logicism, but instead a general methodology.

As discussed above, by the time that Carnap was his student in 1910, Frege had realized that the full-blooded logicism he had advocated through his career had failed. In discussing his memories of Frege in the *IA*, Carnap says:

> I do not remember that he ever discussed in his lectures the problem of this antinomy and the question of possible modifications of his system in order to eliminate it. But from the Appendix of the second volume [of the *Grundgesetze*] it is clear that he was confident that a satisfactory way for overcoming the difficulty could be found. He did not share the pessimism with respect to the "foundation crisis" of mathematics sometimes expressed by other authors.[35]

The absence of any discussion of the paradox, or of possible solutions to it, is not surprising in light of the fact that Frege had given up on his logicism. However, he was not so disappointed by this failure to throw away the entirety of his work, and he carried on teaching his *Begriffsschrift*. As we see from Carnap's student notes, the system that Frege was teaching in 1910 was mostly that found in his 1879 book, though there were a few small changes.[36] Some of the changes include trading some of the axioms of *Begriffsschrift* for rules of inference, and indeed the inclusion of rules beyond *modus ponens*. However, we also know that, while Carnap was deeply impressed by the power of the *Begriffsschrift*, he gave up using it almost as soon as he encountered Russell's *Principia* notation, and so it cannot be the notation that had a lasting effect on his thought.[37] In the class not devoted to *Begriffsschrift*, entitled "Logic in Mathematics" (LM), Frege engages in a lengthy review of various foundational programs in mathematics. As one might have suspected from his writing, especially the *Grundlagen*, Frege is at his rhetorical best when mocking others' attempts at giving definitions of mathematical concepts. From Carnap's notes, it seems that LM was something akin to a live performance; Carnap even notes down the places where the audience laughs at Frege's snarky remarks. While Frege does a typically thorough job of demolishing the other views on offer, he does not go on to put forward a view of his own. Had this course been delivered in

[34] See [Carnap, 1963], p 5, and [Gabriel, 2004b], p 10 fn 21. As is well known, there was a third attendee at these lectures, who Carnap says was a retired army major. Gabriel, in the same footnote, identifies this man as Richard Seebohm.

[35] [Carnap, 1963], pp 4 – 5.

[36] Gabriel makes a similar point in his [Gabriel, 2004b], pp 2 – 10.

[37] [Carnap, 1963], p 11. More will be said about Russell's influence on Carnap, which was substantial, in section 2.2.2 below.

the 1890's, he presumably would have advanced his logicist view, but it is telling that in 1914 he stays silent. While Frege's rhetoric was no doubt effective, it is hard to imagine that this is what Carnap has in mind when discussing Frege's influence on him. So, if it is not the *Begriffsschrift* nor the content of LM that had such an impact, we must look elsewhere.

There is one more incident recorded in Carnap's *IA*, and in his student notes, that merits our attention, namely Frege's demonstration of the error in the ontological proof for the existence of god. What is interesting about this incident from our current perspective is not its content, but instead the methodology that Frege employs. Instead of engaging in metaphysical speculation about what it might be for there to be a most perfect being, he simply gives a formal treatment of the notions in play, in particular existence, and shows that the argument itself has misapplied the concepts.[38] It is this very same methodology that Carnap employs throughout his career. His famous article "The Overcoming of Metaphysics Through the Logical Analysis of Language" (OM) is an exercise in exactly that.[39] There, he shows the ways in which the misapplication of concepts can lead to nonsense formulations like "The Nothing nothings", and how these misapplications can be detected via logical analysis. We might wonder whether such a minor incident really could be *the* thing that made such a lasting impression on Carnap, indeed the discussion only takes up two pages in his notebook. However, even years later in his *IA* he remembers the proof clearly.[40] While this might seem like a minor thing to have taken away from his five lectures, it clearly colored Carnap's entire philosophical outlook.

Frege's impact on Carnap's thinking was not limited to their time together in the classroom. In fact, it seems clear from Carnap's comments on Frege, both in the *IA* and elsewhere, that to a large extent it was the sustained study of Frege's published work that Carnap undertook after his time in the army in World War I that effected his outlook most. It is from this period of study that Carnap came to the belief that mathematics is analytic and *a priori*. In the *IA*, Carnap says,

> I had learned from Frege that all mathematical concepts can be defined on the basis of the concepts of logic and that the theorems of mathematics can be deduced from the principles of logic. Thus, the truths of mathematics are analytic in the general sense of truth based on logic alone.[41]

In this way, then, Carnap credits Frege for his logicism. But, as we noted above, by the time Carnap attended his lectures, Frege was not teaching his logicist approach to the foundations of mathematics. Rather he confined himself to teaching the *Begriffsschrift* notation and to giving an examination of the deficiencies of certain well known programs in the foundations of mathematics.

[38] See [Awodey and Reck, 2004], pp 80 – 81, especially fn 19.

[39] [Carnap, 1959a], p 70. In Ayer's collection, this article is titled "The *Elimination* of Metaphysics Through the Logical Analysis of Language". However, I prefer to translate the German word 'Überwindung' as 'overcoming' as I think it is closer to the spirit of the original.

[40] [Carnap, 1963], p 6.

[41] [Carnap, 1963], p 46.

So, it would seem that the primary lessons Carnap took from Frege were gained not through his personal contacts, but by the study of his published work.

Before moving on to our examination of the influence of Bertrand Russell on Carnap's thought, we should sum up what we have seen so far. At the outset, we saw that Frege undertook to show that neither the empiricists nor the psychologists could give an adequate account of the nature of logic and its relationship to mathematics. He gave an account of the concept 'following in a series', as well as the concept 'being a number' on the basis of purely logical principles. From this account he showed that one could derive all of arithmetic by making use of only logical inferences, and that no 'intuition' of any sort was needed. However, the system that he used to make his analysis and do his derivation was fatally flawed. Despite initial optimism, by 1906 Frege had given up his attempts to rescue his work. By the time Carnap was his student, from 1910 to 1914, all that survived of Frege's work in his lectures was his notation and analyses of number and series, alongside his critiques of some of his rivals' views. From this, we concluded that it was not any substantial Fregean doctrine that Carnap learned from Frege, but rather the general convictions that: (1) logic was analytic, (2) the general logicist thesis that mathematics could be in some sense reduced to logic, and (3) the method of subjecting philosophical claims to logical scrutiny. However, none of these lessons were enough to deflect Carnap from pursuing a career in physics. For him to become the logician and philosopher that he was to be took an encounter with the work of the man who took up the logicist project when Frege abandoned it: Bertrand Russell. It is to his influence that we now turn.

2.2.2 Russell

As we noted in the previous section, Frege's version of the logicist program came to its notorious end with the discovery of a paradox lurking in his fifth Basic Law. This discovery was, of course, made by Bertrand Russell. Unlike Frege, however, the failure of this version of the logicist project did not discourage Russell from pursuing a slightly modified version of it. This pursuit eventually led to one of the most comprehensive books ever written in the foundations of mathematical logic, *Principia Mathematica* (*PM*), co-authored with his collaborator Alfred North Whitehead. It will be the purpose of this section to examine the influence that Russell, and in particular his logicism, had on Carnap's views on logic and the philosophy of mathematics. We begin with a description of Russell's logicism and the ways in which it differs from Frege's, before moving on in the second part of this section to discuss its impact on Carnap's development.

We begin with an account of Russell's view of the nature of logic. In general, his view is fairly straightforward. For example, in his book *Introduction to Mathematical Philosophy* (*IMP*), he says,

> Logical propositions are such as can be known *a priori*, without study of the actual world. [...] This is a characteristic, not of logical propositions in themselves, but of the way in which we know them.

> It has, however, a bearing upon the question what their nature may be, since there are some kinds of propositions which it would be very difficult to suppose we could know without experience.
>
> It is clear that the definition of "logic" or "mathematics" must be sought by trying to give a new definition of the old notion of "analytic" propositions. Although we can no longer be satisfied to define logical propositions as those that follow from the law of contradiction, we can and must still admit that they are a wholly different class of propositions from those that we come to know empirically. They all have the characteristic which, a moment ago, we agreed to call "tautology". This, combined with the fact that they can be expressed wholly in terms of variables and logical constants (a logical constant being something which remains constant in a proposition even when *all* its constituents are changed) – will give the definition of logic or pure mathematics.[42]

So, under the influence of Wittgenstein, Russell adopts the view that logic is analytic and *a priori*. Its statements are tautologies, though Russell admits that he does not have a satisfactory definition of what it is to be a tautology at the time he wrote the book.[43] Though he previously believed that logic had a special subject matter, on the basis of the thought that "every word occurring in a sentence must have *some* meaning", he came to the view that logic is linguistic from his work with Wittgenstein.[44] That is, while it is true that every word in a sentence must have some meaning, not all of the words, in particular the logical connectives and purely mathematical terms like numbers, need denote anything. However, he nonetheless continued to think that whatever mathematics is, it is derived from logic, and it is this portion of his view with which we are particularly concerned here.

Russell wears his logicism on his sleeve, and it is abundantly clear in several of his works. For example, at the beginning of his book *The Principles of Mathematics* he says,

> But now Mathematics is able to answer [the question of its own fundamental nature], so far at least as to reduce the whole of its propositions to certain fundamental notions of logic. [. . .] By the help of ten principles of deduction and ten other premisses of a general logical nature (*e.g.* "implication is a relation"), all mathematics can be strictly and formally deduced; and all the entities that occur in mathematics can be defined in terms of those that occur in

[42][Russell, 1920], pp 204 – 205. Original emphasis. In a footnote to the sentence immediately following the last in this quotation Russell credits Wittgenstein with showing him the tautological character of logical statements, and also famously comments that he is unsure as to whether Wittgenstein is "alive or dead" owing to his involvement in World War I.

[43][Russell, 1920], p 205. Prior to writing *IMP* it seems that Russell may have thought of logic as something akin to 'the most general science'. Indeed that view partially lingers here, and Russell says "Logic, I should maintain, must no more admit a unicorn than zoology; for logic is concerned with the real world just as truly as zoology, though with its more abstract and general features" ([Russell, 1920], p 169).

[44]See [Russell, 1996], p *x*.

> the above twenty premisses. [...] The fact that all Mathematics is Symbolic Logic is one of the greatest discoveries of our age; and when this fact has been established, the remainder of the principles of mathematics consists in the analysis of Symbolic Logic itself".[45]

In another example, from *IMP*, he puts matters quite directly:

> But both [mathematics and logic] have developed in modern times: logic has become more mathematical and mathematics has become more logical. The consequence is that it has now become wholly impossible to draw a line between the two: in fact, the two are one.[46]

Both of these quotations show that Russell was committed completely and without reservation to logicism, despite the paradox that he had discovered in Frege's version of it. In *IMP* he goes so far as to say that the proof that mathematics is just developed logic is simply "a matter of detail".[47] This commitment reached its ultimate expression in *PM*. There, Russell shows in extensive and painstaking detail precisely how one might accomplish the reduction of mathematics to logic. He proceeds in a manner similar to Frege by defining the basic notions of mathematics in terms of purely logical vocabulary, and then by deriving several important theorems using only his definitions and a few logical rules of inference.[48] Special attention is paid to developing his theory of types so as to avoid the paradox that beset Frege's work. What will draw our attention for the rest of this section is the way in which Russell's version of this project differs from Frege's. This difference centers on the question of how much of mathematics is covered by the claim that mathematics is logic.

For Frege, as we saw above, the task was to show that *arithmetic* could be reduced to logic, and thus to show that one need not invoke anything beyond logic to account for it. However, geometry was a different matter altogether. It could not be grounded in logic on his view, but required some other sort of justification. This point becomes quite clear when we examine the extended exchange of letters that Frege carried on with the mathematician David Hilbert during the early years of the 1900s. For example, in one of these letters he says,

> I call axioms propositions that are true but that are not proved because our understanding of them derives from that nonlogical basis which may be called intuition of space.[49]

[45] [Russell, 1996], pp 4 – 5. Hereafter *Principles*.

[46] [Russell, 1920], p 194.

[47] [Russell, 1920], p 194.

[48] This is somewhat contentious. Since the systems that both Russell and Frege used were high order (indeed, ω-order in Russell's case), some current philosophers might not want to call them logical. However, it was common at the time to regard set theory as a part of logic, as Quine points out in [Quine, 1994], p 348:

> In those days [during the writing of "Truth by Convention",] I still conformed to the usage of Frege, Russell, and Carnap in letting the word "logic" cover set theory.

[49] [Frege, 1971a], p 9. Unfortunately, no precise date is given for this letter.

In a similar vein, in his essay "On the Foundations of Geometry" published in 1903, Frege says, "Here we shall not go into the question of what might justify our taking these axioms to be true. In the case of geometrical ones, intuition is generally given as a source".[50] This stands in stark contrast to his project in earlier works, namely to disentangle arithmetic from any kind of dependence on intuition. So, even at this relatively late date in Frege's research into logicism (as mentioned previously, he was to abandon the project entirely just a few years later), he maintains a distinction in kind between arithmetic and geometry, and thinks that only the former can be reduced to logic.

Unlike Frege's view, Russell thought there was no difference in the justificatory grounds of arithmetic and geometry. Both were reducible to logic, and so inherited their characteristic certainty from it. By way of an example, consider the first chapter of *Principles*. There, he distinguishes pure mathematics by saying that a mathematical statement that contains no undefinable signs other than logical constants is pure; the rest he calls applied. However, this difference is only skin deep for Russell. The statements of applied mathematics, he says, are just substitution instances of pure mathematical statements, but with an extra premise added in. His example is Euclidean geometry:

> In applied mathematics, results which have been shown by pure mathematics to follow from some hypothesis as to the variable are actually asserted of some constant satisfying the hypothesis in question. Thus terms which were variables become constant, and a new premiss is always required, namely: this particular entity satisfies the hypothesis in question. Thus for example Euclidean Geometry, as a branch of pure mathematics, consists wholly of propositions having the hypothesis "S is a Euclidean space." If we go on to: "The space that exists is Euclidean," this enables us to assert of the space that exists the consequents of all the hypotheticals constituting Euclidean Geometry, where now the variable S is replaced by the constant *actual space*. But by this step we pass from pure to applied mathematics.[51]

It is quite instructive that the example Russell gives is that of a geometry. What it makes clear is that for Russell there is no difference between arithmetic and geometry, at least not from the perspective of the relationship between mathematics and logic. But, continuing the quotation from the beginning of this subsection, and just a few pages before the quotation just above in *Principles*, he is even more explicit:

> By the help of ten principles of deduction and ten other premisses of a general logical nature (*e.g.* "implication is a relation"), all mathematics can be strictly and formally deduced; and all the entities that occur in mathematics can be defined in terms of those that occur in the above twenty premisses. *In this statement, Mathematics includes not only Arithmetic and Analysis, but also Geometry,*

[50] [Frege, 1971b], p 23.
[51] [Russell, 1996], p 8.

Euclidean and non-Euclidean, rational Dynamics, and an indefinite number of other studies still unborn or in their infancy.[52]

So we see that Russell was quite clear on the matter: both arithmetic and geometry are logic. Moreover, both are reduced to logic via the same procedure. Indeed, there was even a planned volume of *PM* that was to deal specifically with the reduction of geometry to logic. Though this volume never appeared in print, the fact that it was planned as part of the work that was to be the fulfillment of the logicist program lends further support to the thought that Russell thought that *all mathematics*, and not just arithmetic, was part of logic. This difference is quite large, and it is worth pausing briefly to consider where it comes from.

At the beginning of the *Begriffsschrift*, Frege indicates that the motivation for his project is to secure the foundations of arithmetic, insofar as he can, by showing that its theorems can be proved purely logically, as opposed to requiring some empirical investigation as well. He says,

> The firmest proof is obviously the purely logical, which, prescinding from the particularity of things, is based solely on the laws on which all knowledge rests. Accordingly, we divide all truths that require justification into two kinds, those whose proof can be given purely logically and those whose proof must be grounded on empirical facts. [...] Now in considering the question of to which of these two kinds arithmetical judgments belong, I first had to see how far one could get in arithmetic by inferences alone, supported only by the laws of thought that transcend all particulars. The course I took was first to seek to reduce the concept of ordering in a series to that of a *logical* consequence, in order then to progress to the concept of number.[53]

The attitude here expressed, that his aim is to see how much of what we think is part of arithmetic can be reconstructed on the basis of logic alone, when combined with his arguments in *The Foundations of Arithmetic* to the effect that arithmetic cannot be grounded on the basis of either empirical knowledge or any sort of intuition, produces a normative picture. That is, whatever can be properly called arithmetic can be so called because it is constructable from purely logical laws on the basis of Frege's definitions of ordering in a sequence and of number. Even if, as a matter of fact, no changes in which theorems we think are properly arithmetical are called for, the possibility at least existed for Frege. In Russell's view, however, the project was not so much to (at least possibly) change what it is we think we know, but rather it was to account for the mathematical knowledge we do actually have. As Coffa puts the point,

> Logicism is often defined as the thesis that mathematics is reducible to logic. This is correct as long as one understands that at this early stage, mathematics was a reality and logic was a project. In

[52][Russell, 1996], pp 4 – 5. My emphasis.
[53][Frege, 1997a], p 48. Original italics.

> Russell's practice, at any rate, the logicist motto was less a doctrine than a regulative maxim, intended as much to provide a guide to characterizing logic as to clarifying mathematics.[54]

The task in Russell's view, then, was to arrange logic such that he could derive mathematics from it. We might give a fast gloss of this difference as 'logic first logicism' in Frege's case, and 'mathematics first logicism' in Russell's. To sum up the differences between them quickly, for Frege the logicist project flowed from logic to arithmetic, whereas for Russell it flowed from all of mathematics to logic. Before we move on to examining the influence Russell had on Carnap, there is a very important similarity between his view and Frege's that we should mention, namely that both of them saw logic as universal.

In Russell's view, as in Frege's before him, logic was the all encompassing framework for reasoning. While it was not exactly impossible for him to countenance certain kinds of reasoning about reasoning itself, he nonetheless thought that attempts to do so would be circular – that it would presuppose the very principles that were up for consideration. Milne makes this point clear in his [Milne, 2008],

> For Russell, logic is the theory of deduction, presented in an interpreted formal language (whose syntax is never properly stated!); the point of formalization is as a safeguard, an admittedly imperfect safeguard, against the unconscious employment of assumptions. As the theory of deduction logic lies back of any enquiry. Russell says exactly this at the start of ★5:
>
>> Treated as a 'calculus', the rules of deduction are capable of many other interpretations. But all other interpretations depend upon the one here considered, since in all of them we deduce consequences from our rules, and thus presuppose the theory of deduction. ([Russell, 1906], p 183.)[55]

However, Russell was not as meticulous as Frege about maintaining this boundary between first and higher order reasoning, so to speak. While Frege only rarely discusses what might be properly thought of as meta-theoretic concerns, Russell considers them several times, despite maintaining that one could not sensibly do so.[56] As we will see in section 4.2 below, this same tension between the desire to reason about logic, and the inability to step outside of it in order to do so, plays a key role in Carnap's development.

2.2.3 Russell's Influence on Carnap

In the *IA* Carnap says that by 1914 he had begun research into theoretical physics at Jena with the aim of writing a doctoral degree, but his research was

[54] [Coffa, 1991], p 113.
[55] [Milne, 2008], p 49.
[56] See, for example, [Russell, 1996], p 15.

interrupted by the outbreak of World War I.[57] After spending the war years in the army, first at the front and later at a research laboratory in Berlin, he returned to his research but was now unsure whether he wanted to do his degree in physics or philosophy. It was at this time that he read most of the works on logic that influenced his later views, including *PM*.[58] Of course, he had been aware of the book from Frege's lectures. In the *IA*, he says that the reason he became interested in pursuing more logic was with the aim of creating an axiom system for the use of the concepts of space and time in the physical sciences.[59] In his [Carus, 2007], Carus argues that the motivation for this project came directly from the war. Carnap, on Carus' account, thought that the catastrophe of war could only be avoided in the future if the rhetoric and fear mongering that had characterized the buildup was dispelled by a scientific approach to life, which would be expressed most clearly by the replacement of our natural languages with a scientifically constructed 'total system of concepts'. The guarantor that the problematic concepts would not make their way into the total system would be to derive the new language from a small number of unproblematic axioms. The model for this sort of thinking, he thought, was the logical analysis of mathematical concepts that Frege had given. It was, however, a further step to the claim that the same kind of analysis could be given for concepts that feature everywhere and not just in mathematics. Carnap had an inkling of these ideas, but the push to truly pursue them came when he read Russell's book *Our Knowledge of the External World*.[60] In the *IA*, Carnap recounts his reading in a dramatic fashion:

> In the winter of 1921 I read [Russell's] book *Our Knowledge of the External World, as a Field for Scientific Method in Philosophy.* Some passages made an especially vivid impression on me because they formulated clearly and explicitly a view of the aim and method of philosophy which I had implicitly held for some time. In the Preface he speaks about "the logical-analytic method of philosophy" and refers to Frege's work as the first complete example of this method. And on the very last pages of the book he gives a summarizing characterization of this philosophical method in the following words:
>
> > The study of logic becomes the central study in philosophy: it gives the method of research in philosophy, just as mathematics gives the method in physics [. . .]
> > All this supposed knowledge in the traditional systems must be swept away, and a new beginning must be made . . . To the large and still growing body of men engaged in the pursuit of science . . . the new method, successful already in such time-honored problems as number, infinity, continuity, space and time, should make an appeal which

[57][Carnap, 1963], pp 6 – 7.

[58]How Carnap managed to obtain a copy to read is a rather remarkable story. See [Reck, 2004] for the details.

[59][Carnap, 1963], p 12.

[60]See [Carnap, 1963], p 13.

> the older methods have wholly failed to make. ... The one and only condition, I believe, which is necessary in order to secure for philosophy in the near future an achievement surpassing all that has hitherto been accomplished by philosophers, is the creation of a school of men with scientific training and philosophical interests, unhampered by the traditions of the past, and not misled by the literary methods of those who copy the ancients in all except their merits.

I felt as if this appeal had been directed to me personally. To work in this spirit would be my task from now on! [...] I now began an intensive study of Russell's books on the theory of knowledge and the methodology of science. I owe very much to his work, not only with respect to philosophical method, but also in the solution of special problems.[61]

Carnap saw *PM* as a model of the kind of language construction that he needed, as it derived the language of mathematics from a small number of axioms. His post-war project was to extend this kind of analysis to the concepts 'space' and 'time'.[62] While his plans for the total system of concepts never coalesced into written form, the influence of the sustained study of *PM* that he had undertaken in preparation left a lasting impression.[63] As with the previous section on Frege, we now turn to what Carnap took from Russell's logicism into his own.

The story is not a complicated one. After reading *PM* in 1919 while he was preparing to write his PhD dissertation, Carnap came to the view that all of mathematics could be reduced to logic, not just arithmetic as he had learned from Frege. In fact, what he took from *PM* was even stronger than that. He saw that there was no longer a question of whether mathematics *could* be reduced to logic, but rather just a mission to let the rest of the world know that it *already had been.* Or, to put the point slightly differently, in Carnap's view after reading *PM* the logicist claim was no longer a philosophical one, but rather it was a technical one; it was not a claim that the fundamental nature of mathematics was logical (or some other metaphysical claim), but rather a technical challenge to show that one in fact could give an account of the propositions of mathematics using only logical rules and definitions. This challenge, Carnap thought, was met by Russell and Whitehead's book, albeit with a few small difficulties. As a consequence of this view, Carnap also came to think that all mathematics are logic. It is not entirely clear that this constitutes a change in his view, and he may have never followed Frege in thinking that it was only arithmetic that was amenable to the logicist reduction. This may be due to the fact that he

[61] [Carnap, 1963], p 13.

[62] See [Carus, 2007], chapter 1, in particular the section entitled 'War and Revolution', and chapter 3.

[63] It is possible that some pieces of the 'total system' project survived and were incorporated into Carnap's *Aufbau.* See [Carus, 2007] chapters 5 and 6 for an account of the development of that idea. See also [Richardson, 1998] chapter 1.

only became aware of Frege's philosophical views on the matter around the same time that he began to study Russell's position.[64] It is similarly unclear whether or not Carnap adopted the view that all of mathematics is logic precisely *from* Russell. Nonetheless, this is a strong similarity between their views. In these respects, then, Carnap's logicism owes quite a lot to Russell.[65]

2.3 Conclusion

In this chapter, we have examined Carnap's interactions with the two main figures in the logicist tradition, Gottlob Frege and Bertrand Russell. The impact of their ideas on him was deep, lasting from his years as a university student until the end of his life. Ever after Frege mentioned it in a lecture, Carnap never lost the convictions that mathematics could be reduced to logic, and that it was analytic. Additionally, Carnap took Frege as the model for his overall strategy in approaching philosophical problems; he would employ this model for the rest of his philosophical career. After reading Russell and Whitehead's *PM*, he saw how the reduction could be done. He was also impressed by the passion in Russell's writing, as, for example, in the Supreme Maxim of Scientific Philosophizing, "Wherever possible, logical constructions are to be substituted for inferred entities".[66] Carnap took up the banner of scientific philosophy, and in doing so he caught the attention of the new movement of scientifically inclined philosophers that was taking hold in certain parts of Germany and Austria, further setting the stage for meeting for the last of the biggest influences on his philosophy of mathematics, Ludwig Wittgenstein. The interaction between them, and Carnap's development as a member of the Vienna Circle is the subject of the next chapter.

[64] [Carnap, 1963], p 6.

[65] Though it is well known that both Russell and Frege were universalists about logic, the question of whether Carnap is a universalist as well has attracted some recent scholarship. In his [Schiemer, 2013], Georg Schiemer argues that Carnap could not have been a universalist in the mold of Frege and Russell because of the way he seems to be thinking of variable domains in his early model theory, presented in the posthumously published [Carnap, 2000]. However, this interpretation of Carnap's work has been challenged by Iris Loeb in her [Loeb, 2013]. For everything I want to say here, either account is fine. As I will argue in section 4.2 below, Carnap throws off his universalist shackles in the early 1930s as part of his move towards what I call a 'many-languages' view.

[66] [Russell, 1917], p 115.

Chapter 3

Carnap in Vienna

This chapter follows closely on the heels of the previous one in that its primary concern is with two more of Carnap's influences: Ludwig Wittgenstein and the Vienna Circle. However, unlike those in the previous chapter these two influences are not easily categorized together under a single banner. There is quite a lot of overlap since, as we will see, Wittgenstein was also a major influence on the Circle. But, the Circle was not as homogenous a group as it might at first appear. There were major differences between what we will call the 'left-wing' of the Circle, and the 'right-wing'. These differences largely centered on two questions: the nature of meaning, and the form that 'protocol sentences' should take. As I will argue below, it is in attempting to reconcile the tension between these three views that produces Carnap's own unique position. The chapter will be divided into two sections. In the first we address the effect that Wittgenstein had on Carnap. In particular, we show how the Vienna Circle's reading of Wittgenstein's *Tractatus Logico-Philosophicus*, and their adoption of certain ideas from it, caused serious difficulties for their project of separating science from metaphysics. In the second, we present Carnap's solution to these difficulties, and argue that it was through his thinking on this issue that his resolution to certain difficulties in the foundations of mathematics and logic became apparent to him.

3.1 Wittgenstein

The first influence on Carnap's thought that we take up in this chapter is the Viennese philosopher Ludwig Wittgenstein. His impact on Carnap's thinking comes especially through his short book *Tractatus Logico-Philosophicus*; they also had a series of conversations in the late 1920s, about which more will be said below. Carnap first encountered the *Tractatus* in 1922, shortly after it was published. In the *IA*, he mentions that on first reading the work did not seem very impressive.[1] The story of how his opinion changed, and how Wittgenstein became one of the most important influences on Carnap's thinking is also the story of much of Carnap's time in Vienna with the Circle. In this section we

[1] [Carnap, 1963], p 24.

will give an historical account of Carnap's move to Vienna, and his interactions with Wittgenstein while he was there. We will also detail those parts of Wittgenstein's thought that Carnap adopted and eventually abandoned, as well as those parts that he retained in his view, at least through the writing of *LSL*. We begin with an account of how Carnap came to Vienna.

3.1.1 Carnap, The Vienna Circle, and Wittgenstein's *Tractatus*

Filled with missionary zeal by Russell's stirring writing, in the early 1920's Carnap set to work organizing conferences on the new scientific conception of philosophy. Initially, he had planned to hold two conferences, one on 'The Theory of Relations as a Tool for the Epistemologist' and another on his manuscript entitled *From Chaos to Reality*. In the end, only one of the conferences came to pass, and it was a kind of hybrid between the two planned ones. It was held in 1923 at Erlangen, near Nuremberg in Germany.[2] While Carnap's ideas were generally well received at the conference, though perhaps not entirely understood, more important for his future philosophical development was the chance to meet many of the like-minded thinkers in Germany at the time. Attendees included Heinrich Behmann (a student of David Hilbert's at Göttingen, and a long time correspondent with Carnap), and, most importantly for our present purposes, Hans Reichenbach.[3] The meeting at the conference was fateful, as it was through Reichenbach that Carnap was introduced to Moritz Schlick, the organizer and leader of the Vienna Circle.[4]

Despite Carnap and Schlick's association beginning in 1923, Carnap did not actually go to Vienna until 1925. When he finally did go it was at Schlick's invitation, and while there he presented a series of talks to the Circle on early drafts of the material that was to become his *Aufbau*. Sufficiently impressed by what he saw, Schlick invited Carnap to join the faculty at the University of Vienna as a *privatdozent*. In 1926 he moved to Vienna full-time and took up the post, which he held until the summer of 1931. In the years prior to Carnap's arrival, the Circle had engaged in a reading of the *Tractatus*, primarily at the suggestion of the junior members, in particular Herbert Feigl. Their discussions of the text were led by Hans Hahn.[5] When Carnap arrived in 1926, the Circle took up reading Wittgenstein's work for a second time. Carnap recounts the meetings of this second reading in the *IA*,

> In the Vienna Circle, a large part of Ludwig Wittgenstein's book *Tractatus Logico-Philosophicus* was read aloud and discussed sentence by sentence. Often long reflections were necessary in order to find out what was meant. And sometimes we did not find any clear

[2]For Carnap's comments on the Erlangen conference, see [Carnap, 1963], p 14. Carus gives a more comprehensive account of the conference at [Carus, 2007], pp 154 – 160.

[3]For more on the relationship between Carnap and Behmann, see [Reck, 2004], p 167. We will also return to this relationship in the next section below. Some small details of the relationship between Carnap and Reichenbach can be found in [Feigl, 1969].

[4]Carus comments that Schlick was actually invited to the Erlangen conference, but was not able to attend. See [Carus, 2007], p 157.

[5]See [Feigl, 1969], pp 634 – 638.

> interpretation. But still we understood a good deal of it and then had lively discussions about it.[6]

In the meantime, and through no small amount of personal effort, Schlick became acquainted with Wittgenstein. Wittgenstein resolutely refused to attend the regular meetings of the Vienna Circle, but, after some initial hesitation, he did agree to hold philosophical conversations with Schlick and a few others. Carnap was invited to participate as well as Freidrich Waismann, Feigl, and his wife Maria Feigl.[7] It was during the course of this second reading, as well as the subsequent conversations, that Carnap began to change his mind about Wittgenstein's ideas. There are two key themes in the Circle's reading of the *Tractatus* that we will focus on.[8] The first is the relationship between Wittgenstein's rejection of metaphysics and verificationist presentation of the *Tractatus* on the one hand, and the Circle's own rejection of metaphysics and verificatonism on the other. The second theme is Wittgenstein's view of logic as both tautological and universal. We take up each of these in turn in the remainder of this chapter.

3.1.2 Verificationism and the Rejection of Metaphysics

As mentioned briefly in the previous section, in 1926 and 1927, the Circle used their meetings as a reading group to work through Wittgenstein's *Tractatus* for a second time. It was this focused reading and the results of the corresponding discussions that formed the core of many of the doctrines that we commonly associate with the Circle. The first of these doctrines that (at least partially) came from this reading is their verificationism, perhaps the doctrine for which they are most (in)famous.[9] They interpreted Wittgenstein to have given an analysis of the meaningfulness of language that was based on its relationship to the world. In particular, he seemed to the Circle to be saying that the only way a proposition could be regarded as true was by comparing it with the way the world actually is. He further seemed to be saying that the way this comparison was done was via the natural sciences. For example, in the 4s Wittgenstein says:

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[6] [Carnap, 1963], pp 24 – 25.

[7] Records of some of these conversations were eventually published as [Waismann, 1979] and [Wittgenstein and Waismann, 2003]. [Waismann, 1979] also has a brief description of how Schlick came to know Wittgenstein (pp 12 – 16).

[8] Rather a large amount has been written about Wittgenstein's influence on the Vienna Circle, and on Carnap in particular. The reader is commended to [Uebel, 2004], and [Awodey and Carus, 2007] for detailed examinations. For a general account of the meetings, see also [Feigl, 1969], pp 637 – 639.

[9] I say partially because, in fact, the Circle had a long history with various versions of verificationism dating back to the first Vienna Circle, and indeed further to the work of Ernst Mach and the positivists of the late 19^{th} century. See [Feigl, 1969], and also [Neurath, 1973] for the Circle's own official account of this history. It should be further remarked here that assigning authorship to [Neurath, 1973] is rather difficult. I have chosen to attribute it to Otto Neurath because he wrote the first draft. However, both Carnap and Hans Hahn assisted in writing and editing, and several other members of the Circle also made editorial contributions. See [Neurath, 1973], fn 2 for Marie Neurath's comments on the issue.

> Reality is compared with the proposition. [4.05]; Propositions can be true or false only by being pictures of the reality. [4.06]; The totality of true propositions is the totality of natural science (or the totality of the natural sciences). [4.11]; Philosophy is not one of the natural sciences. [4.111]

If the only true propositions are those of the natural sciences, and if philosophy is not one of those sciences (as proposition 4.111 says), then none of the propositions of philosophy can be true. This was music to the Circle's ears, and one of the projects they saw themselves as actively engaged in was sweeping away the old systems of philosophy to make room for the products of modern science. But the picture that Wittgenstein presents in the *Tractatus* is more nuanced than a simple dismissal of traditional philosophy. What he is writing about in the quotation above is what, in his idiom, can be said – that is, what can be expressed in language. But, he goes on to say that there are some very important things which *cannot* be expressed in language.[10] These things can only be shown, that is, exhibited by language but not expressed in it. This distinction plays a crucial role in Wittgenstein's thinking in the *Tractatus.* One example that plays a crucial role in the present investigation is the notion of logical form, which he says can only be shown.[11] As we will see below in section 4.2, this inability to speak about logical form is one of the eventual causes of Carnap's break with Wittgenstein's philosophy of logic. However, for the time being, we focus our attention on the way in which this saying/showing distinction gives rise to Wittgenstein's theory of the nature of logic.

As quoted above, Wittgenstein holds that atomic propositions are true or false in virtue of their relationship to reality. That is, if they are pictures of the way the world is, then they are true, and conversely if they are not pictures of the way the world is, they are false. Propositions can be combined, using the logical connectives, which results in a 'molecular proposition'. These molecular propositions are truth functions of the atomic ones.[12] But, certain ways of combining propositions come out true (or false) no matter which way the world is, due to the particular logical form of these statements. Wittgenstein calls these twin cases the 'extremes', and labels them tautologies in the true case, and contradictions in the false one.[13] He further says of them that they have no sense, because they are unverifiable and so say nothing. But he goes on to say that the laws of logic are not thereby *senseless.* The truths of logic are said to be tautological, and so without sense but not senseless. All of this the Circle swallowed wholesale.

However, the *Tractatus* is not the verificationist, anti-metaphysical, and logically grounded account of meaning that the Circle took it to be. Another major feature of the book, and indeed one that lives uneasily alongside the themes discussed above, is its transcendentalism and mysticism. While these themes run right through the book, they become prominent towards the end in

[10][Wittgenstein, 1983], propositions 6.54 and 7.
[11]See [Wittgenstein, 1983], propositions 4.121, 4.1212, 6.1, and 6.11.
[12][Wittgenstein, 1983], proposition 5.3.
[13][Wittgenstein, 1983], proposition 4.46.

the 6s and proposition 7. For example, Wittgenstein says

> For an answer which cannot be expressed the question too cannot be expressed. *The riddle* does not exist. If a question can be put at all, then it *can* also be answered. [6.5] We feel that even if *all possible* scientific questions be answered, the problems of life have still not been touched at all. Of course, there is then no question left, and just this is the answer. [6.52] There is indeed the inexpressible. This *shows* itself; it is the mystical. [6.522]

Sentiments such as these were extraordinarily important to Wittgenstein's meaning in writing the *Tractatus*. Despite the fact that these comments were much too metaphysical for the Circle's liking, they nonetheless could not simply ignore their inclusion in the book. We might expect, then, to see the Circle repudiate the book, or at least see their enthusiasm for it somewhat tempered by the mystical parts. However, we do not see any such thing. Instead, it is held up as a model of the kind of philosophy they admire, and they cite Wittgenstein often and with approval.[14] An important question that arises is how could the Circle have misunderstood the importance of the metaphysical portions of Wittgenstein's thought so badly?

First, we must remark that the Circle did not blithely ignore the parts of the book they found troubling, and its members were quite up front with the fact that they had not fully understood Wittgenstein's meaning. For example Feigl says, "In the Circle we began to penetrate Wittgenstein's ideas on the nature of language and its relation to the world, his repudiation of metaphysics (notwithstanding a few aphorisms toward the end of the *Tractatus* that had a mystical flavor), and his conception of logical and mathematical truth".[15] Carnap says something similar in the *IA* when discussing the Circle's reading of the book,

> In the Vienna Circle, a large part of Ludwig Wittgenstein's book *Tractatus Logico-Philosophicus* was read aloud and discussed sentence by sentence. Often long reflections were necessary in order to find out what was meant. And sometimes we did not find any clear interpretation. But still we understood a good deal of it and then had lively discussions about it. [...] Earlier, when we were reading Wittgenstein's book in the Circle, I had erroneously believed that his attitude toward metaphysics was similar to ours. I had not paid sufficient attention to the statements in his book about the mystical, because his feelings and thoughts in this area were too divergent from mine.[16]

He goes on to say that it was only in the course of personal interactions with Wittgenstein that he came to see how different his views, and indeed the views of the majority of the Circle's members, on metaphysics were from Wittgenstein's. However, the Circle's misunderstanding was not simply a case of wishful

[14] See for example [Neurath, 1973], p 307 or [Carnap, 1959a], p 65.
[15] [Feigl, 1969], p 634.
[16] [Carnap, 1963], pp 24 – 27.

reading, so to speak. By the time that Wittgenstein agreed to hold discussions with some of the members of the Circle, he had changed his views somewhat. For example, in a conversation from 1930 Waismann reports that Wittgenstein said:

> To understand a proposition means to know how things stand if the proposition is true.
> One can understand it without knowing *whether* it is true.[17]

These first few sentences sound more or less Tractarian; compare them with proposition 4.024: "To understand a proposition means to know what is the case, if it is true. (One can therefore understand it without knowing whether it is true or not)". So, to an audience as familiar with the *Tractatus* as the Circle was it may have sounded as if Wittgenstein's current view was the same as the one he had advanced in the book. However, elsewhere he would say things that were drastically different. For example, from the very same conversation as the previous quotation:

> A proposition cannot say more than is established by its method of verification. [...] *The sense of a proposition is the way it is verified.* Sense itself is a method of verification; that method is not a means, not a vehicle. [...] To say that a statement has sense means that it can be verified.[18]

And elsewhere, he carried on in a similar fashion:

> The sense of a proposition is the method of its verification. A method of verification is not the means of establishing the truth of a proposition; it is the very sense of a proposition. In order to understand a proposition, you need to know the method of its verification. A proposition can only say what is established by the method of its verification.[19]

In other words, here Wittgenstein claims that the method of verifying a proposition is its sense, and that in order to understand that proposition one must

[17] [Waismann, 1979], p 244.

[18] [Waismann, 1979], p 244. Original emphasis.

[19] [Waismann, 1979], p 227. As I will discuss below, this idea was known in the Vienna Circle as the Verifiability Principle, and the Circle was always careful to note that they were indebted to Wittgenstein for it. However, in their reminiscence of Wittgenstein, published shortly after his death in 1951, Gasking and Jackson say that Wittgenstein is wrongly cast as an adherent of this principle. Rather, they suggest that this was simply one proposal of his amongst many ([Gasking and Jackson, 1951], p 79). This picture of Wittgenstein is hard to square with the views expressed in Waismann's record of the conversations from this period. Perhaps Wittgenstein engaged in a bit of revisionist history later in life when Gasking and Jackson knew him; perhaps he did hold that there were many ways of coming to know the meaning of a proposition and these other ways were left out of the conversational record; perhaps he simply misremembered what he had said nearly twenty years earlier. In any case, these issue need not be settled here. What is important to our present inquiry is that the Circle was committed to the Verifiability Principle for a time, and that in their mind it was due to Wittgenstein.

know the method of its verification. As we pointed out above, on Wittgenstein's view if a proposition were to have no method of verification then it would say nothing at all. However, in the *Tractatus*, all that was required to understand a proposition is that one knows how things would stand were the proposition to be true. One could do this without knowing the method of verifying the proposition. Thus, the position Wittgenstein presented in conversation is a purified form of verificationism. Despite this, we also learn from Carnap's recollections of these conversations that, no matter how large the shift towards verificationism that he had taken was, Wittgenstein had nonetheless not abandoned the mystical parts of the *Tractatus*. There was still a gulf in understanding between the members of the Circle and Wittgenstein, however. According to Carnap's later comments on the issue, it centered on the status of metaphysics.[20] But, as noted above, Wittgenstein's new position is couched in enough Tractarian language that Schlick, Carnap, and the other members of the Circle may well have thought that Wittgenstein was simply elucidating in conversation what he had written in the book. Additionally, the extremely verificationist position that he was taking in conversation would have been congenial to the Circle even if they disagreed on the certain metaphysical issues. However, as we will see below, there was a problem lurking in this way of understanding the view that was to dog the Circle for many years.

It is easy to understand, then, how the Circle came to understand the *Tractatus* in the way that they did. Firstly, given that they were already inclined towards a verificationist way of thinking, and given the difficulty of Wittgenstein's writing, they were inclined to see what they wanted in the book. Secondly, when they heard pronouncements in conversation like those quoted above, they seemed to think that Wittgenstein was in broader agreement with their own verificationism than might have actually been the case. This in turn they took as license to ignore those parts of the book that sounded too metaphysical for their liking. In fact, adopting this reading gave the Circle a framework which they could use to say many of the things they already wanted to say about traditional philosophy and its relationship with science. For example, just after citing Wittgenstein as one of the pioneers of the new way of doing philosophy in their 1929 manifesto, they say,

> Beyond this, the Vienna Circle maintain the view that the statements of (critical) *realism* and *idealism* about the reality or nonreality of the external world and other minds are of a metaphysical character, because they are open to the same objections as the statements of the old metaphysics: they are meaningless, because unverifiable and without content.[21]

[20][Carnap, 1963], p 27.

[21][Neurath, 1973], p 308. Original emphasis. The final clause of the last sentence might be read to suggest that the problem with metaphysical sentences is that they are meaningless for two reasons: (1) they are unverifiable and (2) they have no content. However, I believe this is just a rhetorical flourish. What is meant here is that they are meaningless only because they are unverifiable, though their unverifiablility also tells us that these statements have no content on the Circle's view. To a modern reader, this will sound quite strange as it makes clear that whatever notion of meaning the Circle had was not recursive. Consider the way that

The attitude expressed here is of a piece with Wittgenstein's view in the quotation above. In the Circle's parlance it was known as the 'Anti–metaphysical Stance' which was based on a principle they called the Verification Criterion for Meaning (VCM). In brief, and as the quotation from the manifesto makes clear, this principle states that in order for a statement to be meaningful it must be either analytic or empirically verifiable, where analytic means something like 'true in virtue of meaning' and empirically verifiable means complete verification.[22] Carnap enthusiastically took up this view, and for a time was one of its major champions. For example, in his polemical 1932 paper "The Overcoming of Metaphysics Through the Logical Analysis of Language", he says, "In the domain of *metaphysics*, including all philosophy of value and normative theory, logical analysis yields the negative result *that the alleged statements in the domain are entirely meaningless*".[23] What he goes on to argue, just as Wittgenstein had before him, is that the meaninglessness of metaphysical statements is caused by their unverifiability.[24] For example, while discussing the meaningfulness of individual words, he says, "Secondly, for a primitive sentence S containing the word an answer must be given to the following question, which can be formulated in various ways: [...] (4) How is S to be *verified*? ".[25] A bit later on, he reiterates and says,

> Let us briefly summarize the result of our analysis. Let "a" be any word and "S(a)" the elementary sentence in which it occurs. Then the sufficient and necessary condition for "a" being meaningful may be given by each of the following formulations, which ultimately say the same thing:
>
> 1. The *empirical criteria* for a are known.
> 2. It has been stipulated from what protocol sentences "S(a)" is *deducible*.
> 3. the *truth-conditions* for "S(a)" are fixed.
> 4. The method of *verification* of "S(a)" is known.[26]

statements are formed on their view: complex statements are built up out of truth-functional combinations of basic statements. If we take the disjunction of an unverifiable basic statement and some verifiable truth, we should be left with a meaningful true statement. But this cannot be the case for the Circle.

[22]See [Ayer, 1946], especially pp 5 – 9 for a discussion of the variations of the VCM that the Circle examined. [Hempel, 1959] also has a nice discussion of the VCM. We will return to the notion of analyticity and the need for it as a part of the VCM below and so leave it aside for now. Additionally, we will discuss the way in which Carnap's use of the term changes in *LSL* in section 4.2 below.

[23][Carnap, 1959a], pp 60 – 61. Original emphasis. Hereafter this paper is referred to as "OM".

[24]In fact, Carnap has two separate arguments in OM for the meaninglessness of metaphysical statements. The first is the Wittgenstinnian argument to the effect that metaphysical statements must be unverifiable. The second, which is more well known, is that metaphysical statements are simply not grammatically well-formed, though in some cases (pseudo-statements) their ill-formation is difficult to detect. See [Carnap, 1959a], section 4 for this second argument.

[25][Carnap, 1959a], p 62. Original emphasis.

[26][Carnap, 1959a], pp 64 – 65.

In the footnote appended to this list of conditions, Carnap directly cites the *Tractatus* as the "logical and epistemological conception which underlies [the Vienna Circle's] exposition". So, we can see from OM the extent to which Carnap and the Circle had adopted Wittgenstein's ideas and were, at least publicly, promoting them. However, though the Circle's, and Carnap's, affair with verificationism burned hot it was not to last.[27] It is also in OM that we get a glimpse of the cause.

For Wittgenstein in the *Tractatus*, every meaningful proposition is a truth function of atomic propositions. That is, complex propositions are built up out of atomic ones held together, so to speak, by the logical connectives. The Circle enthusiastically embraced this view. One consequence of it, however, is that since every meaningful proposition is a truth function of atomic sentences, then it should be possible to know conclusively for any meaningful proposition whether it is true or false. In conversation, Wittgenstein put the point this way:

> Is it *always* possible for me to doubt whether a proposition is verified? Could it not be the case that verifications only make it probable? But if I cannot specify under what conditions the proposition is to count as verified, I have not given the proposition a sense. **A statement that cannot be verified definitively is not verifiable at all**.[28]

The Circle referred to this part of the view as the Verification Principle (VP).[29] But there is a problem. The Circle was very scientifically minded, after all many of them had degrees in a science, and as such one thing they never wanted to happen was for their philosophical view to impede the practice of the modern sciences. However, the statements that make up our best scientific theories are never completely verified, nor can they be. Consider a law of physics, for example one of Newton's Laws. In order that it be completely verified, we must be able to know that there will never be a case where force acting on a body fails to be equal to its mass times its acceleration. This seems unlikely, as we know that there is no finite number of existential statements – that is, statements expressing single observations that we might make – from which we can derive a universally quantified statement. It is therefore a consequence of the VP that the statements of our scientific theories are senseless by Wittgenstein's lights because they are in principle not fully verifiable. In this way, then, the Circle's fears had been realized and their philosophical views had brought them into tension with scientific practice.

This result was deeply problematic for the Circle, but it was not the worst one they encountered with the VP. The truly fatal issue, from the Circle's perspective, comes from considering just how to spell out the nature of the primitive sentences. Initially, the Circle favored a positivistic account of these sentences. Under the influence of philosophers like Hume, Comte, and Mach, they thought

[27]For a survey of the Circle's affair and troubles with verificationism, see [Misak, 1995], chapters 1 and 2.

[28][Waismann, 1979], p 245. Original emphasis, my boldface.

[29][Carnap, 1963], p 57.

that an account of this kind was the best way to ensure an empirical grounding for knowledge.[30] It is precisely an account of this kind that Carnap develops in his *Aufbau*, which is perhaps the paradigmatic expression of the Cricle's view in the late 1920s. This positivistic account, in brief, holds that the primitive sentences are statements about sense data experiences, for example "A has an experience of red at position x, y, z in his perceptual field at time t".[31] But, as before, a problem emerges when we consider how we are supposed to do modern science on this picture. Put bluntly, the problem is this: it is supposed to be the case that all the statements in our scientific theories are deducible from the primitive statements. But, there is no chain of deduction that will ever get one from a (set of) primitive statements of the positivistic kind to a scientific law, as for example Newton's Laws of Gravitation. This follows from the fact that we only ever have finitely many observation statements, each of which is an existential statement, but scientific laws are universal sentences. Unfortunately for the Circle, this problem has nothing to do with the *form* of the protocol sentences, but only to do with logic. So any way they tried to spell out the nature of the primitive sentences, they still ran into the same difficulty. The only view on the issue that did not suffer from this problem is Carnap's, which we now take up.

These two problems with the Circle's philosophical stance, which are particularly troubling by the Circle's own lights, engendered one of the most significant internal debates in the Circle. At root, the debate concerned the nature of the primitive sentences, which the Circle referred to as 'protocol sentences'. In OM, Carnap summarizes the various positions that members of the Circle took up this way,

> For our purposes [in this article] we ignore entirely the question concerning the content and form of the primary sentences (protocol sentences) which has not yet been definitely settled. In the theory of knowledge it is customary to say that the primary sentences refer to "the given"; but there is no unanimity on the question what it is that is given. At times the position is taken that sentences about the given speak of the simplest qualities of sense and feeling (e.g. "warm", "blue", "joy", and so forth); others incline to the view that basic sentences refer to total experiences and similarities between them; a still different view has it that even the basic sentences speak of things. Regardless of this diversity of opinion it is certain that a sequence of words has a meaning only if its relations of deducibility to the protocol sentences are fixed, whatever the characteristics of the protocol sentences may be; [...].[32]

Each of the proposals in the quotation represents one of the positions that members of the Circle defended at some time. For example, under the influence of

[30] [Carnap, 1963], p 57. See also [Neurath, 1973], p 304 for a list of the Circles influences.

[31] In fact, there is a further problem regarding the first personal nature of such statements, but we leave that issue aside here. The reader is commended to [Richardson, 1996] for an examination of it.

[32] [Carnap, 1959a], p 63.

Wittgenstein's continued strict verificationism, Schlick and Waismann, grouped together as the 'right wing' of the Circle, preferred the positivistic view that the protocol sentences expressed simple attributions of sense or feeling. In response to the difficulties that this view engendered, Schlick simply accepted the (*prima facie* drastic) consequences that (1) statements that appear to express laws of science are not statements properly so called but instead are an important kind of nonsense, and (2) that the aim of science was not the discovery of laws, but rather it is to produce the 'sense of fulfillment' that comes along with the making of correct predictions.[33] So, with regard to (1) he says in his 1931 essay "Causality in Contemporary Physics":

> It has often been noted, indeed, that we can really never speak of the absolute verification of a law, since we always make the tacit reservation, as it were, that we may modify it on the strength of later experience. If I may say a few words in passing about the logical situation, the circumstance just mentioned means that at bottom a law of nature does not even have the logical character of an 'assertion', but represents, rather, a 'prescription for the making of assertions'. (I owe this idea and terminology to Ludwig Wittgenstein.)[34]

Laws of nature do not have the logical character of assertions because assertions are reducible to atomic propositions and, as we have seen, laws of nature are not. But, because they are not reducible in this way, they are also not proper statements at all, but rather meaningless prescriptions. On this view, laws of nature can be regarded as akin to rules for the logical connectives in that they indicate how to construct proper assertions.[35] Moreover, since laws of nature are meaningless, then the aim of science must not be to discover them. This result drives Schlick to say in his essay "The Foundation of Knowledge":

> If attention is directed upon the relation of science to reality the system of its statements is seen to be that which it really is, namely, a means of finding one's way among the facts; of arriving at the joy of confirmation, the feeling of finality. The problem of the "basis" changes then automatically into that of the unshakeable point of contact between knowledge and reality. We have come to know these absolutely fixed points of contact, the confirmations, in their individuality: they are the only synthetic statements that are not *hypotheses*. They do not in any way lie at the base of science; but like a flame, cognition, as it were, licks out to them, reaching each but for a moment and then at once consuming it. And newly fed and strengthened, it flames onward to the next.

[33] In his [Ayer, 1946], A. J. Ayer terms this acceptance "heroic", but goes on to say that the admission that the nonsense is of an 'important' kind is simply an acknowledgment that the view is, as things stand, deficient and in need of further development ([Ayer, 1946], p 37).

[34] [Schlick, 1979], p 188. Popper also notes this quotation at [Popper, 1959], pp 36 – 37, fn 4 and 7, and again at [Popper, 1945], p 282 – 284, fn 51.

[35] [Carnap, 1937], p 321.

> These moments of fulfillment and combustion are what is essential. All the light of knowledge comes from them. And it is for the source of this light the philosopher is really inquiring when he seeks the ultimate basis of all knowledge.[36]

This insistence on verification as the grounds of knowledge, and as the goal of science, stands in contrast to the 'left wing' composed of Carnap, Hahn, Neurath, Feigl, and Frank, who while they disagreed on precisely what solution should be adopted, nonetheless preferred a more liberal account that does not require the protocol sentences to be first–personal reports of sense experiences.[37] For example, Neurath believed that the notion of foundational sentences should be abandoned altogether, while Carnap, to some extent under the influence of Karl Popper, thought that the protocol sentences for a theory should be treated as hypotheses, and thus that any sentence whatsoever could play that role.[38] This proposal of Carnap's struck the Circle, even his colleagues in the left wing, as quite radical and it is worth pausing a moment to spell out clearly what it is and to show just how radical it is.

Carnap reports that he developed his views on the nature of protocol sentences while he was living in Prague. He had moved there in the summer of 1931 to take up a permanent position in the German University there, the Chair for Natural Philosophy which had been newly created at the suggestion of Phillip Frank.[39] Just as Carnap was departing Vienna for Prague, the tension in the Circle's views was starting to show. To make this clear, we will first examine the 1929 manifesto to see the view the left wing of the Circle advocated there. We will then look at Carnap's eventual view of the issue. In summarizing their view in the manifesto, the Circle writes,

> We have characterized the *scientific world-conception* essentially by *two features*. *First* it is *empiricist and positivist*: there is knowledge only from experience, which rests on what is immediately given. [. . .] The aim of scientific effort is to reach the goal, unified science, by applying logical analysis to the empirical material. Since the meaning of every statement of science must be stateable by reduction to a statement about the given, likewise the meaning of any concept, whatever branch of science it may belong to, must be stateable by step-wise reduction to other concepts, down to the concepts of the lowest level which refer directly to the given. [. . .] Investigations into the constitutive theory show that the lowest layers of the constitutive system contain concepts of the experience and

[36] [Schlick, 1959], pp 226 – 227. This text is somewhat confusing because, where Carnap distinguishes between verification and confirmation, Schlick uses these words interchangeably, and means verification.

[37] See [Carnap, 1963], p 57 for the genesis of the right-left distinction in the Circle. As to the differences inside the left wing, see [Uebel, 2004].

[38] [Carnap, 1959a], p 465. See also [Carnap, 1963], pp 57 – 58. For Popper's influence see [Popper, 1963a], and [Popper, 1963b] sections 16 and 17. The view that any sentence whatsoever could play the role of a protocol sentence was held only by Carnap, and never by Popper.

[39] [Carnap, 1963], p 33.

qualities of the individual psyche; in the layer above are physical objects; from these are constituted other minds and lastly the objects of social science. The arrangement of the concepts of the various branches of science into the constitutive system can already be discerned in outline today, but much remains to be done in detail.[40]

Perhaps it is unsurprising that in a programmatic pamphlet the Circle manged to present a univocal view, though considering the lack of consensus that characterized their meetings it is still somewhat remarkable. As a case in point, while most of the Circle agreed that there should be a single language for science, even this extremely general idea was not universally endorsed. Karl Menger often objected to this basic feature of the view.[41] It is possible that this univocality may have only been possible because Schlick was away in the United States on a visit to Stanford University at the time of its writing, and because the manifesto was prepared by only a small group of the members of the Circle.[42] In any case, there are three features of this supposedly uniform view as it is presented here that are important for our present purposes. The first is to note the confidence with which the view is expressed. The writers of the manifesto clearly have not yet seen the problems with the view they are advocating, and think that all that is left to do is to flesh out the details. The second feature is the way requirements are laid down on the project: only systems that are (1) positivistic, and (2) have a first person perspective built in at the lowest level are acceptable. Finally, we should note that direct reference is made to "the given". All of these features are absent from Carnap's eventual view, as we will see below.

It is striking how far Carnap had already diverged from both the Circle's and Wittgenstein's stances by the time he wrote OM (primarily in 1931), even though the view he describes in the paper is the classic Circle one. As noted just above, there he says that there is no longer a settled opinion on the nature of protocol sentences, though he does express optimism that the problem will eventually be resolved. But, as he continued to think on this issue in Prague, his view continued to diverge from the old Circle view. By his own account, quite a lot of the initial writing and thinking on the protocol sentence debate took place in 1932, and in that year as well as the next few he published regularly on the issue in the Circle's journal *Erkenntnis*. For example his essay "Über Protokollsätze" ("On Protocol Sentences" (OPS)) appeared in the 1932/1933 edition, as did several papers by other members of the Circle. The view he develops in OPS marks a radical departure from the old Circle view. On the surface, the subject of the paper is to determine whether protocol sentences must be regarded as "inside" or "outside" the language of science, or, in other words, whether the protocol sentences are sentences of the language or not. This is already a remarkable departure from the single language approach that the

[40] [Neurath, 1973], p 309. Original emphasis. Because of their general resemblance to the project that Carnap undertook in the *Aufbau*, I believe the sentences after the ellipsis are Carnap's rather than Neurath's.

[41] [Menger, 1994], p 141. We will say much more about Meger and his interactions with the Circle in section 4.2.2 below.

[42] [Kraft, 1952], pp 4 – 5.

Circle took earlier, grounded as it was in Wittgenstein's conception of language as universal. It is worth dwelling on this point in order to appreciate just how large a change in view this paper represents. At the outset of the paper, after discussing some of the recent debate, Carnap says:

> The questions of whether the protocol sentences occur outside or inside the system language and of their exact characterization are, it seems to me, not answered by assertions but rather by postulations. [...] [Answers to questions of this kind] are to be understood as suggestions for postulates; the task consists in investigating the consequences of these various possible postulations and in testing their practical utility.[43]

What should strike us immediately about this quotation is Carnap's new pragmatic orientation. This is to say that, on Carnap's new view, disputes over the correct form of the language of science should be resolved by treating rival views as proposals, and then investigating the pragmatic consequences of adopting these proposals. In the heart of the essay, he goes on to examine two such proposals, one due to Neurath and one he credits to Popper, before coming down in favor of a kind of combination of the two toward the end. But, again, these considerations take a secondary role to his methodological point. Just before coming to a determination of which of the proposed languages he prefers, Carnap praises both of them by saying,

> In all the theories of knowledge up until now there has remained a certain absolutism: in the realistic ones an absolutism of the object, in the idealistic ones (including phenomenology) an absolutism of the "given", of "experience", of the "immediate phenomena". There is also a residue of this idealistic absolutism in positivism; in the logical positivism of our circle [...] it takes the form of an absolutism of the ur-sentence (the "elementary sentence", "atomic sentence"). Neurath has been the first to turn decisively against this absolutism, in that he rejected the unrevisability of protocol sentences. From other starting points Popper has succeeded a step further: in his testing procedure there is no last sentence; his system describes therefore the most radical elimination of absolutism.[44]

Despite the tone he takes here, there was nonetheless still a certain absolutist strain in Carnap's view. He says of the two proposed languages that they are both acceptable languages for science because they "can be carried through consistently". It is not clear precisely what he means by this phrase, but it nonetheless comes through as another requirement on proposed languages over and above that of the pragmatic consequences of their adoption. In any case, Carnap did not maintain this view much beyond the publication of OPS, though he continued to think and write on the issue.

Carnap's considered view on the protocol sentence debate did not appear in print until 1936 when it was published in the journal *Philosophy of Science* as

[43] [Carnap, 1987], p 458.
[44] [Carnap, 1987], p 469.

a two-part monograph under the title *Testability and Meaning* (TM).[45] Essentially, the view Carnap presents in TM is a restrained, and somewhat muted, version of the position he argued in OPS. There are two basic claims that make up the view, which are: (1) sentences are cognitively meaningful just in case they are analytic or confirmable, and (2) which terms and sentences are taken as basic for the language is a matter of free choice, or convention. We will examine each of these two in turn. While (1) fulfills the same role in the overall position as the VCM did, there are two significant differences between it and the older Circle view. Firstly, while the VCM is a criterion for meaningfulness full stop, (1) is a criterion for being cognitively (or sometimes 'factually') meaningful. The distinction works by splitting the meaningful sentences into those that are confirmable by scientific methods, and so regard facts and the way the world is, and those that have a kind of emotional pull on us, and so do not regard facts but may nonetheless be quite important to our way of life.[46] With this distinction in hand, Carnap can say that metaphysics is factually meaningless, as the Circle always wanted to, but he need not thereby say that it does not express anything at all. Metaphysical statements only fail to express facts, but they can still convey a kind of expressive meaning. Another way of putting the distinction that Carnap sometimes uses is to say that while metaphysical statements do not express facts, they still indicate an attitude towards life.[47]

The second major change from the old Circle position that we see in (1) is the move from verifiability to confirmability. This change stems directly from the difficulties with verifiability that the Circle faced from the moment they adopted it. The old notion of verification was all or nothing, that is, either a proposition is definitively verified (or is definitively verifiable) or it is not. Confirmation, by contrast, comes in degrees. So, while we still have a cleavage between those propositions which are confirmable and those that are not, the ones that can be confirmed to different degrees by the evidence at hand, or by the testing that one does. This change rather elegantly gets around the problem of verification of scientific laws by allowing that propositions in our theory need not be totally verified, but rather confirmed to a certain degree. Precisely what degree the confirmation needs to reach before the proposition is incorporated into the theory, Carnap says, is a matter of social agreement within particular scientific communities. Whatever the benefits of it might be, this change from verification to confirmation was not universally embraced within the Circle. As was often the case in their debates at the time, the divide broke down to the more liberally minded left wing of the Circle favoring the change (in one form or another), and the right wing arguing for maintaining the requirement of verification.

For our purposes, the second of Carnap's changes in his view of the nature of protocol sentences is more significant. To recap slightly, in the original Circle position, the primitive sentences of the theory must be sense data reports, or

[45][Carnap, 1963], p 58. This essay is also noteworthy because it is Carnap's first publication in English.

[46]See [Carnap, 1959a], pp 80 – 81 for Carnap's own explanation of this point. Some caution is warranted, however, as this explanation was written much later than the original paper.

[47]See [Carnap, 1959a], section 7.

as they put things in 1929 they must "refer directly to the given". As we noted above, these were first personal but should be nonetheless intersubjectively available.[48] Carnap's breaks with this view came in fast succession during the years he was in Prague. They started when he abandoned the thought that there was a unique answer to what form protocol sentences should take. For example, in OM he talks about settling the question of the form of the protocol sentences definitively (if only to say that it has not yet been done). However, in his pamphlet *The Unity of Science*, published in the very same 1932 edition of *Errkentnis* as OM, he gives three different options and indicates that there is not much to choose between them.[49] But by far the biggest break came when he abandoned the idea that there is any particular kind of content that must be in the protocol sentences. Instead of holding that they must be primitive sense data reports of some kind, as all the others in the Circle did, Carnap thought that one could take *any sentences one liked* as primitive. The only constraint that he thought could be applied to this choice was pragmatic utility. For now, we'll refer to this thesis by its traditional name 'conventionalism', though we also acknowledge that since many quite different views in philosophy go by that name it is somewhat unhelpful. As we will see below in section 4.2, Carnap has a long history with this train of thought, though we leave that history aside for now. Instead, we will examine why he re-adopted this radical idea.

For Carnap, the discussions in the protocol sentence debate were always about languages to be constructed for use in reconstructing scientific theories to ensure they are free of metaphysics. Natural languages, on his view, were encumbered with imprecise meanings and difficult-to-formulate syntactical rules, and so it seemed more promising to simply start over. This stands in opposition to those like Neurath who thought that we are always constrained by the particular natural languages we have at a given time, and therefore must attempt a program of disambiguating the meanings of words in those languages; as he put it, "no *tabula rasa* exists. We are like sailors who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials".[50] So the task as Neurath saw things was to discern what changes we should make to these languages in order to resolve the problems that, in the Circle's view, had impeded the progress of science and lead to metaphysical confusions.[51] Moreover, he thought that there was no guarantee that decreasing imprecision in one area of a language will not increase it elsewhere in that language. Carnap, however, was much more optimistic. On

[48] Neurath was particularly adamant that protocol sentences must be both intersubjective and direct reports of an individual's experience. In his essay "Protocol Sentences", he goes so far as to suggest that their proper form is as follows: "Otto's protocol at 3:17 o'clock: [At 3:16 o'clock Otto said to himself: (at 3:15 o'clock there was a table in the room perceived by Otto)]" ([Neurath, 1959], p 202). Sentences of this type were to be constructed purely from factual statements, i.e. those which refer to sensory experience in a verifiable way, and were to be actually said aloud, even in cases where the speaker was alone, to allow for the discussion of dreams and hallucinations. Moreover, the references to the speaker were absolutely essential to the correct account of protocol sentences on Neurath's view.

[49] [Carnap, 1934], pp 24 – 52, in particular p 50.

[50] [Neurath, 1959], p 201.

[51] [Neurath, 1959], p 201.

his view it is possible to construct a new language, and when doing so we are not bound by any existing structure and thus are free to arrange things with that language as we see best:

> *A question of the second kind* concerns a language-system L which is being proposed for construction. In this case the rules of L are not given, and the problem is how to choose them. We may construct L in whatever way we wish. There is no question of right or wrong, but only a practical question of convenience or inconvenience of a system form, i.e. of its suitability for certain purposes.[52]

This freedom extends quite far. We are not only free to choose which concepts and kinds of sentences will be taken as primitive, but also the rules of our language. This is quite a radical position, and an enormous departure from the earlier position of the Circle. On this view, as in OPS before, we need not take as primitive sentences those that refer to sense experiences, or even possible sense experiences. We can, for example, take sentences that express scientific laws as primitive. Moreover, there can be no standard relative to which the choices we make for the construction of our language can be judged. The only consideration can be a kind of pragmatic utility for a purpose. But even when evaluating candidate languages for their practical utility there does not need to be a determinate best choice on Carnap's view. Which concepts we take as primitive, and what rules we give ourselves in our language-system must be balanced against our other theoretical commitments. So, for example, if we prefer to have a phenomenalistic language, then our primitives will have references to 'the given'. If, on the other hand, we prefer to have a physicalistic language, then our primitives will instead have references to things themselves. Even if it were to be the case that one of these ways of setting up a language made expressing our scientific theories easier, but we preferred the other formulation for some reasons, then it would still be acceptable on Carnap's view to pick the formulation that we prefer.

On the face of it, Carnap's conventionalism might look to be a retreat from the view he put forward in OPS. Where before there was no definite end to confirmation, now there is, even if it can only be regarded as an end relative to the structure of a particular language-system. In a similar vein, it may look as if he also pulled back from the claim made in OPS that under the right circumstances any sentence can play the role of a protocol sentence. In the last part of this section, I will argue that while the view in TM is indeed less radical than the one in OPS, it is not best understood as a retreat, and especially not as a retreat to a more conservative position. As noted above, OPS was published in the 1932/33 edition of *Errkentnis.* Following closely on its heels, Carnap published *LSL* in 1934. There, in section V, and in particular in §82, we already see a view that resembles TM more than OPS. So, we know that Carnap

[52][Carnap, 1953b], p 74. Original emphasis. This quotation is quite close to an expression of what Carnap calls the 'Principle of Tolerance' in *LSL*, about which much more will be said in section 4.2 below.

changed his view almost as soon as he had published it. [53] In order to settle the question of whether the new view is a retreat from the old, it will be instructive to present the reasons for his change of heart. I believe that the crux of the issue is to do with what Popper calls the 'Demarcation Problem'. Put briefly, the problem is to give a criterion that separates science from metaphysics, and allows us to decide, in the case of sentences, which are properly scientific and which are metaphysical.[54] In OM, Carnap argues that the criterion is a logical one. He says that in order to be meaningful, a sentence must not only respect the rules of formation for a language (those rules which tell us which strings constitute sentences), but they must also "be capable of entering into relations of deducibility with (true or false) empirical statements".[55] He gives several examples, but one will suffice to make the point clear. First, harkening back to his study of Frege, he considers the sentence "Caesar is a prime number". This sentence and sentences like it, he says, violate the rules of formation because the word 'Caesar' is a thing word, not a number word.[56] So, even though this might at first appear to be a perfectly good sentence, it is not. These sentences he calls 'pseudo-sentences' because they appear to be legitimate expressions of a language but, in fact, are not. After making this distinction in OM, Carnap then moves on to attempt to show that all metaphysical statements can be detected by examining the rules of the language, and further that those that fail to meet these rules can then be removed, leaving a language that is free of metaphysical confusion. In other words, on the view that he is presenting in OM, the border between science and metaphysics is secured by the rules of our language. A very similar project is found at the conclusion of OPS. There Carnap says:

> These investigations in the logic of science do not end with the elimination of absolutism, for we have shown only that it must be purified in one definite but decisive point. The elimination of impurities is important, even indispensable, but it only forms the negative side of the task. Now that we are working in a more positive and unified way, the philosophy of science will be developed even further.[57]

Again, we see the emphasis on the removal of impurities from the language of science, as we did in OM. However, in the 'radical view', no reference is made to the procedure by which we detect these impurities, though perhaps there is a hint in Carnap's comments that proposed languages must be carried through consistently, and that, while any sentence whatsoever can be taken as a protocol sentence, this last is only true 'under appropriate circumstances'. I contend that

[53] In fact, the pace that the members of the Circle were developing their views was so much faster than the pace that they could publish them that it is not uncommon to see more than one paper by a Circle member on the same philosophical problem wherein they advocate different positions within the same edition of *Errkentnis*. It is possible, then, that Carnap had already abandoned the OPS view, to the extent that he did, before it ever appeared in print.

[54] See [Popper, 1959], pp 34 – 39. For Popper, demarcation was never about meaning, though for Carnap it was, at least at this stage.

[55] [Carnap, 1959a], p 72.

[56] [Carnap, 1959a], p 75.

[57] [Carnap, 1987], p 470.

the view we see in TM, then, is just the spelling out of what 'carried through consistently' and 'under appropriate circumstances' mean. That is to say that many of those portions of Carnap's conventionalist view that appear to be retreats from the radical view are in fact elaborations of it. There is, however, one area in which Carnap has significantly moderated his stance.

In his earlier paper OPS Carnap endorsed the view that there is no determinate fixed form of protocol sentences in a given language. At the stage of language construction one can specify a form for them. However, if at some later time the form initially specified becomes inconvenient, then it can be modified on the fly, so to speak. As Carnap puts the point,

> As soon as one wants – should doubt appear or if one wishes to lay a more secure foundation for scientific theses – one can take the sentences previously interpreted as endpoints and reduce them in turn to other sentences which are interpreted as endpoints by decree. In no case, however, is one forced to stop at any specified place. From any sentence one can reduce still further; there are no absolute initial sentences for the structure of science.[58]

However, by the time of both *LSL* and TM, Carnap has backed off from this stance. He still holds that there is no *metaphysically* specified form for protocol sentences – no question of correctness for their form can be entertained – but, relative to a language there is a particular form they must have, namely the one laid out in the rules for that language. In section V of *LSL* he says,

> 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.
>
> [...] If a sentence which is an L-consequence of certain P-primitive sentences contradicts a sentence which has been stated as a protocol-sentence, then some change must be made in the system. For instance, the P-rules can be altered in such a way that those particular primitive sentences are no longer valid; or the protocol-sentence can be taken as being non-valid; or again the L-rules which have been used in the deduction can also be changed. There are no established rules for the kind of change which must be made.[59]

In this new position one is still free to construct one's language as one sees fit, and if the results of adopting it turn out to be inconvenient, then changes can be made as before. However, notably, one cannot simply carry on reducing sentences until one is satisfied as was permitted in OPS. There is now a determinate form to the protocol sentences, though it is fixed only by the rules of a given language. This, then, is the sense in which a retreat has been made from OPS.

Before moving on to the next section, it is worth pausing to take stock of what has been shown so far. Carnap began his time in Vienna right as the

[58] [Carnap, 1987], pp 465 – 466.
[59] [Carnap, 1937], p 317. Original emphasis.

Vienna Circle's fascination with Wittgenstein's thought, and the *Tractatus* in particular, was at its zenith. Under the influence of this book as well as personal contact with Wittgenstein, he adopted a view whereby the meaningfulness of statements was guaranteed by their verifiability. This view landed the Circle, and Carnap as well, in a difficult position: their original aim was to defend science and what they called the 'scientific worldview' from metaphysics, but the tools they developed in order to make this defense, namely the various criteria of meaningfulness, were so extreme that they also made scientific practice meaningless. After years of wrangling and argumentation, both in the Circle's meetings as well as in publications in their journal *Errkentnis*, that resulted in a split in the Circle into left and right wings, Carnap came to a position where the meaningfulness of terms flows from either their empirical confirmability or from the rules of the constructed language-system, rules which in turn can be freely chosen. These changes kept Carnap's theory more in line with scientific practice, and also saved it from a debate on the nature of protocol sentences that, in his mind, had begun to look like the old metaphysical debates the Circle disparaged. However, the theory was not embraced by his colleagues in the Circle; those on the right wing preferred to stick with the requirement of verifiability, while those on the left thought that in allowing any sentence or concept whatsoever to be a taken as primitive Carnap had substantially weakened his commitment to empiricism.[60] At the same time as his views on verifiability and metaphysics were developing in the ways that we have been examining, Carnap also underwent a substantial change in his views on logic and its relationship with mathematics. We take up these changes in the next section.

3.1.3 The *Tractatus* and Logic

The second part of Wittgenstein's thought that played a major role in Carnap's intellectual development is his view of logic. Like Frege and Russell before him, Wittgenstein thought that logic is universal. However, unlike his predecessors Wittgenstein's universalism was not based in the thought that the laws of logic were laws of everything, so to speak. As we will see, the brand of universalism that Wittgenstein proposed was a product of his views on the role of language in representing the world to us. This linguistic conception of logic made a huge impression on Carnap, and its adoption is perhaps the single most important event in his development in the late 1920s and early 1930s. However, as before with verificationism and the rejection of metaphysics, the period during which Carnap actually holds a Wittgenstinnian view is rather brief. In this section, we begin with a short description of the Circle's struggles with the foundations of mathematics in the late 1920's and early 1930's. We will then move to describing the view of logic that Wittgenstein presents in the *Tractatus*, where we will focus on two parts of the view in particular: the tautological nature of logical

[60]This assessment of the view is due to Karl Popper in his [Popper, 1959], p 97. In the cited passage he is addressing Neurath, but the same point holds against Carnap, at least at this stage. Oddly, Neurath thought that this very same argument held against Popper and Carnap, and at one time referred to the view that any sentence could be a protocol sentence as the "Popper-Carnap" view.

statements, and the status of the rules of logic. We will then detail Carnap's struggles with Wittgenstein's view. A discussion of Carnap's eventual break with Wittgenstein's position and the development of his own view, however, will have to wait until section 4.2 below.

An issue that was of deep concern to the Circle was to provide an account of mathematics that was in harmony with their empiricism. In the *IA* Carnap says that their mission, as they saw it, was to,

> [...] combine the basic tenet of empiricism [that all substantial knowledge is grounded in experience] with a satisfactory explanation of the nature of logic and mathematics.[61]

In order to be satisfactory, such an account must do two things: (1) it must be consistent with the view that all substantial knowledge is based in empirical sources (for example, though by no means limited to, sense perception), where 'substantial' means something akin to synthetic, and (2) it must allow for at least as much mathematics as is used in the sciences. By the Circle's lights, the history of attempts to give empirically satisfying accounts of mathematics was not promising. In his *A System of Logic*, John Stuart Mill had tried to give an account satisfying at least the first of these requirements. However, this effort came to a notorious, and notoriously sarcastic, end at Frege's hands.[62] But, Frege's own attempt to provide a foundation for mathematics was also defective, as we saw above, and even if it were not it would still have been problematic for the Circle due to its rather Kantian grounding in pure reason.[63] By the time the Circle was actively discussing the issue, the standard account of the foundations of mathematics was that given in Russell and Whitehead's *PM*. As we saw, it was taken by its authors, as well as several members of the Circle, to have shown that mathematics was reducible to logic. But, the Circle had worries about some of the axioms used in *PM*, in particular those of infinity, reducibility, and choice, none of which which were obviously purely logical. That is, it seemed that these axioms made substantial claims about the world that were not based in a source acceptable to empiricists. For example, the axiom of infinity stipulates that there are at least infinitely many things, but is not based in some experiment, or even in a possible one. If these axioms were not logical, then it seemed that *PM* was open to a variant of the kind of attack that was leveled against Mill. This leaves two options: either one must show that the axioms are indeed logical, or find another account of logic. In the *Tractatus*, the Circle thought they found a route to the second option.

To understand how Wittgenstein's view of logic in the *Tractatus* helps the Circle out of their problem, we must first say a bit about his understanding of language, though with some overlap with what was said in the previous section. The fundamental role of language on his account is to represent the world to us,

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[61] [Carnap, 1963], p 47.

[62] Mill's logic is found in [Mill, 1973]. See [Frege, 1980], p 15 for an example of Frege's assault on Mill's work. An attempt to defend Mill by appealing to the Calculus of Individuals is made by Kessler in his [Kessler, 1980].

[63] See [Carnap, 1963], p 47 for Carnap's comments on the flaws of both of these approaches from the Circle's perspective.

which it does by picturing atomic facts in atomic sentences. Complex sentences can be built out of atomic ones quite simply: they are just truth-functions. In other words, atomic sentences are stitched together using the logical vocabulary "and", "or", "not", etc. Sentences are true or false in virtue of their relationship with reality, and are discovered to be so by comparing the state of affairs they picture with reality; those sentences which are verified by reality are true while those which are not are false. But there are certain sentences which come out true (or false) no matter which way the world is; they are true in every possible way the world could be. On Wittgenstein's view these statements are called tautologies, and the truths of logic are of this kind.[64] Their truth is due to the structure of language itself. However, as we noted above, by the time that Wittgenstein was in conversation with Carnap and other members of the Circle, he was advancing the view that we earlier called the Verifiability Principle: what a statement says is its method of verification. Since the statements of logic have no method of verification they therefore say nothing at all.[65] Wittgenstein further distinguished between those sentences which were nonsense and those which simply lacked a sense. A rough gloss on the distinction is that while the sentences which lack a sense are at least well-formed, the nonsensical ones are neither well-formed nor verifiable. As we will see, this was of critical importance to the Circle, and is largely responsible for their enthusiasm for his views.

The resolution of the Circle's worry worked like this. If one accepts that all substantial knowledge is based empirically while still wanting to preserve the possibility of logical and mathematical knowledge, then one must find a way to say that logical knowledge is not substantial. Accepting the view that logical statements are tautological and so do not have a sense, but are nonetheless not nonsensical, does precisely this. Since logical statements say nothing at all, they certainly say nothing substantial. So understanding logical statements in this way gets the Circle out of the trouble that befell Mill's account. But there is an important caveat. This solution only works on the assumption that one can reduce mathematics to logic, in one way or another. The Circle in general, and Carnap in particular, were already inclined to think along these lines, but Wittgenstein was not. For him, it was true that mathematics was a "logical method", which is to say that we *use* logic in our inferences in mathematics, but mathematics is nonetheless distinct from logic.[66] This is obviously a far cry from Carnap and the Circle's logicism. However, this disagreement did not bother Carnap at all, and he saw no contradiction in taking Wittgenstein's view together with the results from *PM* that showed how to reduce mathematics to logic.[67] There was, however, another problem for the Circle lurking in Wittgenstein's view.

As was noted in the previous section, a part of the *Tractatus* with which the Circle lived only uneasily was its apparently mystical conclusion, namely that once its readers had fully grasped its meaning, then they would see the propositions contained in the book as meaningless, strictly speaking, and in

[64][Wittgenstein, 1983], proposition 6.1.
[65][Wittgenstein, 1983], propositions 6.11 and 6.111.
[66][Wittgenstein, 1983], proposition 6.2.
[67][Carnap, 1963], p 47.

his memorable phrase would "throw away the ladder, after [they have climbed] up on it".[68] The reason the propositions in the book were meaningless, on Wittgenstein's account, is that they purported to say things that could only be shown, in particular they seemed to say things about the nature of logic, and of language, directly. However, in proposition 5.61 Wittgenstein asserts that language cannot *say* things about its nature, only *show* them. So, for example, there can be no discussion of the rules of logic, as they are not the kind of thing that can be said. That there are rules is only evidenced by the actual use of language, and through this use the rules can be exhibited. This presents a series of disastrous consequences for the Circles views. Firstly, it makes the statement of some of their most cherished results – for example that traditional metaphysics is meaningless, or that all substantial knowledge is based on empirical experience – impossible. Of course, one could still exhibit these facts by showing, in the case of the meaninglessness of metaphysical statements, that a particular metaphysical statement is not a truth-functional combination of atomic sentences. But this is not the sweeping dismissal that the Circle wanted, or indeed took themselves to have already given. Moreover, any attempt to articulate a criterion by which one could demarcate the meaningful from the meaningless would fall prey to the same problem, that is, it would itself be meaningless for the reason that it tried to say something that cannot be said. In a similar vein, the debate over the form that protocol sentences should take that was detailed in the previous section also turns out to be meaningless since there can only be one universal language with a definite form, a form that is read off from the structure of the atomic facts. Any debate about this structure would be an attempt to say the unsayable. Making a similar point in their [Awodey and Carus, 2007], Awodey and Carus term this predicament "Wittgenstein's Prison":

> Wittgenstein had recognized that [the laws of logic] were laws of language. He had been the first to consider the entirety of language as nothing but a system of rules. But he had arrived at this idea via a theory of representation that forced language to consist always and everywhere in a *particular system* of rules, arising necessarily from the representational function of language – the picture theory. The possibility of representation determined a particular form of linguistic intuition, so to speak. This elementary logic built into our form of representation was, like a Kantian form of intuition, an inescapable straight-jacket. The very nature of language, in Wittgenstein's view (at least as seen by the Vienna Circle), prevented us from stepping outside it. One could call this quasi-Kantian view "Wittgenstein's Prison".[69]

After pointing to problems similar to these just above, they go on to tell the story of Carnap's escape from this prison through the lens of the work he did on axiomatic theories in the late 1920s and early 1930s, with particular attention

[68] [Wittgenstein, 1983], proposition 6.54.
[69] [Awodey and Carus, 2007], p 181.

paid to the unpublished manuscript *Untersuchungen zur allgemeinen Axiommatik* (Investigations into General Axiomatics).[70] For everything I want to say here, this account of Carnap's transition away from Wittgenstein's philosophy of logic is essentially right, though there are some rough edges.[71] I do, however, think there is one aspect of this move to which they have given somewhat short shrift, namely the interaction between Carnap's understanding of logic and the effect it had on his views in the debates that the Circle had over the language of science. We turn to that issue, and to the position that Carnap finally takes up on the nature of logic and its relationship to mathematics, in chapter 4 below.

3.2 Conclusion

In this chapter we have traced Carnap's development from the time he came to Vienna in 1926, until around 1930. We showed the two ways in which his thinking was shaped by Wittgenstein, through reading his *Tractatus* and through personal conversations. Moreover, we examined the problems the adoption of Wittgenstein's views caused for the Circle in general, and for Carnap in particular. These began with the use of verifiability as a criterion for meaning, which we saw was eventually relaxed to confirmability by a portion of the Circle we called the left wing which included Carnap and Neurath amongst others. We also examined Wittgenstein's view of logic, which, as was noted above, was highly influential for the Circle. We argued that Carnap's view of logic around 1930 was heavily influenced by Wittgenstein in two specific ways: (1) in holding that the truths of logic are true in virtue of being tautological, and (2) that logic is universal. Combining these two tenets with the logicist thesis that mathematics can be reduced to logic yields the optimistic view that the Circle held in the late 1920s, namely that all substantial knowledge is empirical in nature, but that logical knowledge is still possible because it says nothing substantial. However, this view was not a panacea and its adoption led to a series of unpalatable consequences, as we saw. Especially problematic was the Circle's adoption of Wittgenstein's universalism about language, whereby any attempt to say meaningful things about the structure of language is impossible. It was therefore impossible, strictly speaking, to even articulate the view that the Circle held. This predicament we called "Wittgenstein's Prison" in section 3.1.3 above. What we have not yet examined is Carnap's escape from this prison. This is the subject of the next chapter.

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[70]This has been posthumously published as [Carnap, 2000]. Unfortunately, as yet there is no English language edition available.

[71]I think they breeze too quickly over certain aspects of Carnap's engagement with metamathematical ideas from the Hilbert school, as well as those suggested by Russell's introduction to the *Tractatus*. Additionally, their characterization of Wittgenstein's thought is somewhat unfortunate. While it is, of course, true that he was one of the first to emphasize the linguistic nature of logic, it is at best contentious whether he thought of logic as a "system of rules". It seems to me that a better interpretation of the *Tractatus* is to take proposition 5.132 seriously, where Wittgenstein says, "The method of inference is to be understood from the two propositions alone. Only they can justify the inference". Which inferences are the correct ones is *shown* by the truth grounds of the relevant propositions, not encoded somehow by rules.

Chapter 4

Logic, Science, and Tolerance

We have seen so far that Carnap was in contact with several of the major thinkers and schools of thought in philosophy of mathematics and logic of the day. To review briefly, he was Frege's pupil, a correspondent of Russell, and personally acquainted with Wittgenstein. However, so far we have only asserted that his own position on the nature of logic and mathematics was distinct from any one of the others on offer. Though he identified as a logicist, he was not committed in the same ways that Frege and Russell were. While he thought that debates on the status of mathematics were really debates about language, he was not bound by the universalist conception of language that Wittgenstein was. As we will see in this chapter, though he was interested in investigating mathematics from the metamathematical standpoint that Hilbert and the formalists took, he was nonetheless not committed to the foundationalist project that the formalists were engaged in. In this chapter, we will finally examine Carnap's own position in detail with the goal of showing which parts of it are uniquely his own, as opposed to adopted or adapted from the views of others. In particular, we will focus on the 'Principle of Tolerance', and his book *The Logical Syntax of Language*.[1]

The chapter will be split into two sections. In the first, we will lay out certain aspects of Carnap's upbringing that will, I argue below, form the basis of the Principle later in his life. The main focus of the second section is to give a thorough examination of Carnap's account of logic and mathematics as it is presented in *LSL*. On the way, we present the impact that the incompleteness theorems' discovery by Kurt Gödel had in breaking Carnap out of "Wittgenstein's Prison". We will also examine the changes Carnap makes in response to incompleteness in his account of protocol sentences. As I will argue, these changes are the first published manifestation of Carnap's new 'tolerant' views. We will finally give a brief examination of Karl Menger's claim to priority on the Principle, and to what extent, if at all, this claim can be said to be true.

[1] I will refer to this principle variously as the 'Principle of Tolerance', 'Tolerance', and 'the Principle'.

4.1 Carnap's Early Years

Though it is first articulated in *LSL*, it will be the purpose of this section to argue that, at least in some sense, the Principle of Tolerance was a feature of Carnap's thought from an early age, and that he acted on the basis of something akin to it during his years as a student, even if he had not yet articulated it as an explicit principle. In what follows, we will give a brief account of his early intellectual development up through his years as an undergraduate student. First, we will argue that through his early childhood, Carnap's mother modeled a tolerant attitude towards other ways of life from their own, and that this model partly conditioned Carnap's own thinking. Following on, we see how this conditioning played out in his years spent as a student, and in his interactions with other students.

Carnap's mother, Anna Carnap (neé Dörpfeld), exerted a very strong influence on his life and upbringing. In the *IA* Carnap comments that she home-schooled both Carnap and his sister.[2] Additionally, he says that his mother was quite religious and that she attempted to pass that on to her children. But, the particular fashion in which her religiousness manifested itself was not concerned with the details of some doctrine or other, but rather with the thought that "[...] the essential in religion was not so much the acceptance of a creed, but the living of the good life; the convictions of another were morally neutral, as long as he sought seriously for the truth. This attitude made her very tolerant toward people with other beliefs".[3] In his [Carus, 2007], André Carus fills in further details of Carnap's upbringing, supporting the thought that he was raised by his mother in a manner that lent itself to the free exploration of ideas. In particular Carus reports that she stressed to her children the mere conventionality of language. As he puts the point:

> 'She took the same attitude' Carnap adds, 'toward the conventions of language. When I said "er esst" in analogy to "ich esse" [...] she told me, of course, that one says "er isst".' But when asked for the reason, she said there was none; 'it just happened to be the general custom' [...]."[4]

That is, on her view, the reason that some forms of natural languages are acceptable and others are not is purely a matter of social conventions.[5] This view left a deep impression on Carnap. Again, as Carus reports, even much later in his life Carnap was still guided by this way of thinking: "Because of this casual attitude of hers [that is, Carnap's mother] towards customs, conventions, and traditions, I never had the widespread reverence toward the sanctity of

[2]This fact is particularly interesting when combined with the fact that Anna's father, Friedrich Wilhelm Dörpfeld, was a famous educational reformer in 19^{th} century Germany. See [Carus, 2007], chapter 1 for a discussion of this potential influence.

[3][Carnap, 1963], p 3.

[4][Carus, 2007], p 45. This quotation is from the draft version of Carnap's intellectual autobiography. It differs substantially from the published version, and in particular these reminiscences about Carnap's early life with his mother are absent from the version that appeared in print.

[5][Carus, 2007], pp 44 – 45.

traditions, which is such an obstacle in the way of cultural progress".[6] While in this particular passage Carnap is talking about issues that concerned him later on in his philosophical career (i.e. 'language planning'), what is important to the present investigation is the explicit connection that he draws between the attitude he inherited from his mother and his willingness to investigate alternative ways of thinking.

During his time as a student at the University of Jena, and in the years following, Carnap was in contact with a wide network of philosophically-minded young people, largely due to his participation in the German Youth Movement. He would exchange and circulate letters from them to his friends, even during his time in the army in World War I. The willingness to investigate alternative ways of thinking that his mother instilled in him stayed with Carnap, and he says in his intellectual autobiography that during this time he would change his way of speaking to match the philosophical outlook of his interlocutors.[7] As he puts it,

> Since my student years, I have liked to talk with friends about general problems in science and in practical life, and these discussions often led to philosophical questions. [...] Only much later, when I was working on the *Logischer Aufbau*, did I become aware that in talks with my various friends I had used different philosophical languages, adapting myself to their ways of thinking and speaking. With one friend I might talk in a language that could be characterized as realistic or even as materialistic [...] with another friend, I might adapt myself to his idealistic kind of language [...]. With some I talked a language which might be labelled nominalistic, with others again Frege's language of abstract entities of various types [...], a language which some contemporary authors call Platonic. [...] Only gradually, in the course of years, did I recognize clearly that my way of thinking was neutral with respect to the traditional controversies [...].[8]

This is all to say that he did not feel any particular pressure to adopt one way of speaking or thinking about an issue because it was supposed to be the *right* way. The only constraints that Carnap thought were relevant at this early stage were making himself understood by the other participants in the conversation. By analogy to the case of proper conjugations in German mentioned above, it seems that Carnap saw no standards of correctness to which his philosophical language should be bound, he only needed respect the preferences of those

[6][Carus, 2007], p 45.

[7][Carnap, 1963], p 17.

[8][Carnap, 1963], pp 17 – 18. It is worth noting that this view commits Carnap to the existence of a translation (and potentially one that operates purely syntactically yet nonetheless respects meaning) from any language to any other language. This existence claim is highly non-obvious, even where one understands 'meaning' in the way that Carnap does in *LSL*, that is, as preservation of the class of all non-analytic consequences. There are a number of nuances to be spelled out here (does the translation have to be purely syntactic? What is the status of Carnap's purported proof of this in *LSL* (§61)?) but we leave them aside as they are somewhat distant from our focus in this thesis.

with whom he talked. So, we see here that from his childhood, through his years as a student, up through his working on the *Aufbau*, by his own account Carnap adhered to something like the Principle, though it was not yet explicitly articulated as such. This attitude, which places the ability to communicate at the forefront and diagnoses many traditional philosophical problems as failures of communication, I call the "tolerant attitude". I will argue below that the Principle of Tolerance is a natural way of making precise the tolerant attitude that, as we have seen, characterizes Carnap's view from an early age. However, what remains to be shown is the way in which this attitude becomes the famous articulated principle.

4.2 The Principle of Tolerance

Before examining the way in which Carnap arrived at the principle that is codified in *LSL* as "The Principle of Tolerance", let us first review what we have seen so far. Carnap learned his logicism directly from Frege, though as we noted in chapter 2 not through Frege's lectures but via later study of his publications, and, somewhat less directly but no less influentially, from Russell. In addition to his logicism, and under the additional influence of Wittgenstein, Carnap came to hold the view that logic was both linguistic and universal. That is, that logic arises of necessity from the structure of our capacity to represent the world through language. This is the position that, following Awodey and Carus' memorable phrase, we called "Wittgenstein's prison" in section 3.1 above, and that Carnap adhered to around the beginning of 1930. In addition, Carnap and the rest of the Circle followed Wittgenstein in holding to a kind of verificationism wherein the only meaningful statements were those that could be shown to be completely verified by sensory experiences. This caused some serious difficulties for the Circle's view that the paradigm of meaningful work should be the natural sciences, as the statements found in most scientific theories were not of the right, completely verifiable, kind. The year 1930 would prove to be decisive for Carnap in another way, however, and his position would be forever changed by an event in the summer of that year. This was, unsurprisingly, Gödel's discovery of the incompleteness of certain kinds of formal theories.[9] He told Carnap of his result on the 26^{th} and the 29^{th} of August during a pair of meetings they had at the Café Reichsrat in Vienna.[10] It took Carnap some time to digest the result, a fact that has been much remarked in some literature.[11] To illustrate, we consider the conference that took place at Königsburg from the 5^{th} to the 7^{th} of September, 1930.

The aim of the conference was to present several views on the foundations of mathematics. To this end, Carnap, Arend Heyting, and John von Neumann were all invited to give talks on varying approaches to the foundations,

[9] Of course, we now know that every recursively axiomatizable theory with the resources to represent every primitive recursive function will be incomplete as well, but this fact eluded Gödel until sometime in early 1931.

[10] See [Goldfarb, 2003], p 335, [Dawson Jr., 1984], p 115, and [Dawson Jr., 1997], p 68.

[11] See Dawson's "Translator's Introduction" in his [Dawson Jr., 1984] for one particularly strident example.

to be delivered on the first day of the conference. Carnap's talk, entitled "Die Grundgedanken des Logizismus" ("The Logicist Foundations of Mathematics"), focused on setting out both the advantages of logicism, as well as some discussion of the difficulties involved.[12] The second day of the meeting featured short talks by more junior academics. Gödel gave a talk in this second session on the completeness proof that had been the goal of his PhD dissertation. However, what draws our attention is not the subject matter of these talks, or even the content of their remarks in the roundtable conversation that occurred on the final day of the conference. Rather it is the language that Carnap is still using to describe the project of providing a foundation for mathematics. In his talk, after giving a very brief overview of higher-order classical logic with identity, as was standard at the time, Carnap says,

> It is the logicist thesis, then, that the logical concepts just given suffice to define all mathematical concepts, that over and above them no specifically mathematical concepts are required for the construction of mathematics.[13]

We can see from this comment that Carnap still thinks, in the manner of Frege, Russell, Whitehead, and Wittgenstein, that it will be possible to give a characterization of all of mathematics in a *single* language. The point is made somewhat more explicitly in the subsequent discussion session, where Carnap comments,

> The differences among the schools may perhaps be explained by the differences in the demands that are placed on the structure of mathematics by the different points of view. The *logician* (represented first by Frege, later by Russell, and in certain respects also by Brouwer) demands: "Every sign of the language, and hence also of the mathematical symbolism, must possess a definite specifiable meaning". [...] They [physicists] require of the logico-mathematical system that it not only be self-consistent, but that it also be applicable in the realm of empirical science.[14]

In each case, Carnap uses the singular article to refer to *the* structure of mathematics. This, taken on its own, is not conclusive evidence that Carnap still sought a universalist solution, that is, one where a single language serves for the construction of the totality of logic and mathematics. However, read in light of Menger's comments on the persistence of the single-language approach in the Circle's thinking, to be discussed in detail in section 4.2.2 below, it does seem that this usage is indicative of his continued universalism. Moreover, Carnap reiterates his view that a consistency proof for a theory in the language of mathematics suffices for its correctness in his remarks during the final discussion period. Of course, this is forgivable at this stage. After all, Carnap had

[12]The translation of the title of Carnap's talk, due to Erna Putnam and Gerald Massey (see [Carnap, 1959b], p 41), is somewhat problematic. Perhaps a better rendering of 'Grundgedanken' might be 'basic ideas'.

[13][Carnap, 1959b], p 42.

[14][Dawson Jr., 1984], pp 120 – 121. Original emphasis.

only learned of the results ten days before the start of the conference, and the second incompleteness theorem had not yet been proven. However, after it had, and as he came to realize what the results meant, his view of both logic and language in science changed radically, as we shall see below.

Interlude: Incompleteness

Gödel managed to prove the first of his two incompleteness theorems in the summer of 1930, and announced it at the Königsburg meeting in an understated fashion that was typical of his personality.[15] As was noted above, his formal presentation at the conference was confined to the completeness result that he had achieved in his PhD thesis, and it was not until the roundtable discussion the next day that he revealed his new result. In his second interjection into the conversation he says,

> One can (assuming the consistency of classical mathematics) even give examples of propositions (and, indeed of such of the type of Goldbach or Fermat) which are really contentually true but unprovable in the formal system of classical mathematics. Therefore if one adjoins the negation of such a proposition to the axioms of classical mathematics, one obtains a consistent system in which a contentually false proposition is provable.[16]

So, we can see that now mere consistency will not be enough for showing that a single theory captures mathematics, and on that basis one of Carnap's claims from his talk is proved incorrect. However, as is now well known, Gödel did not yet have a proof of the second incompleteness theorem at the time of the Königsburg meeting. So, the single-language approach had not yet completely unraveled. We know that the second theorem came quickly after Gödel returned from Königsburg, and an abstract of the full incompleteness paper, containing both theorems, was presented to the Vienna Academy of Sciences on October 23^{rd} by Hans Hahn.[17] The paper was submitted to the *Monatshefte für Mathematik und Physik* for publication on November 17^{th}, though it did not appear in print until 1931.

From the perspective of our examination of Carnap's development, the second theorem is decisive. While the first shows that consistency is not enough to guarantee the correctness of a formal language for deriving all of mathematics, one might still have thought that some other notion could be developed to sidestep this problem. However, the second theorem shows that no formal theory will be provably adequate whatever the criterion, as long as that theory

[15] See [Dawson Jr., 1984], pp 114 – 115 and especially fn 25.

[16] [Dawson Jr., 1984], p 126.

[17] The date of this abstract is important for settling whether it was Gödel or von Neumann who proved the second theorem first. Von Neumann sent a letter to Gödel in November of 1930 saying that he had proved an extension of the results that Gödel had announced at Königsburg which amounts to the second theorem. However, Gödel had already seen it himself, as the presentation to the Academy, and the published abstract of that presentation shows. See [Dawson Jr., 1997], pp 69 – 70, and [Gödel, 1930].

has the resources for arithmetic and on the assumption that the theory is consistent and codifiable with recursive functions.[18] This is the final blow to the universalist view to which Carnap still adhered despite the difficulties that had been mounting, as we detailed in chapter 3. He simply cannot have a single language which is capable of serving for the derivation of all of mathematics.

As we commented above, Carnap's view began to change around the autumn of 1930, but it did not show until after he moved to Prague in the summer of 1931. The first published indications came in his 1932 paper "On Protocol Sentences". There Carnap advocates an extremely liberal view whereby one is free to alter one's language in any way one sees fit at any time.[19] This is a somewhat desperate attempt to save some portion of his universalism as, while this is still a single-language approach, it is one where the single language does not have a determinate form since it can be altered at any time. The speed with which he moved away from this desperate view, as discussed in section 3.1.2 above, illustrates the deep-seated nature of Carnap's tolerant attitude. At the same time that Carnap was advocating this extremely liberal view of protocol sentences, he was also wrestling with the constraints of Wittgenstein's prison in another way. Spurred in part by his engagement with the metamathematical techniques of the Hilbert school, which he encountered primarily in the logical investigations of Gödel and Tarski, and in part by his own research in logic, Carnap began to push against the universalism about logic that he had held at least since the early 1920s.[20] The story of the resulting break with his influences, as well as with his own earlier views, and the result of that break, namely the Principle of Tolerance, forms the subject of the remainder of this section.

The rest of this section will be split into three parts. In the first, we will examine the way in which Carnap's Tolerance evolved from his consideration of Gödel's incompleteness results. We will examine the similarities between his first published reactions, namely his papers in the protocol sentence debate, and his settled view in *LSL*. In the next section, we will address a challenge regarding the provenance of Tolerance that was made by Karl Menger, though we will ultimately conclude that it is unfounded. In the third part of this section we will present the view that Carnap formulates in *LSL*. Special attention will be paid to the two projects he takes up there: (1) giving a logical foundation for the language of science that is in line with the left wing of the Vienna Circle's

[18] In their [Awodey and Carus, 2003], Awodey and Carus suggest that one way out of this problem for Carnap is to give up the requirement of *provable* consistency. They are right and this is indeed a way he could have gone. However, what is interesting is that he did not, in fact, choose this route. It is possible that it did not occur to him. I think it more likely that the reason he did not simply opt away from proving consistency is revelatory of his stance on the foundations of mathematics, and I will say more about this in chapter 6 below.

[19] This view may, in fact, commit Carnap to already thinking that more than one language is necessary, namely an object language and a language from which to specify the rules of the object language (which Carnap later refers to as a syntax language (see, e.g. [Carnap, 1937], p 4.) Carnap is notoriously dismissive of this commitment, and often claims that natural languages can serve this purpose. We will return to this topic in chapter 6 below, and so leave it aside for now.

[20] For more on the way in which Carnap's own research pushed him towards Tolerance, see [Awodey and Carus, 2001], [Reck, 2004] and [Carnap, 2000]. Tarski's influence on Carnap is well known, see [Coffa, 1991], pp 300 – 305 for an exemplar.

"Unified Science" program, and (2) showing, contra Wittgenstein, that one can represent the syntax of a language in that very language itself. The third section will make plain the interactions between the position that Carnap took on the nature of protocol sentences and the position he takes on the language of mathematics. I will argue that the inspiration for this latter view comes from the success that he found with the former view, and that one of the main aims of *LSL* is to replicate that success, but with a focus on the debates over the foundations of mathematics. In brief, Carnap thought that a way to end certain disputes over the nature of the language of science that he thought were unproductive was to adopt a tolerant view whereby requirements are read as proposals which are then in turn investigated and compared on the basis of their respective pragmatic virtues. In the case of mathematical disputes, the procedure would be analogous: competing views on the nature of mathematics, or on which logical laws are valid, would be read as proposals for ways in which a calculus for doing mathematics could be constructed; as before these proposals are then investigated and compared by means of their pragmatic virtues.

4.2.1 The Birth of Tolerance

To briefly rehearse what was discussed in section 3.1.2 above, under the influence of Wittgenstein, the Vienna Circle advocated the view that the only meaningful sentences were those which could be empirically confirmed.[21] The process of confirmation consisted in deriving from the universally quantified laws of our scientific theory sentences of the appropriate empirically testable type called protocol sentences. A debate ensued within the Circle over precisely what form these protocol sentences should take. By 1932, however, Carnap had become convinced of what he sometimes called the "thesis of the conventionality of language forms" solved this problem.[22] According to this thesis, language is freely constructed and this construction is not antecedently constrained by anything; one is completely free with regard to the form of language that one constructs. So, as we showed above, in his 1932 essay OPS Carnap argues that,

> *Every concrete sentence of the physicalistic system language can serve under certain circumstances as a protocol sentence.* [. . .] Thereby it is a matter of decision which sentences one wants to use at various times as such endpoints of reduction and thus as protocol sentences. As soon as one wants – should doubt appear or if one wishes to lay a more secure foundation for scientific theses – one can take the sentences previously interpreted as endpoints and reduce them in turn to other sentences which are interpreted as endpoints by decree. *In no case, however, is one forced to stop at any*

[21] As was remarked in chapter 3 above, there was a further related divide in the Circle between those who stuck to the criterion of complete empirical verification, like Schlick and Waismann, and those who relaxed this criterion in various ways, as for example did Neurath and Carnap. The word this latter group preferred was 'confirmation' and we follow this usage in the present chapter.

[22] [Carnap, 1963], pp 54 – 55.

specified place.[23]

Again, this is to say that there are no constraints at all on the language of science. Moreover, if the choices one makes in originally constructing the language make it inconvenient later, one is free to change it at any time.[24] This view, however, never gained much traction with the other members of the Circle, and even Carnap was unable to maintain it for very long. By 1934, he had retreated to a somewhat more modest version. Discussing protocol sentences in *LSL*, he says,

> 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. [...] If a sentence which is an L-consequence of certain P-primitive sentences contradicts a sentence which has been stated as a protocol-sentence, then some change must be made in the system. For instance, the P-rules, can be altered in such a way that those particular primitive sentences are no longer valid; or the protocol-sentence can be taken as being non-valid; or again the L-rules which have been used in the deduction can also be changed. There are no established rules for the kind of change which must be made.[25]

Here, we can see that while Carnap has maintained a certain level of freedom with regards to what is to be done with protocol sentences, this freedom is not nearly as radical as his proposal in OPS above. Now, not just anything can serve as a protocol sentence *once a language has been established.* That is, at the stage of language construction we are free to specify any form we wish for the protocol sentences – they need not be "the contents of immediate experience, or the phenomena; and thus the simplest knowable facts," or indeed any of the particular proposals that the Circle had considered.[26] However, it is part of the specification of a language to lay down a specific form that the protocol sentences in that language will take. That is, they must have some form or other, once and for all, that is precisely laid out in the rules that govern one's language. To illustrate, let us consider some examples that Carnap gives in "TM":

> The fluid at space-time-point *b* has a temperature of 100°; [...] A mercury thermometer is put at *b*; we wait, while stirring the liquid, until the mercury comes to a standstill; [...]The head of the

[23][Carnap, 1987], pp 465 – 466. My emphasis.

[24]In the paper, Carnap credits this idea to Karl Popper who he says convinced him of it in conversation ([Carnap, 1987], p 465). This conversation is similarly reported in [Popper, 1963b], p 71, where Popper goes on to say that the conversation in question occurred on a holiday that he, Carnap, and Feigl took together in the Tyrolean Alps. However, what Carnap says here is *not* the view that Popper suggested to him in Tyrol, and indeed Popper later called a version of this view, put forward by Neurath, "throw[ing] empiricism overboard" ([Popper, 1959], p 97).

[25][Carnap, 1937], p 317. Original emphasis.

[26][Popper, 1959], p 96.

> mercury column in the thermometer at b stands at the mark 100 of the scale.[27]

What is critical here is that there is no antecedent preference given for which of these sentences exhibits the form that protocol sentences should take. Moreover, they are not even equivalent, as the logical forms will differ between them, as does the content contained in each of them. This is not a return to the old absolute criteria for languages, however. If the results of adopting a particular language are not to one's liking, it is still possible to make changes to one's language, or to construct a new one. Though this is akin to the radical view, where if one did not like the results of taking certain sentences to be protocol sentences it was possible to change the criteria for being such at any time, it is nonetheless importantly different. In the view presented in OPS there was no requirement that the protocol sentences in a language have any particular form. After all, one could change the form that particular protocol sentences took on the fly, so to speak, by a mere decision to continue reducing a sentence that one previously took to be a protocol sentence. By contrast, in *LSL* it is part of how languages are constructed that some form or other must be given to protocol sentences. There is still freedom to change the form, but it must be done by changing the rules that govern the protocol sentences, and cannot be as *ad hoc* as before; in this latter case, it is still a matter of decision whether to accept a given sentence as a protocol sentence, but if one decides to reject it, then one must give new rules for the langauge.

What is importantly the same in these two views, however, is the aforementioned freedom in language construction. It is precisely this freedom that is eventually enshrined in Carnap's thought as the Principle of Tolerance. There, Carnap makes this freedom explicit by saying,

> Our attitude to requirements of this [absolutist] kind is given a general formulation in the *Principle of Tolerance*: *It is not our business to set up prohibitions, but to arrive at conventions.* [...] 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.[28]

Read in the context of the debate over protocol sentences, the Principle is what licenses the move that Carnap makes to confirmation rather than verification. Additionally, as we will see below, it gives him the tools to avoid certain unpalatable consequences of the Circle's Wittgensteinian views on logic and mathematics. It is to these problems, and the solutions that Carnap gives in *LSL* that we now turn.

As was shown above, in section 3.1.3, by the end of 1930 Carnap found himself in a difficult situation. His commitments to both universalism about logic and verificationism had led him into philosophical trouble, and something new was needed if he was to be able to save some parts of his vision of

[27] [Carnap, 1953b], p 459.
[28] [Carnap, 1937], pp 51 – 52. Original emphasis.

a scientifically-informed, metaphysics-free philosophical theory. The problem that seemed to worry him the most at that time was the question of whether or not one could talk about the syntax of a language. Famously, Carnap and the Circle understood Wittgenstein to think that this was impossible, or at least impossible to do so truly.[29] But, at the same time as he maintained his universalism about logic, Carnap was also interested in some recent developments in mathematical logic. In particular, during the summer of 1930, Carnap had been in close contact with Gödel as he produced his incompleteness theorems. Indeed, Carnap was one of the first people Gödel told of his discovery.[30] One of the key insights that Gödel had in the proofs is the notion that one can encode statements about the language of arithmetic into the language of arithmetic itself. To Carnap's mind, though this method struck directly at the heart of Wittgenstein's distinction between saying and showing which was a lynchpin of the Circle's account of meaningfulness, it nonetheless appeared to make perfectly good mathematical sense. Moreover, the technique was quite fruitful as Gödel had produced a revolutionary result using it.

This tension in Carnap's thought, on the one hand persuaded by his universalism and verificationism while on the other admiring the technical fruitfulness of metamathematical techniques, came to a head in the final months of 1930, lasting until January of 1931 when he finally saw his way out of the difficulty. The moment when his new perspective came to him is dramatically reported in the *IA*:

> After thinking about [the problem of whether or not it is possible to speak about language] for several years, the whole theory of language structure and its possible applications in philosophy came to me like a vision during a sleepless night in January 1931, when I was ill. On the following day, still in bed with a fever, I wrote down my ideas on forty-four pages under the title "Attempt at a Metalogic". These shorthand notes were the fist version of my book *Logical Syntax of Language*.[31]

What he saw that night was a structure where the expressions of one language refer to the expressions of another. In modern parlance we call this the distinction between an object-language and a metalanguage. But, for our purposes at this juncture, the real importance of this episode is that it marks Carnap's realization that he could bring the tolerant attitude from his childhood to bear on his logical and mathematical views. He seems to have realized that by adopting this tolerant attitude he could resolve the divergence between the universalists about logic (Frege, Russell, and Wittgenstein) on the one hand and those who took logic itself to be an object of logical investigation (Hilbert, Tarski, Gödel)

[29]If it were possible, then it would imply that there were facts, in Wittgenstein's sense of the word, about syntax. Rather, on his view, the syntax of our language arises because of the structure of our representational faculties, and so can only be shown by our representations. As noted above, this understanding of Wittgenstein is contested by Kuusela in his [Kuusela, 2012].

[30]See [Goldfarb, 2003], p 335, [Dawson Jr., 1984], p 115, and [Dawson Jr., 1997], p 68.

[31][Carnap, 1963], p 53. In fact, the development from these notes to the book was a labor of almost three years. See [Carus, 2007], chapter 9 for a detailed account.

on the other. Though the realization that abandoning a single-language approach might resolve his difficulties came from considering his stance on logic and mathematics in the light of Gödel's results, the first published evidence of his change of heart comes in the papers on the protocol sentence debate.

As we saw in our discussion of the protocol sentence debate in chapter 3 above, Carnap thought that if the requirement of verification was maintained as an absolute, then we run the risk of "fall[ing] into metaphysical dead ends".[32] Because of this danger, the only appropriate way to understand requirements that appear to be absolutes, he thinks, is as suggestions for possible ways we might construct the language of science. To reiterate the way he puts the point in OPS:

> The questions of whether the protocol sentences occur outside or inside the system language and of their exact characterization are, it seems to me, not answered by assertions but rather by postulations. [...] They are to be understood as suggestions for postulates; the task consists in investigating the consequences of these various possible postulations and in testing their practical utility.[33]

And so, as noted before, the way disputes like those over the nature of the protocol sentences are to be settled is by investigating the consequences of the various proposals. In the case of his universalism about logic and mathematics, Carnap saw a similar situation. For both Wittgenstein and the right wing of the Circle, the adherence to a philosophical requirement forced the rejection of certain technical programs in mathematics, for example both the metamathematical approach of the Hilbert school, and Brouwer's intuitionism. But Gödel, who had adopted some of the formalists' techniques, had made a significant discovery. So here it would seem that the consequences of adopting his approach had a real benefit, namely the ability to show some very deep results, whereas the restrictions that the Circle had originally adopted might prevent one from recognizing these results.[34] But, this resistance to certain mathematical techniques on the basis of a philosophical view was precisely the sort of dogmatism that Carnap had repeatedly railed against in his writings.[35] It now started to become clear to him that the old Circle view suffered from the same defects as those programs which the Circle had mocked.

The many-languages approach that Carnap adopted was suggested to him by his interactions with Neurath and Popper during the protocol sentence debate, and by Gödel in their discussions in the summer of 1930 on the topic of the axiomatic methods in science.[36] It was additionally suggested by Karl Menger, one of the members of the Circle, and the convener of the Mathematical Colloquium which was one of several reading groups that formed in the Circle's

[32] [Carnap, 1987], p 469. Carnap's quotation is taken from an article by Neurath.

[33] [Carnap, 1987], p 458.

[34] I do not mean to suggest that any members of the Circle rejected the incompleteness results. Rather, that as a consequence of the prohibition on expressing the rules of language in that language itself, it might have seemed that the results were achieved in a philosophically suspect way.

[35] As noted above, [Carnap, 1959a] and [Neurath, 1973] are good examples.

[36] [Carnap, 1963], pp 56 – 58.

orbit.[37] As Menger reports in his "Introducing Logical Tolerance", when he joined the Circle in 1927 he repeatedly took exception to the members' use of singular expressions when discussing 'the' language of science, or 'the' logic of science.[38] The results of Carnap's thinking on this issue appear in partial form in OPS, but take their mature shape in *LSL* as the Principle of Tolerance, first introduced as such in the introduction to that book:

> For language, in its mathematical form, can be constructed according to the preferences of any one of the points of view represented; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or other of the choices leads, including the question of non-contradiction. The standpoint which we have suggested – we will call it the Principle of Tolerance [...] – relates not only to mathematics, but to all questions of logic.[39]

The full statement of the Principle comes a short way into the book. After a prolonged examination of different restrictions that various philosophers and mathematicians had argued must be placed on logic, Carnap says:

> In the foregoing we have discussed several examples of negative requirements (especially those of Brouwer, Kaufmann, and Wittgenstein) by which certain common forms of language – methods of expression and of inference – would be excluded. Our attitude to requirements of this kind is given a general formulation in 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.
>
> 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. In the conflict over the logical foundations of mathematics, this attitude was represented with especial emphasis (and apperently before anyone else) by Menger.[40]

As ever, here Carnap is quite bold in his writing. But, dramatic as this sounds, the question that must be answered is whether we can take him at his word. That is, does he really mean to say that there are no restrictions *at all* on language construction? Or, should we instead think that, while there are no

[37] See [Popper, 1963b], p 66.

[38] [Menger, 1979a], p 12. More will be said about Menger's claim that Tolerance is due to him in section 4.2.2 below.

[39] [Carnap, 1937], p *xv*.

[40] [Carnap, 1937], p 52. Carnap's emphasis and formatting. The change in formatting of the text in the last paragraph in this quotation indicates a footnote (or perhaps a comment) in Carnap's text.

restrictions of the kind that he has considered in the pages leading up to his statement of the Principle, there could in principle be restrictions of some kind? Call the reading whereby Carnap is committed to there being absolutely no restrictions whatsoever Wide Tolerance, and correspondingly call the thesis that it is possible, at least in principle, to have some kinds of restrictions on languages Narrow Tolerance. In chapter 6 below, I will argue that Carnap is committed to Wide Tolerance. In this chapter, however, we have two remaining tasks. The first is to lay out, at long last, the position on mathematics that Carnap defends in *LSL*. The second is to address Menger's charge that the attitude Carnap adopts in this period, and encapsulates in the Principle of Tolerance, is essentially the same one that Menger advocated in his paper "On Intuitionism". We take up the priority challenge first.

4.2.2 Menger's Priority Challenge

As mentioned above, Karl Menger was a member of the Vienna Circle from 1927, when he joined the mathematics faculty at the University of Vienna as the Professor of Geometry, until the Circle's meetings came to an end with the deaths of Hahn in 1934 and Schlick in 1936. The Circle benefitted greatly from Menger's participation. He organized the Mathematics Colloquium, which was one of the satellite reading groups that formed around the members of the Circle, and it was also partly due to the time he spent in Amsterdam that L.E.J. Brouwer was invited to give two talks in Vienna in 1928.[41] However, what will occupy our attention here is not the connections that Menger had with Intuitionism, but rather the claim that he makes, both in his essay "Logical Tolerance in the Vienna Circle" and reiterated in his memoir, that Carnap's Principle of Tolerance and the corresponding tolerant attitude he adopts, is fundamentally the same as Menger's own tolerant view, and that Carnap did not properly acknowledge this intellectual debt.

In a terse passage in his memoir, Menger claims that, despite Carnap's assertion that he operated in a 'tolerant' manner at least from the time he was an undergraduate student at Jena in the published version of the *IA*, or from even earlier in the manuscript version (see section 4.1 above), in fact this attitude is due to Menger's questioning the universalism prevalent in the Circle at the time he joined their discussions. Menger says:

> Another of my questions [to the Circle] concerned language. I objected to the recurring references in the Circle to *the* language and repeatedly asked Carnap, Schlick, and other members what justified the implied belief in the uniqueness of language. [. . .]
>
> In the course of the following years, however, Carnap not only gave up th[e] belief [in the existence of a single language of science

[41] The first of these talks was particularly significant because it was as close as Wittgenstein ever came to attending an official meeting of the Circle, and Menger speculates in his posthumously published memoir that the content of these talks inspired at least some of Wittgenstein's later remarks on the infinite. See [Menger, 1994], pp 135 – 137. Also, Feigl claims that it was these two talks by Brouwer that inspired Wittgenstein's return to philosophy at all. See [Feigl, 1969], p 639.

> or of mathematics] but emphasized the importance of the existence of a multiplicity of languages between which one may choose, while Schlick and Waismann continued speaking about *the* language.
>
> In 1937, when his Viennese period was still fresh in his mind, Carnap wrote in his book *Logical Syntax of Language* "...the earlier position of the Vienna Circle, which was in essentials that of Wittgenstein. On that view it was [a] question of '*the* language' in an absolute sense; it was thought possible to reject both concepts and sentences if they did not fit into *the* language." [...] **In contrast to these passages, in the Intellectual Autobiography, written twenty five years later, Carnap strongly emphasizes the idea of freedom to choose one of a variety of languages had already been one of his leading thoughts in his pre-Vienna period. The development of the idea about the multiplicity of languages is one of those facts that Carnap seems to have completely misremembered in his later years.**[42]

Since this was a relatively unedited manuscript – it was still unfinished at the time of Menger's death – there are some difficulties in determining precisely what Menger means in some passages. In the passage above, there are at least three different claims that Menger could have had in mind when he wrote these complaints. Firstly, he could mean that, while Tolerance and Menger's multiplicity view are different things, he is nonetheless due some credit from Carnap for the Principle. But, as quoted above, in the footnote that immediately follows his statement of Tolerance, Carnap does give some credit to Menger for adopting a stance that is in accordance with Tolerance in the debate over the foundations of mathematics.[43] Moreover, he says explicitly that Menger adopted this stance before anyone else, though he includes a small hedge by saying that Menger was only 'apparently' first. Given this, it would seem that Carnap has at least acknowledged Menger's contributions to his thinking, and so it is not likely that lack of such an acknowledgment is Menger's complaint. Perhaps, instead, Menger means that the credit is not enough, couched as it is in terms that suggest that his stance was merely 'in accordance' with what Carnap is suggesting. However, this complaint hardly seems to warrant either the length of the passage or its repetition in at least one other source, [Menger, 1979a], about which more will be said below. There are two other interpretations that are somewhat more serious. They are: (1) the possibility that Menger thinks that the Principle is really the same as his multiplicity view and should be acknowledged as such, or (2) the possibility that Menger thinks that Carnap was inspired by the multiplicity view, and that, while Tolerance is not identical to it, the Principle is nonetheless a development of the multiplicity view. Indeed, these interpretations seems plausible in light of what Menger says on this issue in both the introduction to [Menger, 1979a], and in

[42] [Menger, 1994], p 141. Original italics, my boldface. Presumably the reference to *LSL* being published in 1937 is a small error on Menger's part.

[43] [Carnap, 1937], p 52.

his essay "Logical Tolerance in the Vienna Circle" which is published in the same volume. For example, in [Menger, 1979a] he says,

> Part I includes papers published about 1930 which expound an idea that Carnap, after a short period of opposition in the Circle, fully adopted; and, under the name "*Principle of Tolerance*", he eloquently formulated it in great generality in his book, *Logical Syntax of Language* (1934), through which it was widely disseminated.[44]

In this quotation, Menger clearly claims that the view that Carnap expresses in his Principle is the very same as the one he put forward. That is, Menger claims that Carnap's Tolerance is just Menger's own view, though more 'eloquently formulated'. However, when we look back at the relevant papers, this claim looks unlikely to be true. In the paper "On Intuitionism", which is the one that Carnap cites in his statement of the Principle of Tolerance, Menger makes some gestures at a position that is somewhat similar to the Principle. In the crucial passage he says,

> The author [Menger] has repeatedly expressed the opinion that the heretofore undefined concept of constructivity could be made precise in different ways and degrees. Even in the intuitive parts of geometry there is no word or idea which would inevitably demand a particular definition and could not be made precise in several different ways. No doubt this is true to an even greater extent of the nebulous idea of constructivity. For each of the various versions of constructivity one could develop a corresponding deductive mathematics. [...]
>
> What the intuitionistic attempts to date have done is to attach themselves dogmatically to some particular notion of constructivity (in most cases not clearly circumscribed), to accept only the resulting developments as meaningful, and to reject any others as meaningless. In the opinion of the author [Menger] such a position is totally devoid of cognitive content. For what matters in mathematics and logic is not which axioms and rules of inference are chosen, but rather what is derived from them.[45]

The picture that Menger is presenting here is one where, for any mathematical notion about which we might disagree, what matters in our disagreement is not the metaphysical or epistemological status of the relevant notion. Rather what matters is what one can derive from the deductive system that one sets up in order to make one's metaphysical or epistemological views about the disputed principle precise. Menger calls this view 'implicationistic' because it is concerned solely with what the implications of adopting various mathematical principles are, and not at all with mere biographical facts about particular mathematicians' reasons for adopting or rejecting various principles. As Friedman argues in his [Friedman, 2001], the attitude that Menger expresses in his

[44] [Menger, 1979a], p 1.
[45] [Menger, 1979c], pp 56 – 57.

implicationistic view, and that Carnap says is shared by most 'working mathematicians', is one that is dramatically dismissive of disagreements about foundational principles; after all they are merely uninteresting biographical details about mathematicians' choices.[46] However, as Friedman goes on to argue, this is not Carnap's Tolerance at all. To see the differences requires a brief digression into the use to which these philosophers put their respective principles.

On Menger's view, the interesting question is not which axioms and rules of inference to take as primitive, but what can be derived from various groupings of axioms and rules, and perhaps even more interesting, the issue of the relationships between these various systems. But, this view faces a serious revenge problem. To wit, just how should the investigation of the relationships between the various systems proceed? Any attempt to give a definite answer to this question simply re-introduces at the meta-level the disputes that we were advised to dismiss at the object-level, and so nothing is solved by adopting this stance. However, there is a sense in which, at least from Menger's perspective, this revenge issue does not matter. After all, he thinks that the debate over the foundations of mathematics is merely a collection of "descriptions of subjective psychological processes or expressions of subjective tastes".[47] In short, then, Menger thinks that every participant in the foundations debate is wrong, at least insofar as they attempt to assert their preferences as statements of fact about the nature of mathematics – moreover, he thinks we should adopt the attitude of the 'working mathematician' and simply get on with the business of doing mathematics, though just how is left unclear.

At first, it may seem as if Carnap's Tolerance is quite similar to Menger's view as it was just presented. It is certainly the case that Carnap thinks that the interesting questions about the foundations of mathematics are not about which things are taken as primitive, but rather about the consequences of these choices. After all, "[...] language, in its mathematical form, can be constructed according to the preferences of any one of the points of view represented [in the ongoing debates over the foundations of mathematics]; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or other of the choices leads [...]".[48] He also thinks that much of the debate as it stood in the late 1920s and early 1930s was comprised of "pseudo-problems and wearisome controversies".[49] Finally, it is true that Carnap acknowledges that the attitude he is proposing in the Principle is that of "most working mathematicians", though he does not elaborate on what that means.[50] However, the similarities with Menger's implicationism end here. Where Menger suggests that the way to resolve the disputes about the foundations of mathematics is to simply ignore them, Carnap invites us to regard them as proposals for language-systems which we might adopt for making precise our scientific practice. These proposals are then investigated to see what their consequences are, and finally judged on the basis of the pragmatic utility

[46] [Friedman, 2001], especially section III.
[47] [Menger, 1979b], p 37.
[48] [Carnap, 1937], p *xv*.
[49] [Carnap, 1937], p *xv*.
[50] [Carnap, 1937], p 52.

of adopting them.[51] That is, in cases where mathematicians or philosophers disagree over what counts as acceptable mathematics, what should be done on Carnap's view is to treat any requirements that have been put forward, insofar as they can be made mathematically precise, as proposals for ways to construct a logical system. With the proposed systems in hand, then, we trace out the consequences of adopting each one on our mathematical practice. A decision about which system to adopt is then made by comparing the proposed languages in terms of their perspicuity for a particular task (in *LSL*, the task Carnap has in mind is giving a foundation for the mathematical portion of our scientific practice). This pragmatic orientation is in no way dismissive of the foundational debates in mathematics, rather it provides a method for resolving such disputes which is more substantial than simply ignoring them.[52] It is this last step that marks Carnap's Tolerance as distinct from Menger's implicationism.

To sum up briefly, we have seen that while Menger had a view that was in many ways comparable to Carnap's, there are certain fundamental differences. These differences are centered around the way in which they see their principles being put to use. In Menger's case, his implicationism is designed to simply sweep away foundational concerns with a blanket dismissal, and to thereby clear the path for mathematicians to continue their work. For Carnap, however, the point is not to simply dismiss these concerns *tout court*, but to provide a constructive way to resolve foundational disputes without at the same time reengaging with tiresome, and possibly intractable, philosophical disputes.

4.2.3 Carnap's *LSL* View

The purpose of this section is to give a detailed description of the position on mathematics and logic that Carnap adopts in *LSL*. As we have shown above, the period in which Carnap wrote the book was one in which his views were in flux. The story that we have told so far is one where *Logical Syntax* and the Principle of Tolerance are the result of the collapse of Carnap's previous views. In brief, the discovery of incompleteness showed that a single-language approach to logic and mathematics was untenable. With prompting, partially from Menger and partly from Neurath and Popper, Carnap returned to the tolerant attitude of his childhood, and fashioned a multi-language view from it. The rhetoric of *LSL*, especially at the beginning, is soaring and it is clear that in Carnap's mind the book represents a bold departure from previous orthodoxy on the nature of logic. We turn now to the question of what the new view is, and the role that Tolerance plays in it.

Logical Syntax can be roughly divided into two separate, but related, projects. The first is to construct two languages, named Language I and Language II, to show that they meet certain requirements, and finally to prove certain meta-level results about them, in particular to investigate whether they are capable of giving an effective account of mathematical validity. This first project occupies parts one to four of the book, though the fourth part of the book concerns

[51] [Carnap, 1937], p 46 – 47.

[52] Friedman makes a similar point at [Friedman, 2001], p 232. A similar interpretation of the purpose of Carnap's Tolerance is found in [Richardson, 1994].

what Carnap calls 'General Syntax', which is supposed to show certain results about the syntactical structure of any language whatsoever as opposed to just languages I and II. The second project is entirely contained in the fifth part of the book called "Philosophy and Syntax". This goal of this project is to lay the foundations for the construction of a language adequate for the logical reconstruction of science, and was closely tied into the "Unified Science" effort which Carnap and Naurath were pursuing during the 1930s and into the 1940s. The aim of part V of the book, and indeed the Unified Science program, was to create a language that could serve as the common language for all of the sciences; both formal sciences as, for example, logic and mathematics as well as natural sciences like physics or biology. Though the ideas in this second part of the book are better known in the context of his distinction between the formal and material modes of speech, they are also extremely important for understanding Carnap's view of logic and mathematics.[53] In the rest of this section, I will lay out Carnap's view of logic and mathematics by working through each of these two projects, and then showing how the salient pieces are related to each other. We begin with a discussion of the second project.

The fifth part of *Logical Syntax*, entitled "Philosophy and Syntax", is an attempt to show how one can use formal tools to give a single language capable of unifying the various sciences. In other words, the idea is to create a language with a precisely defined logical structure where it can be shown how the concepts in use in various sciences are related to each other, and, for the candidate languages that Carnap is interested in, how the concepts of natural sciences like biology or chemistry can be defined in terms of those in use in the special sciences. The aim of this project, however, is not foundational in the traditional sense, though it does give a kind of 'foundation' for the sciences. It will not matter to Carnap specifically which of the sciences' terms are used to define those in the other sciences. In order to show this, Carnap engages in a lengthy examination of the meaningfulness of sentences which begins by distinguishing between 'object-sentences' (sentences whose subject appears to be objects or their properties) and 'logical sentences' (sentences which are concerned with the meanings of terms or the inferential relationships between sentences). Within the category of object-sentences a further distinction between genuine object-sentences and pseudo-object-sentences is drawn, which serves to demarcate those sentences which are about objects from those which appear to be but in fact are not. The point of drawing these distinctions is to show that there is nothing besides science needed to explain the results of science – that is, there need not be anything metaphysical used in the sciences – and to diagnose what he sees as widespread confusion on this point.[54] In order to see why these considerations do the work Carnap wants them to, however, we need say a bit more about how one knows if a sentence is a genuine object-sentence

[53]This section of the book also served as the basis for a series of lectures that Carnap delivered in London in 1934, which were then published in a popularized format as his monograph *Philosophy and Logical Syntax*, and thence became better known.

[54]This conviction of the Circle's is memorably announced in the 1929 manifesto with the slogan "In science there are no 'depths'; there is surface everywhere [. . .]" ([Neurath, 1973], p 306).

or only a pseudo-object-sentence.

In order to get a handle on the line Carnap draws between genuine object-sentences and their pseudo-object-sentence imitators, we need to first define some terminology. First, we note that all sentences are sentences of some language or other, and that each of these languages has a formal structure. Within these structures it is possible to give a precise characterization of their domain of discourse. That is, we are able to say precisely and exhaustively what objects we are talking about in a given language. With that, we can then see by logical analysis of the statements of the language in question which of its statements refer to objects in the language's domain and which do not. Those statements that are well-formed but fail to refer, then, are the pseudo-object-sentences. So, languages conceived of in the way that Carnap suggests are composed of a vocabulary (predicates and names), and a domain (objects for the names to range over). The structure is completed with two types of rules: rules of formation, which give the conditions under which various combinations of symbols constitute a sentence, and rules of transformation, which give the inferential relationships between sentences.[55] Within the transformation rules, a further categorization is drawn between L-rules and P-rules, that is between rules that regard logical inferences and those that regard empirical ones. An example of an L-rule is *modus ponens*, while as examples of P-rules Carnap suggests the universally quantified laws of physics. With these distinctions drawn, we can close under the rules and arrive at the theory of the language we have constructed. Within the theory, we can then define a few classifications of statements. Call those statements whose truth or falsity follows from the rules alone (or alternatively from the rules plus the empty set of assumptions) 'analytic' if they are true, and 'contradictory' if false. The other sentences of the language depend for the determination of their truth on factors other than the rules of the language, and are called 'synthetic'.[56] It is important to notice that within the analytic statements there will be some statements which we might have normally thought were synthetic depending on which L-rules are chosen. Finally, we define the content of a sentence to be the set of its non-analytic consequences.[57] We are now in a position to return the to the task of distinguishing the object-sentences of a language from the pseudo-object-sentences.

As we just defined it above, sentences have content when they have non-analytic consequences. But, analytic sentences themselves will have no consequences with content; this follows from the definition of analyticity.[58] Consider some statement of a language, $\mathcal{L}_1$. In order to determine whether or not it is a genuine object-sentence, we first check to see that it is not analytic. If it is, then we know it is not an object-sentence at all, so we are done. Having determined that the sentence in question is synthetic, we next we check whether or not the sentence has content. If it does, then we have a genuine

[55] [Carnap, 1937], §2.

[56] Carnap's diagram on [Carnap, 1937], p 185 is instructive for understanding his view of these divisions.

[57] [Carnap, 1937], 42. See also *Ibid.* p 175.

[58] In this way, then, Wittgenstein's thought that the truths of logic and mathematics are 'empty', discussed in 3.1 above, is preserved in Carnap's *LSL* view.

object-sentence, and the set of these will be the only meaningful statements in the language for Carnap. If it does not have content, however, we have a meaningless pseudo-object-sentence.

With this analysis of the meaningful sentences in a given language in hand, Carnap can now return to the question of the 'foundations' of the various sciences. The problem of distinguishing the confused pseudo-object-sentences from the genuine sentences that he had set out to address is now solved. The foundation for the various natural sciences that he proposes, then, is just those things which the language that one constructs for representing that science (or those sciences) commits one to.[59] However, this leaves him with an apparent problem, namely his commitments with regard to the formal sciences. If one were to take the view that we have been developing as it stands so far, then one would be committed to everything that one's mathematical theory commits one to in precisely the same way as one is committed to those things that one's theory of physics, say, commits one to its objects. But, for a committed empiricist like Carnap, accepting numbers or sets as existing in precisely the same way that atoms do may seem problematic. However, the answer to this problem is obvious, at least from the perspective of Carnap's development that we have built up over the last few chapters.

Taking the view he learned from Frege as a starting point, namely that logic is the paradigmatic analytic science, and combining it with Russell's successful reduction of mathematics to logic results in the view that mathematics itself is analytic. This on its own is not quite enough to resolve the difficulty, but the combination of Carnap's logicism with Wittgenstein's view that logic is fundamentally linguistic, as we have seen happens in *LSL*, transforms the problem from one of unacceptable commitments to mathematical objects into one where the referents of mathematical expressions are simply empty (because tautologous) sentences that express the linguistic form of the language of which they are a part. That is to say that since logic, on the *LSL* view, is nothing but the system of rules one lays down for the language at the construction stage, and that since mathematics on this view is reducible to logic, then mathematics too is just some portion of the consequences of those rules. This means that depending on what rules one starts with, one will end up with different logics and therefore different mathematics as well. This is no problem for the view, though, because it is precisely what Tolerance licenses.[60] The position that Carnap arrived at, then, is one that saves both the analytic truth of logic and mathematics, but also preserves some potion of the empiricist thought that knowledge of the content of the world comes only via the senses.[61] However, this appeal to Wittgenstein's view – or at least to the Circle's understanding of

[59]This view is one that similarly remains with Carnap for the rest of his career. It is most famously found in his 1950 essay "Empiricism, Semantics and Ontology".

[60]Carnap's view, characterized as we have here, has been given the rather catchy title "tautologicism" by Awodey and Carus. For example, see [Carus, 2007], p 188, and [Awodey and Carus, 2009], p 81.

[61]It should be remarked, however, that because so much of what would have been the domain of a posteriori investigation in a classical empiricist view is built into our languages in Carnap's, his position would likely have been unrecognizable as empiricist by the likes of, for example, Hume or Mill.

it – itself engenders a problem for Carnap. Namely, it was one of Wittgenstein's central tenets that language could not express its syntax, only show it. In *LSL*, however, Carnap is committed to making the syntax of a language explicit, and not only that, but explicit in that language itself. This is the project of the first four parts of the book, and we now turn to it.

We will begin with a brief review of Wittgenstein's views on why the syntax of a language must be only shown and not said. As we argued in section 3.1.3 above, rules of a language and even statements about a language must, on Wittgenstein's view, be formulated somehow from inside the language if they are to be meaningful, which is to say that they must be formulated inside the language if they are to be said. But because the syntactical rules of a language are not matters of empirical fact, they are not proper contents of meaningful statements on his view. They can, however, be shown. The rules are exhibited by the forms of meaningful statements of the language, and additionally by the instances of successful inferences, as was noted at the end of section 3.1.3. However, as we also noted, Carnap saw no problem in adopting only part of Wittgenstein's views on this matter, and so left aside the thought that the syntax of a language must be un-sayable in that language. Instead of understanding the problem as one of philosophical scruples, rather he saw it as a technical problem. Namely, the issue was to determine whether one could actually construct a language such that its syntax is expressible in that very language, and he interpreted Wittgenstein to have made a technical claim that such a construction was not possible.[62] The refutation of that claim is the project that Carnap embarks on in the first portion of *LSL*.

Carnap announces that this is his aim at the very outset of the book. In the introduction he says,

> With Language I as an example, it will be shown, in what follows, how the syntax of a language may be formulated within that language itself (Part II). The usual fear that thereby contradictions – the so-called 'epistemological' or 'linguistic' antinomies – must arise, is not justified.[63]

There are two striking features of this comment that merit our attention, both to do with the worry about paradox. The first is the stated goal, that is to show that languages whose syntax is expressible in that very language are not thereby paradoxical. The second is the fact that Carnap seems to worry about paradoxes at all, which is somewhat odd given the interpretation of Tolerance that I will attribute to him, as we will see below. We take these two points in turn, beginning with the first. Though he does not attribute the thought that the expressibility of a language's syntax causes paradoxes in it to Wittgenstein in the introduction, he makes it clear that he has precisely Wittgenstein's doctrine in mind at the beginning of Part II where he says,

[62] It is important to note that Wittgenstein would have never agreed that he had made such a claim, if only to mark how far Carnap has moved away from adopting Wittgenstein's views wholesale.

[63] [Carnap, 1937], p *xiv*.

> Up to the present, we have differentiated between the object-language and the syntax-language in which the syntax of the object-language is formulated. Are these necessarily two separate languages? If this question is answered in the affirmative (as it is by Herbrand in connection with metamathematics), then a third language will be necessary for the formulation of the syntax of the syntax-language, and so on to infinity. *According to another opinion (that of Wittgenstein), there exists only one language, and what we call syntax cannot be expressed at all – it can only "be shown". As opposed to these views, we intend to show that, actually, it is possible to manage with one language only; not, however, by renouncing syntax, but by demonstrating that without the emergence of contradictions the syntax of this language can be formulated within this language itself.*[64]

There are two parts to the task. First, Carnap must show how to construct a language which can represent its syntax in that language itself. Having done that, he must then show that the language so constructed is consistent. So, how then is Carnap to accomplish these tasks? The answer for the first part was suggested to him, as he recounts things in the *IA*, by Gödel.[65] In the summer of 1930, Gödel had shown Carnap his incompleteness results, and in so doing had also shown how to use the technique of 'arithmetization' that he had pioneered for the proofs. It is this technique that allows Carnap to carry out his goal. Though arithmetization was novel at the time it is now quite well known, so we need not belabor this point; moreover, there are no surprises in Carnap's implementation. What is more interesting, and indeed much more revelatory of how Carnap understood his view, is his consistency proof.

The proof spans three sections of the book, beginning with §34*i* and continuing through §36.[66] The overall strategy for the proof is to show that while every demonstrable sentence of the language is analytic, not every sentence of the language is. The 'demonstrable' sentences are just those sentences which are derivable from the empty set of premises. This definition is very similar to that given for analyticty, but Carnap is at pains to note the differences between these two notions which, as with arithmetization, were novel at the time *LSL* was written. Nonetheless, it is worth pausing to see how Carnap marked the differences. To see how he distinguishes these two notions we will first need some more definitions.[67] We begin with the definiteness: a sentence is said to possess a definite property when there is an algorithmic method of determining in a finite number of steps whether it has that property or not. A goal

[64][Carnap, 1937], p 53. My emphasis. One might suspect that the problem with a hierarchy of syntax-languages might have been suggested to Carnap by Russell's introduction to the *Tractatus* (see [Wittgenstein, 1983], pp 22 – 23). Another possible source of this idea is Tarski. Unfortunately, I cannot find any evidence one way or the other on this issue.

[65][Carnap, 1963], p 54.

[66]The sections in the English translation of *LSL* that are marked with letters after the section number is material that was in the original manuscript, though Carnap admits it was in a slightly different format, but was removed to save space in the first published version. See [Carnap, 1937], p *xi*.

[67]Here I follow the exposition given in [Procházka, 2006], sections 2, 5, and 6.

that Carnap takes up in *LSL* is to develop a criterion of mathematical validity which is definite in the language of classical mathematics. However, we know that the property of 'derivability' will only be definite – and therefore able to serve as the desired criterion of mathematical validity – in very weak languages. Indeed, as Gödel's theorems show, in languages with the resources for addition and multiplication there are sentences which will be mathematically valid but not derivable. Call these sentences 'Gödel sentences', as is now common, and we will refer to them by the symbol $\mathfrak{G}$. In *LSL*, Carnap writes this sentence as:

$$\neg \text{BewSatzII}(r, \text{subst}[\ldots]) \qquad (\mathfrak{G})$$

where 'r' is a free variable ranging over series numbers in Langauge II, 'BewSatzII(a,b)' is a two place relation that is "true when and only when a is the series number of a proof in accordance with the rules [of Language II], and b is the series number of the last sentence in this proof", and 'subst[...]' is a shorthand for the necessary values of the substitution relation which replace the variable b in 'BewSatzII' with the series number of this very sentence.[68]

A first instinct that one might have to get around this problem is to abandon a commitment to finite rules of inference. In particular one might think that enriching the language with a rule that allows one to go from an infinite series of sentences like:

$$\varphi(1), \varphi(2), \ldots, \varphi(i), \ldots$$

to the universally quantified sentence:

$$\forall x \varphi(x)$$

will sidestep the problem. This rule is now known as an ω-rule. Before we can motivate the thought that adding a rule of this sort to one's logic could be a way around incompleteness, however, we need to say a bit about just how the proof of the theorems works. The proof of the incompleteness theorems involves two parts: in the first, we construct a sentence which denies that a sentence with a particular series number has the 'BewSatzII' property, and in the second we show that this sentence is not demonstrable in Language II as long as II is consistent.[69] The details of the first part need not trouble us here, and instead we focus on the second. We can see that $\mathfrak{G}$ is true just in case there is no number r which is the series number of a proof which has sentence represented by 'subst[...]' as its final line. But, as we defined it, 'subst[...]' is just short hand for the series number of the sentence which results from inserting the series number for $\mathfrak{G}$ in the second argument place in the 'BewSatzII' relation in

[68] [Carnap, 1937], p 132.

[69] Sometimes it is said that these sentences 'say of themselves that they are true but not provable'. While this may be a useful aid to understanding how Gödel sentences work, I think it is misleading in the current context because, following a rather strict reading of Carnap, we are presently working in an uninterpreted language. That is, when we deny that there is a sentence with a certain series number possessing a certain property, we have not yet said what the 'meaning' of the property *is*. So, even granting that we will interpret the property as being 'provability-in-$\mathcal{L}$', we do not need to do so in order to prove the theorem, and so have not yet provided such an interpretation.

$\mathfrak{G}$ itself. Put simply, $\mathfrak{G}$ is true just in case there is no number which is the series number of the proof of $\mathfrak{G}$. So, if it is true, then $\mathfrak{G}$ is not provable in Language II. But, what we need to show is that $\mathfrak{G}$ is undecidable, that is that neither it nor its negation is demonstrable. So, we now need to show that the negation of $\mathfrak{G}$ is not demonstrable either. For reductio, assume that the negation of $\mathfrak{G}$ is provable. This is equivalent to proving the sentence:

$$\exists r \text{BewSatzII}(r, \text{subst}[\ldots]).$$

That is, if $\mathfrak{G}$ is false, then there is some value of r which is the series number of a proof which has $\mathfrak{G}$ as its final line. But, since r in the original Gödel sentence is a free variable, the substitution of any numerical value for it in the sentence *also* yields a demonstrable sentence. In other words, we could prove that:

$$\neg\text{BewSatzII}(n, \text{subst}[\ldots])$$

for each particular numerical value n. This would mean that, while for each n we can prove that it is not the series number of a proof with $\mathfrak{G}$ as its final line, there is still nonetheless a number which *is.* That is, the language would be ω-inconsistent. So, on the assumption that Language II is ω-consistent, we have shown that neither $\mathfrak{G}$ nor its negation is demonstrable in II. With these preliminaries regarding the proof of the incompleteness results out of the way, let us return the question of whether the ω-rule offers a way to avoid the problem.

In part, what inspires thinking in the direction of an ω-rule is the proof that the negation of $\mathfrak{G}$ is not demonstrable. The situation, as we saw, is that we are able to prove for each particular n that it is not the series number of a proof with $\mathfrak{G}$ as the final line, which, in essence, is a list of sentences:

$$\begin{gathered}\neg\text{BewSatzII}(1, \text{subst}[\ldots]),\\ \neg\text{BewSatzII}(2, \text{subst}[\ldots]),\\ \vdots\\ \neg\text{BewSatzII}(n, \text{subst}[\ldots])\end{gathered}$$

which is infinite.[70] The thought goes that if instead we were permitted to infer from this list to their universally quantified counterpart,

$$\forall n \neg\text{BewSatzII}(n, \text{subst}[\ldots])$$

then it would be the case that the language was complete, because we could prove $\mathfrak{G}$ by inferring the equivalent statement $\forall n \neg\text{BewSatzII}(n, \text{subst}[\ldots])$. Carnap was not the only person considering such an approach around this time.[71] For example, after he heard about Gödel's theorems from his assistant Bernays, Hilbert, who had previously objected to infinitary rules on philosophical grounds, thought that an ω-rule might be finitistically acceptable.[72] In his lecture "The Grounding of Elementary Number Theory", delivered in December of 1930 to the Philosophical Society in Hamburg, and later published in the *Mathematische Annalen*, he says,

[70] We should also add the caveat that '1', '2', etc. need to be defined as number symbols.
[71] For more on Carnap and the ω-rule, see [Buldt, 2004].
[72] [Reid, 1970], pp 198 – 199.

> The infinite is realized nowhere; it does not exist in nature, nor is it admissible as a foundation of our rational thought. And yet we cannot dispense with the unconditional application of the *tertium non datur* and of negation, since otherwise the gapless and unified construction of our science would be impossible. So operation with the infinite must be secured in the finite; and precisely this occurs in my proof theory.[73]

Despite his uneasiness with the infinite, which seems to still be part of his thought here, several transfinite rules are included in the presentation of his proof theory, and amongst them is an ω-rule.[74] Indeed, a few paragraphs after the above quotation, he goes on to say,

> I have succeeded in proving these theorems [on the relationship between consistency and provability] at least for certain simple cases. I obtained this result by adding to the already given rules of inference (substitution and inference schema) *the following equally finite new rule of inference*:
> If it has been proved, for any given numeral $\mathfrak{z}$, that the formula
>
> $$\mathfrak{A}(\mathfrak{z})$$
>
> is always a correct numerical formula, then the formula
>
> $$(x)\mathfrak{A}(x)$$
>
> can be laid down as a starting formula.[75]

What is particularly striking about this comment is Hilbert's claim that the ω-rule is just as finite as the other rules in his system. It suggests the lengths that some were prepared to go in search of a consistency proof for mathematics. Seen in this light, then, Carnap's proposal to adopt a similar rule to Hilbert's is not out of step with some of the contemporary thinking. But this strategy has a very straightforward problem.

The first fact we need to observe, which was originally pointed out by Tarski, is that with the addition of an ω-rule to an existing language, $\mathcal{L}$, we are not using the same syntax anymore precisely because we have added a rule of inference.[76] So, strictly speaking, we are dealing with a new language, $\mathcal{L}_\omega$. While moving to this new language solves the incompleteness problem encountered in the *original* language, namely because it can now prove the sentence it could not prove before, no light is thereby shed on the status of the old language. That is, the notion of derivability characterized in $\mathcal{L}$ is still inadequate, and we will need new arguments to show that the notion of mathematical validity in $\mathcal{L}_\omega$ fulfills any requirements we might have. So, what are the prospects for such arguments?

[73] [Hilbert, 1931], p 1152.
[74] See [Hilbert, 1931], pp 1153 – 1154 for the rules presented at that lecture.
[75] [Hilbert, 1931], p 1154. My emphasis.
[76] See [Procházka, 2006], p 89.

Recall that the initial goal was to give a tractable and precise definition of mathematical validity. In this case 'tractability' amounts to being definite in Carnap's sense, and is what we would call 'effective' or 'computable' in modern terminology. However, the incompleteness theorems showed that while direct derivability was tractable in this way, it was not an adequate account of validity. When we augment the language with an ω-rule, and so move to the new language $\mathcal{L}_\omega$, we gain completeness back, and so have an account of mathematical validity, but this new notion of validity is no longer definite (effective/computable). Carnap is clearly aware of the specific deficiencies of the ω-rule approach,

> But, for our particular task, that of constructing a *complete criterion* of validity for mathematics, this procedure [of equating mathematical truth with derivability], which has hitherto been the only one attempted, is useless; we must endeavour to discover another way.
>
> 3. In order to attain completeness for our criterion we are thus forced to renounce definiteness, not only for the criterion itself but also for the individual steps of the deduction.[77]

So, rather than continuing to search for a way around the incompleteness phenomenon, Carnap instead sets out to show that despite the inability to represent mathematical truth as simple derivability, he can nonetheless define a notion which exactly characterizes it, namely analyticity. Of course, as he admits in the above quotation, this notion will be indefinite in the language in which it is given. In other words, he appears to bite the bullet on the consequences of incompleteness. This admission will have interesting consequences for Carnap's view, which we will take up after we discuss the definition of analyticity below.

Carnap's definition of analyticity is extremely complicated – the definition of it in Language II runs for twelve pages in *LSL* – and we need not dwell on the gory details here.[78] There is, however, one very important aspect of the definition that deserves our attention. The question that Carnap confronts at the end of section §34*d*, wherein the definition of analytic for Language II is finished, is whether or not the predicate '[...] is analytic in Language II' can be represented in a precisely formulated language, and in particular whether or not Language II can be that language. This is to say that Carnap is finally ready to tackle the problem he set himself at the outset of the book. He is now armed with a criterion of mathematical validity, which he has shown picks out all and only those sentences which are mathematically valid. However, both the definition of analyticty and the proof that it is complete with respect to classical mathematics are given, as Carnap puts it, "[...] in a word-language that does not possess a strictly determined syntax".[79] So, what remains to be

[77][Carnap, 1937], p 100. This passage comes in a larger discussion of various strategies for giving a criterion for mathematical validity, and spans the final sentences of the second strategy into the first sentences of the third (hence the floating numeral 3).

[78][Carnap, 1937], p 102 – 114.

[79][Carnap, 1937], p 113.

considered is whether or not analyticity can be formulated in a language with a precise syntax, and whether Language II can be that language.

After the long buildup, the immediate consideration of these two questions is unceremonious and brief. Carnap notes that it is obviously possible to specify a language with a strict syntax that can define "analytic in Language II"'. But, he goes on to note that for Gödellian reasons it is impossible to use Language II for this purpose, though he does not immediately spell the point out in detail. Instead, he returns to the problem in sections §59 through §60*d* where he shows that, as a modern reader would expect, were the predicates "analytic in Language II" and "contradictory in Langauge II" part of Language II itself, then it would be possible to construct sentences that say of themselves that they are contradictory. The final answer to the questions that Carnap posed at the outset of the book, namely whether it is possible to give a precise syntactical characterization of the notion of mathematical validity in a language which is also capable of serving as its own syntax-language, comes in two theorems in section §60*c*.

> **Theorem 60c.1.** If [a language] S is consistent, or, at least, non-contradictory, then '*analytic* (*in* S)' *is indefinable in* S.
>
> **Theorem 60c.2.** If [a language] S is consistent, or at least non-contradictory, then *no proof of the non-contradictoriness* or consistency *of* S *can be formulated in a syntax which uses only the means of expression which are available in* S.[80]

He goes on to note that this is true for a variety of terms that might be of interest as, for example, 'valid', 'consequence', and 'equipollent'; this point is close to what we now call Tarski's Theorem, though Carnap stops short of noting that truth is amongst the indefinable notions.[81] This is all to say that, while he has shown that it is possible to represent the syntax of a language with that very language, it is not possible to show that the language satisfies certain criteria while restricting oneself to only that language. Proofs of consistency or of completeness require languages with greater expressive power. As we will see in chapter 5 below, this lack of scruples about ascending to a more powerful language in order to prove fundamental properties of the language is a move that Carnap makes which several logicians and philosophers, most notably Gödel, think is illegitimate.

4.3 Conclusion

The aim of this chapter was to examine Carnap's view of logic and mathematics as he put it forward in his book *LSL*. Where in previous chapters we were concerned to examine those parts of other philosophers' views which Carnap adopted into his own, our motivation here was to highlight the parts of *LSL* that

[80] [Carnap, 1937], p 219. Original emphasis.

[81] [Carnap, 1937], p 219. The similarity between Carnap's work here and Tarski's Theorem has been noted several times. A characteristic example is found in [Gödel, 1987b], p 389.

were uniquely Carnap's. The largest of these unique features is the Principle of Tolerance, to which we paid significant attention. We first showed how an openness to different ways of thinking was instilled in the young Carnap by his mother, then carried into his years spent as a student. The connection to his early behavior was also important, though perhaps not decisive, for answering the challenge for priority in adopting Tolerance leveled by Karl Menger.

Much more decisive in that discussion were the differences in the work that the two philosophers took their respective principles to do. For Menger, the goal, we saw, was to simply brush aside metaphysical and foundational worries. For Carnap, by contrast, the goal was to give a foundation, though this was not intended to be a foundation in some ultimate sense, but only in a pragmatic one. That is, if we inquire into why the mathematical truths that our theory delivers as true are so, we find that it is an artifact of the way in which the language of our theory was constructed. Moreover, the Principle of Tolerance tells us that there can be no further question regarding the correctness of the theory, or its answerability to some extralinguistic realm.

We also endeavored to show that Carnap's adoption of the Principle was influenced by his ongoing debate over the nature of protocol sentences and construction of a language for science with Otto Neurath, and further by discussions of his position in that debate with Karl Popper. It was Popper's particular influence that turned Carnap's radical view that any sentence at all could serve as a protocol sentence under the right circumstances, to the more restrained view that we find in *LSL* and TM. Finally we turned to the view presented in *LSL* itself. We previously noted the influence of both Gödel, Tarski, and Behmann in turning Carnap's interests toward the possibility of metalogical investigations. In this chapter we saw this train of thought come to fruition in the investigations that Carnap undertakes with regard to constructing a tractable account of mathematical validity, and the possibility of representing the syntax of a language in that very language.

The discussion in this thesis so far has been centered on the genesis of Carnap's view in *LSL*. From here, however, our focus shifts from explaining the view, to answering a particular strategy of criticizing it. This critique, as we will see, has its origins in Gödel's posthumously published papers, though it has been put forward several times by other philosophers as well. We examine each of them in chapter 5 below. The extended discussion of the development of Carnap's views will play a decisive role in mounting a defense on his behalf of the Principle. This defense will be the subject of the final chapter.

Chapter 5

Gödel and the Arguments From Incompleteness

Up to this point in the thesis, we have been primarily engaged in an exegetical and historical project. The main concern has been to set out what Carnap's view, as it was presented in *LSL* was, and how he came to hold it. In this chapter, and for the remainder of the thesis, we shift gears somewhat, though perhaps it will not be so obvious from the outset. Our goal moves from establishing what it is that Carnap thought, to, in the first instance, examining a family of criticisms of his view, and later developing a response to those critiques.

Since the publication of the third volume of *The Collected Works of Kurt Gödel*, in which his developed but not previously published, papers are collected, there has been a simmering debate about the conclusiveness of the arguments that Gödel offers in his 1953/9 paper "Is Mathematics Syntax of Language?" against what he calls the 'syntactical viewpoint'. This view of the nature of mathematics, according to Gödel, was held around 1930 by Morritz Schlick, Hans Hahn, and Rudolf Carnap, and, "[. . .] can be characterized as being a combination of nominalism and conventionalism" with a strong commitment to empiricism as well.[1] Fundamentally, the debate centers on whether the attacks that Gödel launches reveal the incoherency of the syntactical view, or reveal a deep misunderstanding of it. As will emerge in our investigations, an issue that is central to this debate is the extent to which, if at all, Carnap appreciated the effects that Gödel's incompleteness results had for the view found in his book *LSL*, which we have discussed in section 4.2 above. In this chapter we will give an account of Gödel's arguments, and the ensuing debate surrounding it.

This chapter will be divided into two sections. In the first, I will give a brief account of Gödel's time in Vienna and his relationship with the Circle in general, and to Carnap in particular, while he was there. We will then move on to a sustained examination of his argument, paying special attention to the first section of the third version of Gödel's 1953/9 paper, wherein he gives the most complete version of the critique. We will finish this section with a slight

[1] [Gödel, 1995a], p 334. That Gödel does not attribute this view to the Circle at large is an interesting feature of the critique. We set it aside for now, but will take it up again below.

sharpening of the problem by casting it in the light of the issue of the applicability of mathematics in the sciences. The second section will be concerned with versions of this critique that have been made by other philosophers. There are four other versions that we will examine. The first of these is due to Hilary Putnam, and is found in his 1983 Herbert Spencer lecture "Philosophers and Human Understanding". The second version, which is the most recent, is due to Patricia Blanchette. She raises a problem based the applicability of our mathematical language and the supposedly content-free nature of mathematical statements on Carnap's view. The third is a more sustained attack that comes from Michael Potter's book *Reason's Nearest Kin* in his final chapter which is devoted to Carnap. The fourth is due to Gabriella Crocco in her article "Gödel, Carnap and the Fregean Heritage". This article takes a more neutral tone in the debate, but, as we will see, ultimately sides with Gödel.

5.1 Gödel, Incompeteness, and *Logical Syntax*

In this section we examine the work of Kurt Gödel and its relation to Carnap's view of logic and mathematics. The section will be split into two parts, one that reviews Gödel's life in Vienna, and his association with the Vienna Circle and with Carnap in particular. These details of his life are now well known, and so this section will be brief. In the second part, we move on to detail Gödel's posthumously published paper that critiques the view of logic and mathematics which was common in the Circle around 1930, entitled "Is Mathematics Syntax of Language?". Because this paper is chronologically the first, albeit not the first published, version of the general critique with which this thesis is concerned, the analysis of it will be substantial.[2] However, an evaluation of it, and indeed of the others examined in this chapter, will have to wait for chapter 6 below.

5.1.1 Gödel and Carnap: Two Gentlemen of Vienna

Gödel was born at Brno, Moravia, which at that time was known as Brünn and in the Austro-Hungarian Empire, in 1906. His family was relatively well to do, and as a result his upbringing was comfortable, though perhaps made slightly more difficult by his notorious propensity for illness.[3] In 1924 he went to the University of Vienna to study theoretical physics. However, he switched his area of study to mathematics around 1925 or 1926, after attending a lec-

[2]There is a small worry that, although this version is the first, it is nonetheless not the best. In part, I take this on board, and examine several other versions of the criticism in this chapter. However, because Gödel's version is a reference for nearly every subsequent version, I still think it warranted to give it pride of place.

[3]See [Wang, 1987], section §1.2 for Gödel's assessment of his family status. An anecdote that demonstrates their circumstances, however, is the difference between his study of Russell and Whitehead's *Principia* and Carnap's. In section 2.2.2 above, we remarked on the letter that Carnap sent to Russell asking for help in obtaining a copy of the book since he could not afford it. By contrast, when Gödel wanted to study it, he simply placed an order with the publisher ([Dawson Jr., 1997], p 53). With regards to his health issues, references to them can be found throughout the literature on his life. See [Wang, 1987], p 15 for a paradigmatic example.

ture series by Schlick on philosophy of mathematics, as well as a course by Friedrich Furtwängler on number theory, a course which he later said was the most wonderful he ever heard.[4] His decision to attend the lectures by Schlick was fortuitous, as there he made the acquaintence of Feigl and another student of Schlick's, Marcel Natikin. In late 1925 or early '26, he received an invitation to start attending the Vienna Circle's meetings, presumably at least in part because of his friendships with two of the younger members.

By his own account, Gödel's involvement with the Circle was always a bit uneasy. Though he was friends with some of the younger members, especially Feigl and Natkin, he also reports that he did not hold many of the mainstream Circle views. In a letter to Herbert Bohnert, written in 1974, Gödel says,

> I owe a great deal to the Vienna Circle. But it is solely the introduction to the problems and their literature.[5]

This comment goes to show the extent to which Gödel wanted to distance himself from the Circle later in life. Indeed, he claimed that he was a committed realist about mathematical entities as early as 1925.[6] He is also at pains to mention that, unlike most of the members of the Circle, Wittgenstein never had much influence on him. Despite this unease with their way of thinking, his name nonetheless appears in the list of members of the Circle in the 1929 manifesto. His relationship with the Circle grew more distant after 1930, however, and he reports that he stopped attending the meetings altogether in 1933.

From the perspective of our present inquiry, however, more interesting is his introduction to logic. In the winter semester of 1928/29, Gödel attended a lecture course offered by Carnap entitled 'The Philosophical Foundations of Arithmetic'. The two were friendly with each other, and there are 50 pages of records that Carnap kept of occasional conversations with Gödel beginning in 1928 and running through until 1948.[7] In the aforementioned letter to Bohnert, Gödel says that his conversations with Carnap were not very numerous, and that they were not likely very influential on Carnap's work in *LSL*.[8] As I argued in chapter 4 above, however, I think that Gödel is significantly understating his effect on Carnap's thinking. Additionally, this might be a further attempt on Gödel's part to make clear the differences between his own views and those of the Vienna Circle members. In his [Feigl, 1969], Feigl recalls that the younger members used to meet with Carnap to discuss matters of logic and mathematics very frequently, and that Gödel participated in these meetings.[9] Whatever the case may be about how influential these meetings were on Carnap, it is nonetheless true that Carnap was also a major early influence on Gödel. In a questionnaire that Gödel completed sometime around 1975, he notes that Carnap's lecture course was one of the most important influences in his becoming

[4] [Dawson Jr., 1997], p 24.

[5] [Gödel, 2003a], p 323.

[6] [Wang, 1987], p 22. Wang speculates that Gödel became convinced of his lifelong platonism by reading Plato as an undergraduate.

[7] [Wang, 1987], p 49.

[8] [Gödel, 2003a], p 322.

[9] [Feigl, 1969], p 640. See also [Dawson Jr., 1997], p 27.

interested in the questions of completeness and incompleteness.[10] The material covered in the course is almost certainly that taken from Carnap's textbook *Abriss der Logistik* (*Construction of a Logical System*), published just after the course finished in 1929, as well as some taken from Hilbert and Ackermann's *Grundzüge Der Theoretischen Logik* (*Principles of Mathematical Logic*), also published in 1929. This lecture course came at a critical time for Gödel as he was just about to begin writing his PhD on a highly related topic, namely the completeness of the first order fragment of the system from Russell and Whitehead's *PM*. Just how Gödel decided to write on this topic for his thesis is not clear, but perhaps a clue comes in a footnote in the introduction to it where he cites an unpublished manuscript of Carnap's on the subject.[11] In any event, it seems safe to say that the content of the lecture course, its timing, and the subsequent series of discussions between Gödel and Carnap make it highly plausible that the direction of influence was not only one way. Give their close association, Gödel would have been one of the people who knew Carnap's views best, other than Carnap himself. We turn now to the critique he wrote of those views.

5.1.2 Is Mathematics Syntax of Language?

Two versions of Kurt Gödel's 1953/9 paper, "Is Mathematics Syntax of Language?", are published in the *CW*, though it is known that at least six versions were drafted.[12] This paper was originally written as a contribution to the *Library of Living Philosophers* volume on *The Philosophy of Rudolf Carnap*. Gödel and Paul Schlipp, the volume's editor, had a long correspondence regarding whether or not the paper would be published, and if so, when it would be available for Carnap to read and respond to it.[13] Finally, in a letter dated February 3, 1959, Gödel sent word that he had decided not to publish the paper, saying,

> [. . .] I am extremely sorry that I cannot give an affermative answer to your inquiry of Jan. 24. In view of the fact that my article would ~~be~~ severely criticize some of Carnap's statements, it does not seem

[10]The questionnaire can be found in [Wang, 1987], pp 16 – 21. There, Gödel oddly characterizes the content of the lectures as 'metalogic' (p 17), but Wang notes that the title of the course in the university of Vienna's catalog is "The Philosophical Foundations of Arithmetic" (p 22), as noted above.

[11][Gödel, 1987a], p 63. The claim that Carnap is the source which directed Gödel to this as a thesis topic is contentious; see [Dawson Jr., 1997], p 54 for a discussion. The cited manuscript is almost certainly *Untersuchungen zur Allgemeinen Axiomatik* (*Investigations in General Axiomatics*), where Carnap sets out to prove a theorem he called the "Gabelbarkeitssatz" which says, in effect, that a theory is complete just in case it is categorical. Awodey and Carus translate the name of this theorem as the "forkability" theorem in their [Awodey and Carus, 2003]. The theorem is, of course, easily seen to be at least problematic: there are complete theories in first-order logic with infinite models (not all of which will be isomorphic), as Awodey and Carus point out (p 159). They go on to argue that it is not actually false as stated because its formulation is not precise enough to evaluate (see pp 158 – 161).

[12][Gödel, 1995a], p 324.

[13][Gödel, 1995a], p 324. The correspondence between Schilpp and Gödel regarding his contribution to this volume is substantially published in [Gödel, 2003b], pp 238 – 245.

> fair to publish it without a reply by Carnap.... The fact is that I have completed several different versions, but none of them satisfies me. It is easy to allege very weighty and striking arguments in favor of my views, but a complete elucidation of the situation turned out to be more difficult than I had anticipated, doubtless in consequence of the fact that the subject matter is closely related to, and in part identical with, one of the basic problems of philosophy, namely the question of the objective reality of concepts and their relations. On the other hand, ~~in view~~ because of widely held prejudices, it may do more harm than good to publish half done work.[14]

The volume was finally published in 1963, and Gödel's paper did not appear in it. However, he did keep six versions of the paper in his files. Two of these versions are reprinted in the Collected Works Volume 3, and two more are reprinted in *Kurt Gödel: Unpublished Philosophical Essays*.[15] To my knowledge, versions one and four have not appeared anywhere in print. In the remainder of this section, I will primarily focus on a close reading of the third version of the paper, with an eye towards presenting Gödel's criticisms of the conventionalist position and attempting to make clear the underlying assumptions of these criticisms. The third version is by far the most complete, and so restricting our attention to it serves to present the argument in the form closest to what might have appeared in the published version, were there to have ever been one. Roughly speaking, this third version is split into two sections. In the first, a critique of the syntactical viewpoint is offered while in the second, Gödel gives some positive arguments in favor of his own position. Since the current debate is centered on the extent to which the critique of the syntactical view is successful, I will be focusing on the first section of his paper.

Gödel begins the third version of his paper with a brieft characterization of the syntactical viewpoint. It reads:

> Its main objective, according to Hahn and Schlick, was to conciliate strict empiricism with the a priori certainty of mathematics. According to this conception (which, in the sequel, I shall call the syntactical 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.[16]

One might wonder who, exactly, Gödel takes himself to be characterizing here. In footnote nine he admits that it is likely that Carnap would not subscribe to this view at the time this paper was written in the 1950's.[17] There is a

[14] [Gödel, 2003b], p 244. Gödel's edits.

[15] Versions three and five appear in [Gödel, 1995a], while versions two and six are published in [Rodríguez-Consuegra, 1995].

[16] [Gödel, 1995a], p 335. Original emphasis. By 'sequel' Gödel means the rest of his essay.

[17] [Gödel, 1995a], pp 335 – 336.

substantive question as to whether or not Carnap or the other Logical Positivists mentioned at the beginning of the paper would have ever subscribed to it, and I will return to this subject in greater detail in the third section of the present chapter. At present, we grant Gödel that the Logical Positivists would have subscribed to this view for the sake of understanding his argument. In any case, according to Gödel the view is interesting for the technical advances that its adherents have made, and specifically mentions Carnap's *LSL* and a paper by F. P. Ramsey.[18] However, he also thinks that certain terms must be understood in a particular way in order for the view to be carried through. So, before he passes judgment about whether or not the view is a coherent way of understanding mathematics, he engages in a lengthy exploration of what he takes the view to be saying, and specifically explores the meanings of three key terms: 'mathematics', 'language', and 'syntax'.[19] We will now examine what is said about each of these in turn.

With regard to the meaning of 'mathematics' in the syntactical view, Gödel says that the main desideratum must be to maintain mathematics in as much as it can be used in natural science. That is, the Logical Positivist never wants it to be the case that some purely mathematical (or logical) statement implies the truth or falsity of some empirical statement, but they do want to be able to apply mathematics to the natural sciences so that they can derive consequences of the laws of nature which can later be verified.[20] So, Gödel says that what will have to be meant by 'mathematics' is the totality of classical mathematics. That is, the sort of mathematics needed to do science, as for example analysis or various sorts of algebra, and not more abstract branches of mathematics like set theory. The converse is also true, and so it can not be the case that some parts of mathematics are actually parts of the natural sciences. As Gödel puts it,

> Nor can certain parts of classical mathematics, such as the theory of the continuum, be discarded as belonging to physics. For they imply number-theoretical propositions without existential quantifiers, i.e., propositions whose purely mathematical character cannot be contested [...]. Therefore "mathematics" will have to mean classical mathematics.[21]

In other words, if some portion of mathematics was grounded empirically, then the separation between the purely formal sciences, mathematics and logic, and the factual or empirical ones would be lost. Putting things another way, if there were some empirical facts that ground certain mathematical ones, then that portion of mathematics would lose its character as free stipulations because they are entailed by the way the world is.

[18] [Gödel, 1995a], p 336. The Ramsey paper is [Ramsey, 1926].

[19] [Gödel, 1995a], p 337.

[20] [Gödel, 1995a], p 337.

[21] [Gödel, 1995a], p 337. He further claims that, though it is classical mathematics that must be understood here, nothing would be changed if one took the Positivist to be speaking about intuitionistic mathematics.

Another problem that Gödel points out for the syntactical viewpoint is that in order for mathematics to be applicable to natural science, then the theorems need to be known to be true. But, what does 'known to be true' mean in this context? He says that it is not the theorems themselves that are constitutive of mathematical truth, but rather their dervivability from certain axioms.[22] However, all that one gets from equating mathematical truth with derivability is an 'implicationistic' view of mathematics; Gödel cites the paper by Menger ([Menger, 1979c]), which we examined in section 4.2.2, as the inspiration for this view. That is, these theorems will be 'true' only in as much as they follow from the (assumed to be true) axioms. But, this kind of conditional truth will raise at least the question of whether the axioms are consistent. If they are, then the relationship between natural science and mathematics is safe. But, of course, if they are not, the syntactical view is in trouble, because all propositions will follow from contradictory axioms.[23] If every proposition follows, then mathematics loses its applicability in the sciences, because it will be impossible to know if the derivations of the consequences of the laws of nature that we do yield true results (because both the truth of the consequent and its falsity will follow from the axioms). Though he thinks it a very strong critique of the syntactical viewpoint, Gödel does not belabor the point in this part of the essay, and instead confines it to a footnote. However, we will return frequently to this thought, as it forms the core of what Gödel took himself to be doing. To sum up then, in the syntactical view 'mathematics' should be understood as classical mathematics and it must also be understood to be implicationistic.

The next of the terms that should be discussed is 'language'. Recall that the syntactical viewpoint, as Gödel characterized it above, is committed to a very 'strict empiricism'. In order that we are able to properly address 'language', we should say a bit about what a strict empiricism is in this context. Gödel says in footnote four to version three of the paper, "The tenet of empiricism in question evidently is that, in the last analysis, all knowledge is based on (internal or external) sense perceptions and that we do not possess an intuition into some realm of abstract mathematical objects".[24] In other words, the syntactical view is committed to a very traditional empiricism in the vein of Locke and Hume where all knowledge is from sense impressions or relations of ideas, and where we have no recourse whatever to anything but the five senses as a means of generating knowledge about the world.[25]

[22][Gödel, 1995a], p 338, footnote 13.

[23]This is on the assumption that the underlying framework is not paraconsistent, which seems a safe assumption given the historical context. However, there are reasons to suspect that Carnap was more open to investigations along paraconsistent lines (see [Carnap, 1937], p *xv* and section 6.1.1 below), though I acknowledge that phrasing things this way is somewhat anachronistic.

[24][Gödel, 1995a], p 335.

[25]This characterization is remarkably unfair to the Vienna Circle. While it is true that they were empiricists of some stripe, and even positivistic at some points, by the time of the debate over the form of the protocol sentences, at least Carnap and others had completely abandoned the kind of traditional empiricism that Gödel paints them with here. However, the footnote continues, "Since, moreover, because of the a priori certainty of mathematics, such a realm cannot be known empirically, it must not be assumed to exist at all. Therefore the objective of the syntactical program can also be stated thus: To build up mathematics as a system of

So, given this kind of empiricism, what can possibly be meant by 'language'? According to Gödel, it will have to be some kind of symbolic language, and moreover it must be a finite one.[26] It must be symbolic because all aspects of the language must be available to sensory perception. If it is infinite in any fashion, either in the quantity of symbols in the language or in the length of the sentences, then there will be (at least possible) sentences in the language that can not be completely produced in empirical sensation.[27] In the case of an infinitistic language, there will have to be some appeal either to a mathematical or linguistic 'intuition' in order to grasp the whole of the sentences of infinite length. But, this appeal to intuitions is problematic for the syntactical viewpoint in two ways. Firstly, it is not clear what this intuition might be. It is possible that it could be an alternative sense that functions roughly analogously to our normal sensory perception, but takes abstract objects as its target, or it could be something else entirely – that is, something that does not function in any way analogously to the five senses. However, positing a sense over and above the generally accepted five seems at odds with the strict empiricism discussed above. In either case it is the second way that this account of language is troublesome that is more damaging. If one can not perceive infinite sentences directly, since they can not be completely written down, but still takes these sentences to be meaningful and part of the language, then according to Gödel they must be a kind of abstract entity.[28] But this is exactly the sort of object that the syntactical viewpoint begins by rejecting! Indeed, if it is necessary to posit abstract entities to understand some of the sentences of mathematics, then it could certainly never be the case that these sentences were pure syntax without content or object as the view claims.

Gödel's discussion of 'syntax' closely follows the one on language. He says that in order to have an notion of syntax that will be acceptable to the Logical Positivist, there will have to be two constraints on it. Firstly, it must be the case, just as it was for language, that the syntactical rules are finitary. For just as we saw before, if they are not, then we are implicitly making use of a kind of mathematical intuition.[29] At first glance, this may not look like much of a worry, either for the way in which we understand language, or for the way in which we understand syntax. But part of the problem is that the syntactical viewpoint is not intended to be a revisionist stance about mathematics.[30] That is, the view is not that mathematical practice has been somehow in error and must be changed, but rather that it is our interpretation of what it is that

sentences valid independently of experience, without using mathematical intuition or referring to any mathematical objects or facts" ([Gödel, 1995a], p 335 fn 4). This latter description is more accurate, I think.

[26]There could be a worry here about ignoring other types of empirical intuition in favor of vision only. Gödel does not engage with this objection, and there is every reason to suspect that very similar objections to the ones Gödel makes would go through for other sense modalities.

[27][Gödel, 1995a], p 338.

[28][Gödel, 1995a], p 338.

[29][Gödel, 1995a], p 338.

[30]At any rate, it is not revisionist as Gödel presents it in this paper. Whether or not his characterization of the position is completely accurate or not is an issue that must wait until chapter 6.

mathematical statements are and how they are to be understood that has been problematic. However, one consequence of this is that the view must be able to account for mathematical statements of the form, "There exists an infinite set of expressions with a certain property" because these kinds of statements already occur in the corpus of mathematics, which the view is not trying to change.[31] But, as we saw above, it will be impossible to produce such a set of expressions in empirical intuition, and therefore impossible to claim knowledge about what properties its members do or do not have. If we can not account for our knowledge of all of the kinds of statements found in the mathematical corpus, then it seems like the account is lacking.

The second constraint that must be placed on our understanding of syntax is that all the syntactical rules must be known to be independent of any empirical observations purely on the basis of their formulation.[32] If it was not clear from their formulation then the rules would have some kind of content, namely that some extralinguistic considerations would confirm or disconfirm their applicability, both to the objects of mathematics, should there be any such, or to our calculations about the empirical world. One might think that those committed to strict empiricism in the manner of the syntactical viewpoint might welcome subjecting mathematics to a test of this kind to ensure that it is completely free of 'metaphysical' notions. But, we must bear in mind that, just as we noted above, the task that this view has set itself is not to revise mathematical practice, but to explain it. One of the features of mathematics as we know it is the necessary truth of its statements. If it were the case that some empirical observations or other extralinguistic considerations could disconfirm the supposed true theorems of mathematics, the necessary character of the truths would be gone. Instead the test for truth would be some contingent features of the empirical world as opposed to derivability from, or consistency with, a set of conventionally selected axioms. Given this, and making use of the same reasoning as he did in the section on the way in which mathematics must be understood, Gödel says that the requirement to maintain independence from any empirical claims is equivalent to a requirement for consistency.

Gödel says that there is another possibility for what could be meant by syntax. Up until this point in the present chapter, we have been gesturing towards this alternative understanding when we say that the syntactical viewpoint is not revisionist about the practice of mathematics, but it is necessary to get clearer about what exactly the consequences of these two features of the syntactical view are. If one does not want to revise the practice of mathematics, then one accepts all the theorems and methods that mathematicians have been using.[33] However, Gödel says that these are "[...] usually presented as a science of certain objects, about which certain propositions are asserted to be demonstrably

[31] This example is due to Gödel. See [Gödel, 1995a], p 338.

[32] The sense in which I mean 'formulation' to be interpreted is simply the order and kind of symbols used in writing the rule down.

[33] Though Gödel is not explicit on this point in [Gödel, 1995a], by 'methods' I understand him to mean the use of both infinitary inference rules as well as the full power of classical mathematics, as for example second order quantification.

true [...]".[34] So, since reducing mathematics to syntax is problematic, as we have just seen, perhaps it is possible to interpret mathematics as syntax of language instead. Gödel goes on to say that if one wants to interpret mathematics this way, then it,

> [...] will have to mean: (1) that the formal axioms and the procedures of proof of mathematics can be deduced from suitably chosen rules of syntax, and (2) that the conclusions as to ascertainable facts which are obtained by applying mathematical theorems and which formerly were based on the intuitive truth of the mathematical axioms can be justified by syntactical considerations.[35]

The main argumentative force here is on the second condition that the correctness of the syntactical rules can be itself known by the form of the rules. On Gödel's view, mathematical intuitions could ground the conviction that the axioms and methods of proof, as for example mathematical induction, used in mathematical practice are correct.[36] However, if one is committed to getting rid of all talk of intuitions, as the syntactical viewpoint and the Logical Positivists are, then of course one will have to look elsewhere for the guarantor of the acceptability of the syntactical rules. As ever, this acceptability consists in the rules not implying the truth or falsity of any empirical statements, which as we have seen before in the sections on how to understand mathematics and the section on how to understand syntax, is equivalent to the demand for a proof of the rules' consistency. However, Gödel also claims that interpreting mathematics as the syntax of language forces the Logical Positivist to make an even stronger claim, namely that, "only procedures of proof which cannot be rejected by anyone who knows how to apply these concepts at all" can be used in the derivation of mathematics.[37] As we saw earlier, the syntactical rules cannot have any content, and so if someone who knew how to apply these rules could find a reason to reject them, then the rules would would not be self-evident in virtue of their form. If they were not self-evident in virtue of their form, then they must have some kind of content which could serve as the basis of a principled rejection of the rules, and which will cause the rule to be unacceptable to the logical positivist.

At this juncture, it is worthwhile to take stock and review what exactly the syntactical view is, now that Gödel has filled in some of the ways in which key terms need to be understood. As we said above, it is a mixture of empiricism, nominalism, and conventionalism wherein mathematical statements are interpreted as the syntax of language. It is also a non–revisionist position that does not think that our mathematical practice has been in error, just our interpretation of what it is we are doing when we do mathematics. This mathematics

[34] [Gödel, 1995a], p 335, fn 6.

[35] [Gödel, 1995a], p 339.

[36] [Gödel, 1995a], p 340. In this case, by 'correctness', Gödel must mean something like 'when used in a mathematical context, are known not to take us from true premisses to false conclusions', or 'known to be consistent', or 'known to be true'. What he does not mean, however, is something like empirical adequacy. C.f. [Gödel, 1995a], p 341 fn 19.

[37] [Gödel, 1995a], p 341.

is taken to be classical, in that it will be required that all of the mathematics used in the sciences is covered by the syntactical account.[38] Because of the syntactical view's strong commitment to empiricism, both language and syntax are taken to be strings of symbols of finite length, as infinite strings cannot be produced in empirical perception. Some of these strings are specified as 'syntactic rules' which are the rules for determining which other strings follow from a given string. The syntactic rules are content-less but must be known to be correct. This knowledge can only come from examining the form of the rule, as it has no content wherein its correctness could be located. Another consequence of having no content is that the syntactical rules can not imply the truth or falsity of any empirical observations. This has the added benefit of preserving the traditional *a priori* certainty of mathematics. The similarly traditional applicability of mathematics to natural science is also preserved by the content–free nature of the rules. That is, because they have no content, they can be applied to any kinds of statements whatever, including the statements of the natural sciences. But, as we have seen, Gödel thinks that if this view is to be seriously entertained, then its adherents must produce a proof of the rules' consistency, and moreover, one that does not make use of any methods that would be rejected elsewhere in the view (e.g. no use can be made of infinitary methods in mathematics because they have elsewhere rejected the acceptability of constructions employing infinitely many occurrences of symbols (for example, in an ω-rule) because they can not be completely produced in empirical sensation).

What, then, can be said about the possibility of giving such a consistency proof? It would seem that if a proponent of the syntactical view could produce the required proof, then quite a few of the supposed problems would be resolved. With regard to this question, Gödel first observes that some very familiar restrictions are going to have to be in place to make sure that the proof is acceptable to the Logical Positivists. Namely, the proof must be finitary, both in the syntactical rules themselves, and also in the derivation of the axioms of mathematics from those rules.[39] There can be no reference made to any extralinguistic facts, to any sentences of infinite length or sentences whose referents it is not possible to produce in sensory intuition, and certainly not to any 'abstract' concepts.[40] Indeed, if any abstract concepts or infinitary methods are used, Gödel says that the program will be turned upside down,

> [...] instead of clarifying the meanings of the non–finitary math-

[38]The mathematics can be said to be classical (as opposed to intuitionsitic) as well, though Gödel suspects that his argument in the paper would go through even in it was understood as intuititionistic ([Gödel, 1995a], p 337.

[39][Gödel, 1995a], p 341.

[40]In footnote 20, Gödel give some examples of what he means by abstract and transfinite concepts. Those he lists as abstract are 'proof' and 'function', but says that they have to be understood a particular way, "[specifically in their] original "contensive" meaning, i.e., if "proof" does not mean a sequence of expressions satisfying certain formal conditions, but a sequence of thoughts convincing to a sound mind, and if "function" does not mean an expression of the formalism, but an understandable and precise rule associating mathematical objects with mathematical objects (in the simplest case integers with integers)" ([Gödel, 1995a], p 341 fn 20).

> ematical terms by explaining them in terms of syntactical rules, non–finitary terms are [used] in order to formulate the syntactical rules; and, instead of justifying the mathematical axioms by reducing them to syntactical rules, these axioms (or at least some of them) are necessary in order to justify the syntactical rules (as consistent).[41]

Put simply, the syntactical view must avoid infinitary methods and abstract objects on pain of circularity. It should be noted here, however, that the aim of Gödel's argument has shifted in this passage. He began by trying to explain the constraints that must be placed on any attempt at a consistency proof that would be acceptable to the syntactical view. But, in the passage quoted above, the focus has shifted to the possibility of *justifying* the syntactical rules. The issue of justification seems to be tightly connected with the possibility of a consistency proof in Gödel's mind. This is an important difference with at least some of the adherents of the syntactical view, and I will return to it in chapter 6 below.

With this limitation on consistency proofs established, Gödel then considers whether one could base such a proof on empirical induction. The procedure would have to be like this: first, one would establish some syntactical conventions from which once can derive mathematics. Having accomplished such a derivation, one starts using the mathematics so produced and, if no contradictions arise, then on this basis one concludes that the conventions one has chosen are consistent. Gödel suggests that this works roughly analogously to the determination of empirical laws of nature.[42] Indeed, he goes so far as to call it empirical induction because of the way in which the syntactical view must construe what is going in this procedure. That is, since the language must be finite strings of symbols, and the rules for the manipulation of these symbols must be known solely on the basis of their form in empirical perception, then it might be thought that this view had been dispatched earlier on in the section on syntax, though admittedly in a hasty fashion. However, Gödel is at great pains to spell out exactly why this strategy results in undesirable consequences for someone who is committed to the syntactical view in a much more detailed way this time by saying,

> Similarly, if on the basis of its empirically known consistency C, some mathematical convention R is added to some system S of mathematics and empirical science, then, though not C itself, still the empirical consistency C' of some slightly weaker convention will, in general, be demonstrable in $S + R$. This refutes the argument that the mathematical "conventions", although factual knowledge

[41] [Gödel, 1995a], p 342.

[42] [Gödel, 1995a], p 342. For example, we know that objects accelerate due to gravity at a rate of 9.8 meters per second squared. However, we know this (at least partially) on the basis of repeated observations. The suggestion here is similar – we know that the conventions we have chosen are consistent on the basis of repeated observations that no contradictions have thus far arisen from using the mathematics we derive from them.

about symbols may be necessary for setting them up, do not express or imply any facts.[43]

That is, if one attempts to justify some set of conventions on the basis of empirical induction, then one will be unable to claim that those conventions are contentless.[44] As we have noted above, if the conventions have some content then they will lose their *a priori* character, and so anything that is derived from them will be *a posteriori* as well. However, this is exactly what Gödel thinks must be the case for the conventionalist – any attempt to produce acceptable conventions will depend on the foreknowledge of some empirical facts (for example, at a minimum it must be known how to write down the symbols of the language). So, if the acceptable conventions must be based on some empirical knowledge, then they can only be taken to be *a priori* in that they add nothing new to this body of knowledge.[45] This in turn implies that any conventions that a Logical Positivist might make will only be conventional relative to a certain language or other body of knowledge. If they are only conventions relative to some body of knowledge, Gödel then notes that, considered apart from that body of knowledge the conventions will likely have some kind of content.

Having laboriously laid out what exactly the syntactical view must hold – namely that mathematics can be interpreted as the syntax of language only if language means a formal language composed from strings of symbols of finite length producible in empirical sensation and manipulated according to content–less syntactical rules that are self–evidently correct in that they imply neither the truth or falsity of any empirical statements – Gödel closes the first section of his paper with a discussion of why this position is untenable. He begins this final evaluation in footnote 19, where he says,

> I believe that what must be understood by "syntax", if the syntactical program is to serve its purpose, is exactly equivalent to Hilbert's "finitism", i.e., it consists of those concepts and reasoning, which are contained within the limits of "that which is directly given in sensual intuition" ("das unmittelbar anschaulich Gegebene"). Cf. Hilbert 1926, pp 171 – 173. The section of our knowledge thus defined is equivalent (by a one-to-one correspondence of its objects) with recursive number theory (cf. Hilbert and Bernays 1934, pp 20 – 34 and pp 307 – 346), except that it may rightly be argued (cf. Bernays 1935, p 61) that combinatorial objects with an exorbitant number of elements or an exorbitant number of operations to be performed must not occur in finitary considerations.[46]

This is to say that syntax, as it must be understood by the syntactical view, is finitary number theory. But, if we take it this way, and put this understanding

[43][Gödel, 1995a], p 342. It is quite hard to determine precisely what Gödel has in mind with this argument. One plausible reading is that by adding R to S we are able to derive *new empirical* consequences, in particular the empirical evidence for the consistency of C'.

[44]We will return to the issue of justifying the linguistic conventions on the basis of empirical induction in sections 5.2.3 and 6.2 below.

[45][Gödel, 1995a], p 342.

[46][Gödel, 1995a], p 341.

together with the other considerations mentioned above, then we arrive at a position that looks worryingly like Hilbert's Program, as Gödel claims. This equivalence between the finitist program and the syntactical view forms the sharp point of his criticism of the view. There are, however, two prongs to this critique. The first is what we have seen up until now – namely that the whole view rests on the possibility of giving a finitistically acceptable consistency proof. The argument can be summed in five steps:

1. If one wants to maintain the syntactical view, then one is in need of a consistency proof.
2. In order to give a consistency proof, one must use mathematics.
3. The mathematics that one must use must be derived from stipulated syntactical rules.
4. But, in order to derive mathematics for the consistency proof, the syntactical rules must be assumed to be consistent.[47]
5. However, this – now assumed – consistency is exactly what was set out to prove.

This is to say that attempts at an acceptable consistency proof will, according to Gödel, be viciously circular.

An aspect of this circularity argument that we have not yet explored is the impact of the second incompleteness result. Put very simply, the second incompleteness theorem shows that without appealing to a theory that is stronger than the theory for which we are trying to show consistency, it is impossible to finitistically prove the consistency of a theory that is rich enough to include primitive recursive arithmetic.[48] Since this is the case, then it will also be the case that we must make use of either (a) stronger axioms, or (b) make use of some infinitary methods in our consistency proof. Neither of these options will be acceptable for a logical positivist. Option (b) has been discussed at some length above. If one were to allow infinitary methods to be used, we will be making use of the content of the expression, which runs counter to the requirement that the syntactical conventions and the mathematical rules derived from them must be free of any content. In the case of (a), one will simply be begging the question. That is, if one uses stronger axioms to guarantee the consistency of the syntactical conventions, then one must also give an acceptable consistency proof for the new axioms. But, this second consistency proof will be subject to the same issues as the first was, and some appeal will have to be made to some even more powerful axioms to show that the second group of axioms is consistent. The picture one is left with is a kind of infinite hierarchy

[47] Since, as has been mentioned at several places above, if they are not consistent then every proposition follows, including the statement of the rules' consistency and its explicit negation.

[48] Gödel mentions that it is possible to be quite precise about what 'powerful' means with regard to a formal language, and offers "[...] It must be possible, by means of the concepts and axioms used in the consistency proof, to construct a 'model' for those proved consistent, i.e. to define the concepts demonstrably satisfying the given axioms and not satisfying any proposition disprovable from them" ([Gödel, 1995a], p 345) as an example.

of progressively more expressive languages where, as the hierarchy is generated by inventing new languages to give consistency proofs for less expressive ones, it will always be an open question whether the most expressive and most recently created language is consistent. With this picture in mind, it is easy to see that the best that can hoped for is a kind of conditional consistency where the starting conventions are consistent just in case some set of stronger axioms are. Moreover, this regress is vicious – at every level the same problem will reoccur. As we observed above, a kind of conditional certainty is not what the syntactical view is aiming at. This is finally the critique that Gödel has been aiming at: if one accepts the syntactical view then one can only have a kind of conditional certainty that one has a consistent system of mathematics, which is no certainty at all.

5.1.3 Who is Gödel's Target?

At a couple of places above we have noted that there is some ambiguity in precisely who Gödel takes his argument to target. If we are to evaluate its effectiveness, however, we must have a clear idea of what it is supposed to be effective *against*. There are two obvious candidates: (1) Carnap, and (2) an amalgamation of various members of the Vienna Circle that includes at least Carnap, Schlick, Hahn, Menger, and possibly others. It will be the aim of this short section to determine which of these options is most likely to have been Gödel's interlocutor. We will proceed primarily by examining the evidence from the various draft versions of the paper ([Gödel, 1995a], and [Gödel, 1995b]), though we also look at his comments in his 1951 Gibbs Lectures ([Gödel, 1995c]), written around the same time and on a similar theme as the draft papers.

At the very outset of the paper, as we noted above, Gödel says that the target of his critique is something he calls the 'syntactical view', and says that it was held by Schlick, Hahn, and Carnap. He says they held this view around 1930 under the influence of Wittgenstein.[49] Given this, it seems like the obvious answer to our question is option (2). Moreover, in an early footnote, Gödel says,

> I would like to say right here that Carnap today [i.e. in the 1950's] would hardly uphold the formulations I have quoted (cf. §45). Moreover some of them were only given by Hahn or Schlick, and probably would never have been subscribed to by Carnap. However, I am not concerned in this paper with a detailed evaluation of what Carnap has said about the subject, but rather my purpose is to discuss the relationship between syntax and mathematics from an angle which, I believe, has been neglected in the publications of the subject.[50]

It is clear from this passage that he intends his paper to address a general position, and that he is aware that it is possible that no individual ever subscribed to it. Indeed, he is at pains to point out that it is possible that Carnap in particular never did. True to his word, as we have seen above, he does not engage in any sustained exegesis of any of the philosophers who might have been the

[49] [Gödel, 1995a], p 334.

[50] [Gödel, 1995a], pp 335 – 336 fn 9.

subject of his critique. However, if we examine the second half of that paper, or the fifth draft, or even his Gibbs lectures, I argue that we find his true target becomes plain.

Through the first 43 sections of the paper, Gödel treats the syntactical viewpoint as if it is one that might have been held by any number of people. To wit, he cites several examples from various philosophers that seem to be in sympathy with it.[51] But, starting in §44, right at the end of the essay where Gödel is driving his point home, the focus turns exclusively to Carnap. To wit, though Carnap is not supposed to be the single target of the paper, he is nonetheless cited more than any other single author – in the third version of the paper Carnap has 13 citations, the next highest is a tie between Schlick and Hahn with 7 each. In the final sections of that version of the paper, from section §44 to the end, Carnap is referenced five times, and none of the other philosophers mentioned in the paper are referenced at all. In the fifth version, the situation is even more stark; there Carnap is the only philosopher that Gödel mentions, either by name or by citation. Additionally, Gödel explicitly asserts in both versions that *LSL* is the most developed exposition of the syntactical view. As he puts things in version three:

> In his *Logical Syntax of Language* ([Carnap, 1937], pages 102 – 129) Carnap has carried this program out.[52]

Version five is even more direct, as the very first sentence reads:

> It is well known that Carnap has carried through, in great detail, the conception that mathematics is syntax (or semantics) of language.[53]

It is only in version three that these statements are qualified somewhat, and that Gödel notes that there are other authors whose work falls under the syntactic viewpoint as well.[54] This could have gone some way towards softening the attack, but, as we see from the number of references, Carnap is still the real target: Ramsey is mentioned twice, Hilbert and his school three times, while Carnap, as noted above, is mentioned or cited 13 times. From these considerations, then, it seems safe to say that, despite his protestations to the contrary, Gödel is actually targeting Carnap, at least insofar as Gödel perceived his work to be the most complete version of the syntactical view.

5.2 Variations of Gödel's Critique

Since the publication of the third volume of the *CW*, a small cottage industry has sprung up attempting to sort out to what extent, if at all, the criticism

[51] For example, on page 335 there is a discussion of Hahn in footnote 8, on page 338 there is one of Menger in footnote 13, and on page 341 some of Hilberts views are treated in footnote 19. Of course, Carnap is discussed repeatedly throughout the paper; footnotes 9 and 14 are characteristic examples.

[52] [Gödel, 1995a], p 336.

[53] [Gödel, 1995b], p 356.

[54] [Gödel, 1995a], p 336. There he cites Ramsey as well as "much of the work of the Hilbert school".

that we examined in the first section of this chapter succeed in showing that the syntactical view of mathematics cannot be carried out as advertised. As we observed in the introduction there are, broadly speaking, two camps: (1) those who think that Gödel's critique is successful (at least in some respects), and (2) those who do not. The remainder of this chapter will be devoted to exploring the variations of Gödel's attack that have been put forward by members of the former group. We will look at four different versions. The first, due to Michael Potter, is very similar to Gödel's, though he argues that considerations around the usability of language militate against Carnap's view. The second comes from Gabriella Crocco, who argues that if mathematics is understood in the way that Carnap wants, it will not be possible to give an account of applied mathematics. The third criticism that we will examine is that due to Patricia Blanchette, who argues in a very similar fashion to Crocco that there is no possibility of applied mathematics on Carnap's view. The final variation we will examine is made by Hilary Putnam. He argues in a slightly different fashion from the others, and claims that Carnap's Tolerance is self-undermining.

5.2.1 Crocco's "Gödel, Carnap and the Fregean Heritage"

Gabriella Crocco's paper "Gödel, Carnap and the Fregean Heritage" is divided into two sections. The first, which will not otherwise concern us here, is a discussion of the extent to which both Gödel and Carnap can be located in the Fregean tradition. It is the second portion of Crocco's paper that we will spend some time examining. She begins by laying out the attitudes of both Carnap and Gödel with regard to foundationalism. For our current purposes, a foundationalist project with regards to mathematics will be one in which an attempt is made to give the basis for our belief in the theorems of mathematics. Crocco says that Gödel and Carnap have, "antithetical conceptions of the task of logical analysis".[55] That is, Gödel is, at least some of the time, engaged in a foundationalist type project, and Carnap is very explicit that he is not.[56] The problem then, as Crocco sees it, is to analyze whether or not Gödel is being uncharitable in his reading of Carnap or not; if Gödel is attempting to criticize Carnap by saying that he fails to give a foundation for mathematics when, in fact, he is not trying to give one, then the criticism is a kind of *non sequitur*.[57] In order to answer this new question, Crocco again divides her discussion into two parts. The first regards the form of Gödel's argument. For the sake of brevity, I will not engage very much with her considerations here, as we have seen the structure of the argument in some detail above. The only thing that might be added is to note that, in Crocco's view, Gödel divides his argument such that it can handle three formulations of the syntactical view. Roughly they are: (1) the position that Gödel claims that Schlick and Hahn held in the early 1930's – that mathematics could be reduced to language; (2) the view that Gödel attributes to Carnap in *LSL* – that the theorems of mathematics can be replaced by syntactic truths; and (3) the position that Gödel thinks

[55] [Crocco, 2003], p 31.
[56] See for example [Carnap, 1963], p 18.
[57] [Crocco, 2003], p 32.

Carnap held after his turn to semantics where the truths of mathematics are true in virtue of the semantic rules of the metalanguage.[58] This analysis of the form of the argument is likely correct, however the strategy that Gödel adopts with regards to each of the three formulations is essentially the same. Crocco herself does not see significant differences between them and addresses them all together.[59]

I will now turn to an examination of the more substantive second part of Crocco's analysis of whether or not Gödel's critique works. Crocco notes that, as we saw in the first section, Gödel set the conditions of coherence and finitism for any satisfactory explication of language and syntax the Logical Positivist might give.[60] She then moves on to discuss six more criteria that Gödel wants to add to the original two. Fundamentally, as Crocco sees it, the critique revolves around the perceived inability of a Logical Positivist to hold all eight criteria simultaneously,

> The six conditions which are listed and that could be subscribed to by a member of the Vienna Circle cannot be satisfied simultaneously and that suffices to refute linguistic reductionism because: "[...] only in that case [of the satisfaction of the six conditions] could a satisfactory **foundation** of mathematics be given independently of experience and without using mathematical intuition [...]".[61]

To be sure, such a demonstration that the Logical Positivist cannot hold all the necessary conditions for her position at one time would be very harmful for the viability of the view. But, it seems that one may legitimately wonder whether or not the inability to give a foundation is actually relevant, considering that the task that Crocco has set herself is to show that, "Gödel does not confine himself to repeating [...] the foundationalist dogma on the necessity of giving a foundation of justification of mathematics".[62] In order for the argument to be truly convincing, it should be the case that Gödel points out where the syntactical view fails on its own terms, that is not while trying to give a justification for mathematical knowledge and the applicability of that knowledge to the world. However, given the passage that Crocco pulls out to state Gödel's goal for the paper, it appears that he is not actually able to divorce himself from his own foundational concerns. Bluntly stated, it is a strange criticism of a view to say that it is deficient in some way because it did not give an

[58][Crocco, 2003], p 33.

[59][Crocco, 2003], p 35 – 37. She does not say this explicitly, but rather simply addresses the content of the arguments all together in section 2.4 of her paper.

[60][Crocco, 2003], p 33. It should also be noted that Crocco takes it for granted that the opponent for Gödel's essay is Carnap, where I have cast him as speaking about Logical Positivists more generally. For clarity, Gödel begins version 3 of his paper by speaking about Logical Positivists generally, and later (from §43 onwards) switches to addressing Carnap specifically. Version 5, by contrast, begins addressing Carnap and *LSL* directly in the first sentence.

[61][Crocco, 2003], p 33. The emphasis is Crocco's. The embedded quotation is from [Gödel, 1995a], p 343 (though Crocco inaccurately cites this as in volume II of the *CW*). For whatever reason, neither Gödel nor Crocco see fit to add the original two conditions of coherence and finitism in their sum of the supposedly mutually incompatible conditions, so the total that they give is six.

[62][Crocco, 2003], p 22.

account it was not trying to give. However, this is not what Crocco claims is happening in the paper. According to her, as the argument progresses, Gödel eliminates two of his conditions from consideration – the fourth, which is the condition that all syntactical sentence can be known to be consistent by their form only, and the fifth, which says that it should be the case that mathematics can be derived from specially chosen rules of syntax.[63] The effect of discarding these two conditions, according to Crocco, is to allow Gödel to engage with the syntactical view on its own terms. However, I think it will become clear as we move through her argument, that Crocco's account does not do enough to help Gödel.

So, if Crocco thinks that Gödel is not giving a foundationalist critique, what, according to her, is he doing? First, she claims that Gödel readily concedes that Carnap has managed to construct a system of rules whereby we can describe the practice of mathematics linguistically. But, she says, this replacement, if it is really going to be an accurate description or replacement of our previous practice, it must give the same predictive results as the old 'intuitive' mathematics would have, especially in the area of applied mathematics.[64] Otherwise we have not faithfully replaced the old intuitions with syntactical rules, but rather constructed a separate mathematical practice. Moreover, if we are going to use our newly constructed conventional mathematics to, for example, build bridges, we need to be able to trust its predictions. As Crocco puts it,

> In other words the linguistic explication of mathematics should not destroy our trust in the predictive power of mathematics. [. . .] Using the physical theory of elastic body [sic], which can be formulated only using a certain portion of mathematics, we can predict whether a certain bridge, constructed according to these laws, falls down or not. Any trust in these predictions would be unjustified if the rules which allow us to formulate these prediction were simple conventions without content.[65]

What she thinks that Gödel is after, then, is an account of how mathematics can be applied to the empirical world, and it is precisely this sort of account that the syntactical view is unable to provide. She goes on to say, "We have to justify the consistency of our rules just because we want to use them to make predictions. An explication which will not explain this trust will just be *pragmatically inefficient* because it would not explain the fruitfulness of our mathematics and its usefulness for us".[66] But, once again, we must ask whether this critique meets the syntactical view on its own ground. However, we will not answer this question now, but will take up the issue in chapter 6 below. Now, we turn our attention to a version of the problem developed by Michael Potter.

[63] [Crocco, 2003], p 34.
[64] [Crocco, 2003], p 35.
[65] [Crocco, 2003], p 35.
[66] [Crocco, 2003], p 35 – 36. My emphasis.

5.2.2 Potter's *Reason's Nearest Kin*

In his book *Reason's Nereast Kin*, Michael Potter addresses the position that Carnap takes in *LSL* in the next to last chapter. There he notes that Gödel's argument depends on a "language–independent notion of empirical fact" which Carnap would hardly accept, and so says that the argument appears to be a non–starter.[67] However, he goes on to argue that there is a way in which the critique can come back if we consider what is required if one is to be a speaker (or at least a competent user) of a Carnapian language.

The situation that Carnap found himself in after Gödel's incompleteness proofs, as we have seen, led him to abandon the use of an effective consequence relation in the languages he presents in *LSL*.[68] However, a non–effective consequence relation cannot be specified by a finite set of rules. So, in order for someone to be a competent user of a language with such a consequence relation, they must be able to grasp, and apply, an infinite set of rules. Against this, Potter first complains that any competence should be describable in finite terms, which will be impossible in the case of a language with an infinite set of rules. Moreover, he argues that even an appeal to an implicit grasp of the rules will not help a Carnapian account. As he puts it,

> The competence we ascribe to someone whom we credit with an implicit grasp of a finite set of rules is nonetheless a finite competence: it should be describable in finite terms. To say that the grasp is implicit is only to recognize that someone can be unaware of the exact limits of their own competence. If the competence in question cannot be finitely described at all, on the other hand, it is hard to see why it should be thought explanatory to describe it as a grasp of a set of rules at all.[69]

This is all to say that languages with an noneffective consequence relation appear to be impossible for finite beings – like humans – to use. However, this is not the deepest problem as Potter sees matters. The fundamental issue for him is whether Carnap's view permits an account of our grasp of linguistic meaning *at all.*

As he has set things up so far, Potter thinks Carnap has a fairly stark choice on the matter of his account of linguistic competence: either his languages have an noneffective consequence relation, in which case just how we come to grasp the rules of our language is unclear, or his languages have an effective consequence relation, in which case speakers' ability to say anything about the world

[67] See [Potter, 2002], p 270. This point was originally made by Thomas Ricketts and Warren Goldfarb in their [Goldfarb and Ricketts, 1992] and again in Ricketts' [Ricketts, 1994] (Potter notes only the latter). That such a notion helps Carnap, or that rejecting a language–transcendent notion of empirical facts is a good reading of Carnap's work has recently been challenged by Matti Eklund in his [Eklund, 2010].

[68] Potter characterizes this maneuver by Carnap as a constraint on the types of languages he is willing to consider ([Potter, 2002], p 274). As I will argue in chapter 6 below, this is not an absolute constraint, but is only true if the task one sets for one's language is to capture all of mathematics (obviously so, since otherwise incompleteness would not pose a threat). In this way, then, Carnap has not truly retreated from Tolernace.

[69] [Potter, 2002], pp 274 – 275.

becomes a matter of experimental fact. We have already discussed the first of these alternatives, and so will leave it aside; instead, we will focus on the second alternative here. First, we must remember that Carnap defines the content of a sentence to be the class of its non-analytic consequences. Next, we observe that, at least in the case of languages which have the principle of explosion, if a theory is inconsistent, then every sentence is an analytic consequence of every other sentence in the theory. This means that none of the sentences refer to the world at all, though they might have been expected to. It would seem, then, that what is needed, if we are to be sure that the theory has synthetic sentences in it, is a proof of its consistency. But, as we know, by Gödel's theorems we cannot have this proof (in languages strong enough to be interesting). This of course does not rule out the possibility that the theory is, in fact, consistent. What it does rule our, according to Potter, is that we can have anything over inductive certainty that it is. In other words, we can note that we have not yet encountered a paradox in our theory, and on that basis we can continue to act as if it is consistent, but this in no way tells us that we will never discover some inconsistency.[70] Potter puts the point this way,

> According to Carnap there must always remain a lurking doubt that my language might turn out inconsistent and hence not refer to the world, a doubt which on his view I have no more than inductive evidence to dispel. [...] Even if I am capable of concieving of things non-linguistically, I do not have any non-linguistic conception of the empirical distinct from and set against my linguistic conception of it. So Carnap's view makes it an experimental fact that I have a conception of an empirical world at all.[71]

To summarize, then, Potter thinks Carnap is trapped either way he goes. On the one hand, he could select a language with a non-effective consequence relation, which according to Tolerance he is perfectly entitled to do. However, if he does, then he cannot give an account of our grasp of languages. If, on the other hand, he selects a language with an effective consequence relation, again perfectly in line with Tolerance, he gives up the possibility of (1) giving a complete account of mathematics, and more than that also the possibility of ever having more than empirical evidence that the language is able to refer to the world. These consequences, Potter thinks, are devastating.

5.2.3 Blanchette and the Applicability of Carnapian Mathematics

In a recent manuscript, Patricia Blanchette has offered a similar problem for the Carnapian account of mathematics to that given by Gödel.[72] Blanchette notes that in Gödel's essay, there are three grades of consistency requirements for a framework in which to do mathematics:

[70] [Potter, 2002], pp 276 – 277.

[71] [Potter, 2002], p 277.

[72] Unfortunately, the manuscript itself was not available at the time of this writing. However, Blanchette's view was made clear in her talk [Blanchette, 2013], and she has generously given permission for using that as a basis for my discussion of her objection.

- The rules of the framework must be consistent, though we need not have a proof of this fact.
- We must have good reason to believe that the rules are consistent.
- We must have a proof of the rules' consistency.

We have already seen, in section 5.1 above, that it is impossible to satisfy the third requirement; this much just is the critique due to Gödel and so we leave it aside here.[73] But, what can be said of the prospects for the other two requirements? Answering this question is the focus of her manuscript. What Blanchette aims to show is that, by considering the applications of mathematics in making empirical predictions, the syntactical view cannot meet either of the other two consistency requirements either.[74] On that basis, she finally concludes that Gödel's arguments have shown a serious flaw in Carnap's position.

Before discussion Blanchette's view of the prospects of Carnap's position with respect to the two consistency requirements, we first pause to discuss the role of applied mathematics in the syntactical standpoint. In section §84 of *LSL*, Carnap says,

> But the task [of giving a foundation for mathematics] which is thus outlined is certainly not fulfilled by the construction of a logico-mathematical calculus alone. For 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. [...] A logical foundation of mathematics is only given when a system is built up which enables derivations of this [applied] kind to be made.[75]

Here, Carnap makes it plain that the applications of mathematics in the sciences are central to his view. As he formulates it here, a foundation for mathematics is not given at all unless the system allows for this kind of application. For the time being, we leave aside the details of how Carnap envisions the linkage between the pure and applied mathematics to be made in his frameworks. Instead, we focus,

[73]In her talk, Blanchette notes that there could be a question as to what sort of proof we must give. In his paper, Gödel argues that, by Carnap's own lights, the only acceptable proofs will be finitary ones, which are ruled out by the incompleteness theorems. Blanchette agrees, but notes that others in the literature, particularly Ricketts in his [Ricketts, 1994], and Awodey and Carus in their [Awodey and Carus, 2003], are not so willing to go along on this point. We return to this issue in chapter 6 below.

[74]The problematic nature of Carnap's views about mathematics and logic on the one hand, and his desire to apply mathematics in the sciences on the other has similarly been remarked on by Torsten Wilholt in his [Wilholt, 2006]. Unlike Blanchette, or indeed the picture of Gödel's critique that she presents in her paper, Wilholt claims that the problem of the applicability of mathematics in the sciences was one of the motivating forces for the early logicists, and, while Carnap claimed a kind of continuity with that tradition (as we argued in chapter 2) he nonetheless lost sight of the problem of applications during this phase of his thinking. For reasons that will become clear in chapter 6 below, I do not think this is correct.

[75][Carnap, 1937], p 326. Original emphasis.

as does Blanchette, on the relationship between this claim about applications and the need for consistency.

We begin our discussion with the first possible consistency requirement, namely that the rules of the framework be consistent. It is an appealing response to the problem posed by the incompleteness theorems to say that there is no need for a *proof* of consistency, only a need for consistency itself. However, Blanchette notes that it is key to the possibility of applying one's mathematics in the sciences that one can use the mathematics to make empirical predictions. That is, one must be able to say that a mathematical proof gives one a reason to expect certain empirical results to occur in testing. If one does not know, or at least have reason to believe, that one's theory is consistent, she argues, then one cannot take a mathematical result as a reason to expect any empirical occurrences. That is, without the additional knowledge that the system we are working in is consistent, why should we take the fact that a certain string of symbols follows from some other string(s) of symbols as evidence that, to take Gödel's example, a bridge will not fall down when put under a certain load?[76] Put bluntly, then, without at least evidence for believing that our framework is consistent, there is no possibility of making use of mathematics in the sciences. Blanchettte concludes, then, that if we are to take the applicability of mathematics seriously, that the first consistency requirement is too weak.

Having now shown that according to Blanchette Carnap's view cannot meet either the first or the third possible consistency requirements, we move to the last remaining one. The second potential requirement is that we have good reason to believe that our framework is consistent, even though we do not have a proof. But, absent a proof, Blanchette and Gödel argue that the only kind of reason we can have to believe that our framework is consistent is empirical induction. But, this would mean that the claim of consistency would be grounded in certain synthetic observations, as for example the fact that one has been using a system for some time and has not yet encountered a contradiction in it. This, in turn, completely undermines the claim that logic and mathematics are free stipulations. The freedom was supposed to be a result of the thought that no empirical facts could bear on mathematical facts. Now, however, a purely mathematical fact, namely the statement of the consistency of the framework, is held to be true on the basis of purely empirical considerations, that is on our experiences of working with a particular system. So, the second consistency requirement can not be met by the Carnapian either.

Blanchette concludes that, since none of the three consistency requirements can be satisfied by the syntactical viewpoint, that view has a serious defect. This defect is made especially pressing given the emphasis Carnap himself places on the applications of mathematics in resolving the apparent disagreement between formalists and logicists.

[76][Gödel, 1995a], p 340.

5.2.4 Putnam's "Philosophers and Human Understanding"

The final variant of Gödel's critique that we will examine is due to Hilary Putnam. It is found in the text of his Herbert Spencer lecture entitled "Philosophers and Human Understanding". After some brief introductory remarks, Putnam takes up the question of whether logical positivism is "self-refuting".[77] His primary line of attack is through trying to show that the VCM is self-refuting, and that it is central to logical positivism. As we noted in chapter 3 above, the Circle entertained several versions of the VCM, but in essence they all say that a statement is meaningful just in case it is either analytic or empirically verifiable. The version that Putnam gives in his lecture is as follows:

> A statement must either be (a) analytic (logically true, or logically false to be more precise) or (b) empirically testable, or (c) *nonsense*, i.e., not a real statement at all, but only a pseudo-statement.[78]

Against this criterion of meaningfulness, Putnam offers a quick argument (he calls it a "little gambit").[79] First, he invites us to wonder what the status of the VCM is. We note that it is certainly not analytic in any obvious sense of the word, and neither is it empirically verifiable. So, we conclude that by its own lights it must be meaningless. But, this seems to be worse than a contradiction – if it is meaningful then we can show that it is meaningless, and, on the other hand, if it is meaningless then it is meaningless (obviously). Either way, then, the VCM turns out meaningless.

The Vienna Circle was aware that there were problems with the VCM from the very start, as we saw above. This, in part, accounts for the large number of variations of the Criterion they considered. The version that will interest us here – the one that Putnam calls the most interesting – is that due to Carnap. Putnam's comments on Carnap are contained in a footnote towards the end of the section on the VCM. Since it is rather short, we quote it in full:

> The most interesting view was that of Carnap. According to Carnap, *all* rational reconstructions are proposals. The only factual questions concern the logical and empirical consequences of accepting this or that rational reconstruction. (Carnap compared the 'choice' of a rational reconstruction to the choice of an engine for an airplane.) The conclusion he drew was that in philosophy one should be tolerant of divergent rational reconstructions. However, this principle of tolerance, as Carnap called it, *presupposes* the verification principle. For the doctrine that no rational reconstruction is uniquely *correct* or corresponds to the way things 'really are', the doctrine that all 'external questions' are without cognitive sense, *is* just the verification principle. To apply the principle of tolerance to the verification principle itself would be circular.[80]

[77] [Putnam, 1983], pp 184 – 191.
[78] [Putnam, 1983], p 184.
[79] [Putnam, 1983], p 191.
[80] [Putnam, 1983], p 191.

This gambit is rather densely presented, so it is worth pausing to spell out just how it works.[81] One way that Carnap might have responded, and perhaps did in fact respond, to the argument that the VCM is meaningless presented above is to say that requirements of this kind, i.e. those which appear to restrict which languages are seen as legitimate, do not have the status of absolute restrictions but are rather proposals. Since these requirements are mere proposals, Carnap thought that we are free to adopt them or not, and it is this freedom, as we have noted several times, that is presented in the Principle of Tolerance. However, this freedom only makes sense, Putnam argues, if we can be certain that there is no factual way to adjudicate between the proposals. If there were some fact of the matter about which were correct, or tracked the way the world actually is, then we ought choose those principles and people who chose otherwise would simply be making wrong choices. Principles which restrict our choice of languages must be, strictly speaking, nonsense, albeit nonsense of a special kind. Put bluntly, then, if Carnap wants to maintain that the VCM is a proposal via an appeal to Tolerance, his appeal is tightly circular since Tolerance requires the VCM.

As Ricketts diagnoses matters, both Putnam's gambit and Gödel's critique rely on what Ricketts calls a "language-transcendent notion of empirical fact".[82] Part of the critical move in Gödel's arguments, as we saw above, is to note that it should not be the case that a purely mathematical sentence (or set of sentences) logically entails the truth or falsity of any empirical sentences. As Rickett's points out, this means that there must be some notion of what it is to be empirical apart from whatever line between the analytic and synthetic that is drawn by a particular language.[83] Otherwise the requirement would be trivially satisfied. Putnam argues that Carnap's tolerance, and indeed his whole view, is fundamentally an empiricist one. This empiricism must come before any language, on Putnam's reading, because otherwise there would be no need to appeal to the VCM to justify Tolerance. In a recent paper, Matti Eklund puts the point helpfully by saying,

> What language is adopted would appear only to matter to which propositions get expressed; the truth-values of propositions do not change. [...] What I can have is a sentence of my language, L1, 'BLAH', such that neither it nor its negation is true, and then I can make changes to my language, such that 'BLAH' of the language thus modified, L2, is true. [84]

In other words, languages can differ on which propositions are expressible in them. Whether a proposition has a truth-value at all, however, is not a matter that is settled at the level of *language*. Indeed, both Eklund and Putnam find it completely implausible that language has a role to play in assigning truth-values to propositions. But this just is the insistence that there must be some

[81] Here, I follow the discussion of this argument given at [Ricketts, 1994], p 178.

[82] [Ricketts, 1994], p 180.

[83] [Ricketts, 1994], p 180. We will return to this point in chapter 6 below.

[84] [Eklund, 2010], p 8.

fact of the matter beyond language about what truth-values propositions have, or which way the world is.

5.3 Conclusion

In this chapter we have examined several interrelated criticisms of Carnap's view. Very broadly construed, our task has been divided into two parts: in the first we directly examine the critique of Carnap, while in the second we analyze the strategy of the critique and argue that they all proceed in a similar manner. Most of the time was spent detailing the version due to Kurt Gödel. We saw how he played Carnap's empiricism off against his view that there is no uniquely correct logic (or mathematics) because these arose as the analytic portion of a freely stipulated language. In order to maintain a separation between the analytic and synthetic portions of the language, Gödel argued that the rules of the language must be known to be consistent; otherwise they would entail every sentence and so there would be no synthetic sentences at all. In the second version we examined, due to Patricia Blanchette, we examined a way to twist the claim that mathematics is just the analytic portion of the language the other way from Gödel's version. That is, we showed that on the assumption that one could have a purely syntactical and content-less account of mathematics after all, one could no longer give an account of the applications of mathematics in the natural sciences or in any other synthetic discourse. In short, the problem was that if mathematics is to be content-free, then it cannot refer to the world (or to any other domain of objects). But, any sentence of *applied* mathematics must so refer, otherwise it would not be applied! So, on the syntactical account, we are either forced to say that those contentful sentences are either not mathematical, which sounds very bad, or that there is no such thing as applied mathematics, which sounds even worse. The third version we examined was Hilary Putnam's. His 'gambit' purported to show that Carnap's Principle of Tolerance could not be maintained without an appeal to the Verification Criteria for Meaning. The VCM, he argued, was self undermining, and so Tolerance too is undermined.

In the second of the two projects from this chapter, we examined the variants of that critique. In the case of both Gödel's and Putnam's critiques, we argued that a notion of empirical fact that was independent of language was required. This notion will play a critical role in our assessment of the success of their attack on Carnap in chapter 6 below. In the case of Crocco, Potter, and Blanchette's versions, no such notion of extralinguistic fact is required. Instead, they all rely on considerations of what it might be like to apply, or to use, a Carnapian language to drive their worries. As I will argue at length in the next, and final chapter, all four of these arguments misunderstand, and fundamentally underestimate, the role that the Principle of Tolerance played in Carnap's thinking.

Chapter 6

On Tolerance

We begin this final chapter with a short review of what has happened so far. The first four chapters were concerned with spelling out Carnap's account of logic and mathematics in *LSL*, along with the historical background of how he came to hold that view. To that end, we examined two strains of influences: the Frege/Russell logicist tradition on the one hand, and the Wittgenstinnian early Circle on the other. In chapter five, we began the second part of the thesis, and examined a family of critiques of Carnap's view. There we showed that, while each had a slightly different focus, they nonetheless shared certain similarities. This chapter is the second half of that project. In it, we show how this family of critiques can be answered by Carnap. This answer will depend on a particular reading of the Principle of Tolerance, which distinguishes between two potential interpretations of the Principle – a 'wide' interpretation and a 'narrow' one – and argues that the wide interpretation is the one closest to Carnap's intent.

The chapter will proceed in two parts. In the first, I will set out my reading of the Principle of Tolerance. I will argue that the 'wide' reading should be interpreted as the correct one, and defend it from a revenge worry due to Michael Friedman. The second part will be focused on how wide Tolerance can answer the problems we posed in chapter 5 for Carnap's overall picture of logic and mathematics, with some time devoted to each of the critiques. Put briefly, my argument will be that each of these criticisms underestimates just how wide Carnap intended the Principle of Tolerance to be. At the end of the chapter, I will pause to give some reflections on why settling this debate about Carnap's conventionalism matters from a larger perspective.

6.1 The Principle of Tolerance

In this section, we return to the question of the proper interpretation of the Principle of Tolerance. In chapter 4 above we distinguished two versions of the Principle which we labeled 'wide' and 'narrow'. According to the narrow version, languages can be constructed in any way we see fit, but they are subject to certain high level restrictions. These might include a commitment to consistency, or to their being a meaningful distinction between the analytic and synthetic sentences in the language (e.g. that the analytic sentences are "true in virtue of meaning", or alternatively that neither of these categories can be empty), or the requirement that there be an (at least possible) user of the proposed language. As we will see below, this narrow interpretation of Carnap's Tolerance has been common, but is mistaken. The second, wider reading of the Principle is one whereby there are no constraints whatsoever on the construction of languages. One is just as free to propose inconsistent languages as consistent ones, and correspondingly with any of the other high level restrictions that a proponent of the narrow version of tolerance might favor. This is, obviously, an extremely radical thesis, and attributing it to Carnap will require careful argumentation. We will proceed by marshaling textual evidence from *LSL* to show that the wide interpretation of the Principle makes the best sense of what he says in the book. I will then turn to addressing the various critiques from chapter 5 above. We will show that, in each case, either wide Tolerance solves the problem, or that there was no problem to begin with in virtue of the wide reading of the Principle.

6.1.1 Tolerances: Wide and Narrow

In this section, we distinguish two readings of the Principle of Tolerance. The first, which I will argue has been the more common of the two, I call the 'narrow' reading. According to it, when Carnap says that there are "no morals in logic", he does not mean that all bets are off, so to speak.[1] Rather, he means that anyone is free to propose a formal system for mathematics, which is adequate for actually doing mathematics by the lights of certain very high level restrictions.[2] We will show that, despite the popularity of this reading, it is nonetheless mistaken.[3] Our strategy will focus on the textual evidence from *LSL*, and I will conclude from this evidence that Carnap is committed to the wide understanding of Tolerance.

As we have noted several times above, Carnap is often bold in his writing. Nowhere is this tendency more apparent than in his discussions of the Principle of Tolerance. While its formulation, given in section §17 of *LSL*, is dramatic, it is eclipsed entirely by what he says in the introduction. For example, he says,

> For language, in its mathematical form, can be constructed according to the preferences of any one of the points of view represented; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or other of the choices leads, *including the question of non-contradiction.*[4]

[1][Carnap, 1937], p 52.

[2]There is nothing special about taking mathematics as an example; in his introduction, Carnap says "The standpoint we have suggested [...] relates not only to mathematics, but to all questions of logic", and moreover, in the first section of the book proper, he says, "The method of syntax which will be developed in the following pages will not only prove useful in the logical analysis of scientific theories – it will also help in the *logical analysis of the word-languages*" (See [Carnap, 1937], p *xv* and p 8, respectively. Original emphasis).

[3]Examples of philosophers who have maintained, albeit mostly implicitly, the narrow reading include those mentioned with chapter 5 along with Michael Friedman, at least in his [Friedman, 2001], as I will show below.

[4][Carnap, 1937], p *xv*. The comment about "points of view represented" is in reference to

Here, there is an explicit denial that there are any constraints on language construction. It is a very strong statement indeed, as he even gives up consistency as a necessary condition on a language in order for it to be considered. Moreover, there is a denial that one must justify one's choices. The only thing that Carnap says is relevant to the evaluation of languages, at least here, is the consequences to which accepting various candidate languages leads. He is clearer on the point about justification just before the above quotation. He says,

> 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 deviations 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-problems 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 the construction for sentences and the rules of transformation (the latter are usually designated as "postulates" and "rules of inference") may be chosen quite arbitrarily.[5]

So, according to Carnap, there is no need for a proof of a proposed languages' adequacy. That is, we can suggest any language we wish for a task – "we have *complete* liberty in *every* respect" – and neither our proposed languages or our eventual choice need be provably 'correct', whatever the salient standard of correctness happens to be. The final bit of text we should look at to begin with is part of the statement of the principle itself, namely the comment that,

> *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.[6]

Though it comes a fair way into the book, at least in comparison to the quotations from the introduction, it is nonetheless of a piece with the tone of those early passages. What is key to our present task is again the emphasis on liberty and freedom from restrictions on which languages get proposed. Even the last clause – "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" – is phrased in terms of a task. That is, it is only in the case that one wants to discuss one's form of logic that this condition applies; even the need to give syntactical rules is not an absolute. However, we must be cautious

views in the foundations of mathematics. Later (in sections §16 and §16*a*) it becomes clear that he has intuitionism and Wittgenstein's views on identity in mathematics in mind.

[5] [Carnap, 1937], pp *xiv* – *xv*.

[6] [Carnap, 1937], p 52. Original emphasis.

against reading too much into the rhetoric of the introduction, and the early parts of the book. As has been noted before, it was a tendency in the Circle for their rhetoric to run ahead of their arguments or results. So, we now turn again to the details of Carnap's behavior in *LSL*. We will examine two episodes from the book: his treatment of intuitionism, and his consistency proof for Language II. These are all examples of the use that he makes of the Principle. In each case, we will examine a slightly different aspect of Carnap putting Tolerance to work.

The first example of Carnap's use of the Principle that we will examine is his discussion of intuitionism. This discussion comes in section §16 of the book, and runs for only a few pages (from midway down p 46 to midway down p 49). He begins by expressing frustration that no one has given a formal treatment of the intuitionistic view, and indeed that some of the intuitionists see the task of giving a formalism as unnecessary.[7] About this state of affairs, Carnap says,

> Once the fact is realized that all pros and cons of the Intuitionist discussions are concerned with the forms 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 or that way?".[8]

Of course, this "realization" just is the Principle of Tolerance applied to the debate between intuitionists and classical logicians. That is to say, since the intuitionists have not yet produced a formal treatment of their view, or at least not one that Carnap deems adequate for discussion, then for the purposes of determining which language should form the basis of our mathematical reasoning, or which logical principles can be accepted in our mathematical practice, we are free to make precise their claims in any way we think fit. Carnap puts the point this way,:

> It is in order to exclude [indirect proofs which lead] to an unlimited, non-constructive existential sentence that Brouwer renounces the so-called *Law of the 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 fulfills the fundamental conditions of Intuitionism in a simpler way than the form of language suggested by Brouwer (and partially carried out by Heyting).[9]

The way to adjudicate between the various proposals, just as the Principle says it should be, is by comparing their pragmatic features. In this case Carnap

[7] Carnap does note Heyting's book as an interesting first attempt, though does not have more to say about it than that at this stage of the book ([Carnap, 1937], p 46). When he takes the issue up again, however, he complains that Heyting's formalization is inadequate because the distinction between object-language and syntax-language is not drawn ([Carnap, 1937], pp 249 – 250).

[8] [Carnap, 1937], pp 46 – 47. Original emphasis.

[9] [Carnap, 1937], p 48. Original emphasis.

thinks that Language I achieves the same aims as the language that Heyting proposes as a formalization of Brouwer's philosophical view, but it does so in a simpler way. On that basis, and only on that basis, is it to be preferred. Returning to the question of the proper interpretation of the Principle of Tolerance, what stands out about Carnap's treatment of intuitionism in *LSL* is its early placement in the book. Though it comes before the formal statement of the Principle, it serves to set the stage for that statement.[10] His discussion is, in that way, a case study in how to *act* in accordance with Tolerance. That is, instead of engaging with debates over the nature of negation, or of whether quantification is restricted or not, instead he gives rules and argues for them on the basis of the consequences of their adoption. Another part of *LSL* that helps make clear the way in which Carnap understood the Principle is his purported consistency proof, which we examine next. However, before moving to the question of consistency, we pause briefly to discuss what it is, exactly, that Carnap is tolerant of.

As we saw above, Carnap's reconstruction of the intuitionist position is a drastic departure from anything that Brouwer would have accepted. In the first instance, Carnap's focus on formalizations in a language is antithetical to Brouwer's perspective.[11] Moreover, Carnap shows no hesitation in ignoring Brouwer's view:

> We hold that the problems dealt with by Intuitionism can be exactly formulated only by means of the construction of a calculus, and that all the non-formal discussions are to be regarded merely as more or less vague preliminaries to such a construction.[12]

This dismissal of non-formal discussion before the construction of a linguistic framework is at the heart of the Principle. It is what Carnap means when he says that one must "[...] give syntactical rules instead of philosophical arguments" in his statement of Tolerance.[13] In other words, what the Principle enjoins us to be tolerant of is precisely formulated languages which are proposed for adoption, and *not* any philosophical justifications for those proposals. As we noted above, in the absence of a proposed formal language, these philosophical considerations will be completely superfluous on Carnap's view, because it will not be clear what position they support. Carnap shows this implicitly by producing a linguistic framework, namely Language I, which he claims captures the spirit of intuitionism, despite its obvious departures from the philosophical claims that Brouwer makes.[14] With this in mind, we now turn to Carnap's discussions of consistency.

Carnap addresses the question of consistency several times in *LSL*. The first two times, as we have already seen, are in the introduction. There, he dismisses

[10] In the fist edition, the statement of the Principle comes in the very next section. For the second edition, Carnap inserted a short part, section §16*a*, on Wittgenstein's theory of identity.

[11] See [Mancosu, 1998], p 2.

[12] [Carnap, 1937], p 46.

[13] [Carnap, 1937], p 52.

[14] For example, the Law of the Excluded middle is valid in I, while rejected by Brouwer. See [Carnap, 1937], p 48 and p 34 Theorem 13.2.

the worry that allowing a language to represent its own syntax will result in contradiction as well as the demand that a proposed language be consistent in order to be considered for adoption. Despite this early dismissal, Carnap nonetheless offers a proof of the consistency of Language II (in §34*i*). Additionally, he remarks again on the issue in the section of the book on "General Syntax" (§59). We will largely constrain our discussion to the earlier section §34*i* because the two sections are very similar, though we will comment somewhat on §59 as well.

The proof that Carnap gives for Language II is somewhat laborious, and we will not be particularly concerned to cover the details. For our purposes, it will suffice to summarize the strategy. In broad strokes, what Carnap shows is that every demonstrable (in his idiom, provable in ours) sentence of II is analytic. Crucially, as we saw in chapter 4 above, the converse is not true, though he does not give a proof of this fact at this stage of the book.[15] Rather, at this point he confines himself to showing that there is a single sentence, $\neg(0 = 0)$, that is not demonstrable in II.[16] It then follows that no two sentences of the forms S_1 and $\neg S_1$ are demonstrable in II, for if they were then so would $\neg\mathfrak{N}$. What is more interesting than the details of this proof from our perspective is Carnap's comments after the proof on the relationship between the result he has and Hilbert's program. After a few remarks to the effect that his term 'definite syntactical concepts' is approximately equivalent to Hilbert's 'proof with finite means', he says,

> Whether with such a restriction [to the use of only definite syntactical concepts in a consistency proof for classical mathematics], or anything like it, Hilbert's aim can be achieved at all, must be regarded as at best very doubtful in view of Gödel's researches on the subject (see §36). [. . .] The proof 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. [. . .] Even if [our proof] 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.*[17]

[15]This becomes obvious only later in section §36, where he gives his version of Gödel's proof. See [Carnap, 1937], pp 131 – 134.

[16]The sentences $(0 = 0)$ and $\neg(0 = 0)$ play a special role in *LSL*. To mark this special status, Carnap gives them the special symbols $\mathfrak{N}$ and $\neg\mathfrak{N}$ respectively. Every sentence in II can, by means of the rules of reduction, be transformed into one or the other of them (see [Carnap, 1937], p 102.). There is a sense in which they are akin to the True and the False in Frege's thinking in that every true sentence (or analytic sentence in II since the terms will coincide) is reduced to $\mathfrak{N}$, while every false sentence (or contradictory in II) is reduced to $\neg\mathfrak{N}$.

[17][Carnap, 1937], p 129. My emphasis.

Though he does not make the connection explicit here, the last sentence of this quotation is critical to our understanding of Tolerance. One of the tasks that Carnap set himself was to show that all of classical mathematics – that is analysis – can be represented in Language II, and that it does not contain any contradictions. This he has managed to do, as the proof shows. However, as he comments in the quotation, in order to effect the proof he has had to make use of resources that go beyond those available in II, namely the concepts 'analytic-in-II' and 'consequence-in-II' which are both indefinite in II as we noted in chapter 4.[18] In the proof, however, these concepts are used in the metalanguage where they may very well be definite.[19] What attracts our attention now, however, is both his frank admission of this fact, as well as his total lack of apparent concern over the effect that it has on the status of the proof. Were we to have concerns about whether or not the proof is good, then we are free to construct a third language which is capable of serving as a metalanguage for the metalanguage for II, and then to carry out a similar proof of the consistency of II's metalanguage in it. This will have the same character as our original consistency proof, namely that it does not guarantee us against paradox in the theory in which we conduct the proof. What must lay behind Carnap's rather *laissez faire* attitude towards consistency proofs is, I claim, wide Tolerance.

The Principle of Tolerance, on either the wide or the narrow interpretation, already licenses Carnap's dismissal of foundational concerns. That is, the worry that our proof does not guarantee absolute consistency, but only consistency relative to some other theory, has no bite for him because leaving aside such demands for absolutes is just what the Principle enjoins us to do. However, what we see in the above quotation is not simply Carnap's anti-foundationalism, but the thorough going nature of his understanding of Tolerance. Were he to intend the Principle to be construed narrowly, then instead of giving a consistency proof in the straightforward manner that he does we would expect him to abandon his efforts entirely, since the proof is, at best, question begging. However, since he does not give up, we should conclude that the narrow reading cannot be the intended one. Recall that, for Carnap, statements about which language to choose can only be evaluated relative to a stated goal. So, if our goal is to show that our object-language is consistent, then we need to construct a syntax-language with the appropriate resources (as, for example, being able to express the concept "analytic-in-I"). This point generalizes, and choice of task will have consequences for the way in which we should construct the syntax-language. Carnap puts the point directly himself in section §45:

> Our attitude towards the question of indefinite terms conforms to the principle of tolerance; in constructing a language we can either exclude such terms (as we have done in Language I) or admit them (as in Language II). It is a matter to be decided by convention.

[18] In fact, a result of Gentzen's, proved a few years after the publication of *LSL*, showed that talk of the 'strength' of a language is rather imprecise. See [Gentzen, 1936] where he gives a strength measure by the least ordinal needed for induction in the language.

[19] Of course, the metalanguage for II (which we may as well call III) will have corresponding concepts which are indefinite in *it*. We know, for example, that the concept 'analytic-in-III' will be indefinite in III for reasons analogous to those we gave for II in chapter 4.

> If we admit indefinite terms, then strict attention must be paid to the distinction between them and the definite terms; especially when it is a question of resolubility. Now this holds equally for the terms of syntax. [...] Some important terms of the syntax of transformations are, however, indefinite (in general) [...]. *If we wish to introduce these [indefinite] terms also, we must use an indefinite syntax-language (such as Language II).*[20]

So, as with the object-language, there are no morals at the meta-linguistic level either. That is, we are free to construct our syntax-language in any way we see fit, with the recognition that some ways of doing so will fare 'better' than others for particular tasks.

So, as we have seen, Carnap not only intended for Tolerance to apply at the object level, but at the level of syntax-languages as well. We conclude our discussion of wide Tolerance by noting Carnap's closing comments in the introduction to *LSL*,

> 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 possibilities.[21]

Here, again, there is no mention of any constraints on Tolerance. Instead, there is a bold declaration that the obstacle of correctness has been 'overcome'.[22] So, we see that the ocean of possibilities is boundless in two ways: both at the level of choices of object-language and at the level of the choice of syntax-language. Before we can use this understanding of Tolerance, which I dubbed the wide reading above, to resolve the criticisms discussed in chapter 5, however, we must first address the worry that this very understanding itself undermines Carnap's view.

6.1.2 Metalevel Tolerance and Friedman's Revenge

In his 1991 paper "Tolerance and Analyticty in Carnap's Philosophy of Mathematics", Michael Friedman develops a reading of Tolerance that is in many ways similar to the interpretation I gave in the previous section. On his interpretation of the Principle, there are no constraints on the proposing of a language. We are just as free to accept a language in which our background logic is constructivist as we are to accept one with a classical logic in the background. However, the difference between our interpretations becomes stark when one considers how we might choose between proposed languages. Carnap tells us that, where before we might have philosophical arguments that one language correctly captures the nature of a concept (as, for example, logical consequence), when we

[20] [Carnap, 1937], pp 165 – 166. My emphasis. Friedman makes a similar point in his [Friedman, 2001], p 227

[21] [Carnap, 1937], p *xv*.

[22] This is the same word – 'überwindung' in the German original – that Carnap uses with regard to metaphysics in the title of "OM".

adopt the Tolerant standpoint we see that these debates are vexed, and that our task really consists in determining the consequences of adopting the proposed languages. Once we have determined what these consequences are, we are in a position to make a purely pragmatic choice between the proposals.[23] However, things are not so easy when we get down to the details of what 'determining the consequences of adopting a proposed language' amounts to. In this subsection, we take up Friedman's worry. We will show that it does not damage Carnap's position as long as he is understood in the way I have suggested.

Let us suppose that one wants to follow Carnap's advice and determine which of these two logico-mathematical systems are best for formalizing the language of science, intuitionist or classical. Presumably, in order to establish an answer to this question, we will have to construct a suitable syntax-language which can express all the necessary concepts for *both* languages; that is, we will need a neutral place to stand so that we can judge the consequences of each of the proposals. However, as Friedman notes,

> In giving a metatheoretical description of [the language of classical mathematics], we therefore need a metalanguage even stronger than the language of classical mathematics itself (containing, in effect, classical mathematics plus a truth-definition for classical mathematics). And we need this strong metalanguage, not to prove the consistency of the classical linguistic framework in question, but to simply describe and define this framework in the first place so that questions about the consequences of adopting it (including the question of consistency) can then be systematically investigated.[24]

That is, for all the reasons we have noted above, and in chapter 3, the syntax-language for the language of classical mathematics will have to include the resources to define concepts like "analytic-in-$\mathcal{L}$" and "consequence-in-$\mathcal{L}$" (where $\mathcal{L}$ is the language in question). This means, however, that the metalanguage we construct to judge between the intuitionist proposal and the classical one – to wit, the supposedly neutral syntax-language – is committed to resources that the intuitionist would reject. So, the goal of stepping back from the first order dispute over which logic to use into a metalanguage from which we can investigate the consequences of adopting each proposal without prejudging the issue appears doomed. In Friedman's words,

> In order to apply the principle of tolerance, we must view [the] choice [between an intuitionist language and a classical one] as a purely pragmatic decision about "linguistic forms" having no ontological implications about "facts" or "objects" in the world. [. . .] Accordingly, we must view the logico-mathematical rules in question, in both linguistic frameworks, as sets of purely analytic sentences. Given Carnap's own explication of the distinctions between logical and descriptive terms, analytic and synthetic sentences, however, we must have already adopted the classical logico-mathematical rules in

[23]For a characteristic example, see [Carnap, 1937], pp 46 – 47.
[24][Friedman, 2001], pp 242 – 243.

> the metalanguage. Thus, to understand the choice between classical and intuitionistic logico-mathematical rules in accordance with the principle of tolerance, we must have already built the former logico-mathematical rules into our background syntactic metaframework. We must have already biased the choice against the intuitionist in the very way we set up the problem.[25]

Because of the inability to neutrally frame the decision between classical and intuitionistic mathematics, Friedman concludes that the principle of tolerance is self undermining in this case. There is an obvious route of escape for Carnap, which we briefly consider next, before turning to my own solution.

A possibility that Friedman suggests might be available to Carnap is to adopt a weaker, intuitionistically acceptable metalanguage, for example one that excludes unlimited universal quantification, instead of the full-blooded classical one we used above. A bit further on in his paper he considers this option, but ultimately, and correctly in my view, finds it lacking as well. While it will allow for the exploration of the consequences of adopting the two competing frameworks without prejudice to one over the other, what he says it will not do is allow for a "[...] sharp contrast between *merely* pragmatic questions of 'linguistic form' having no ontological import, on the one side, and genuine theoretical claims, on the other".[26] To see why this is the case, it is helpful to recall the reason we adopted the full strength of classical mathematics in the metalanguage in the first place, namely to be able to make use of indefinite notions like analyticity. These notions are what serve to mark the distinction between the genuine theoretical questions, and those which are settled by convention. So, if we adopt a metalanguage which is too weak to characterize such notions, then while we might get the intuitionist back into the game, so to speak, we lose the ability demarcate the boundaries of the field of play.

Friedman thinks that the tension his argument reveals in Carnap's Tolerance is ultimately fatal to the program in *LSL*.[27] However, I contend that he has not applied the Principle correctly in the argument we have just laid out. In particular, he has neglected to note a shift in the salient task that is critical to seeing that Tolerance is in no way undermined by the use of intuitionistically unacceptable resources in the metalanguage. Recall that for Carnap languages are evaluated relative to a particular task. So, in the paradigm case, if our concern is to give a logical foundation for classical mathematics, then we ought pick a language which embraces non-constructive proof techniques. Conversely, if we are concerned to ensure that we can decide for each numerical predicate

[25] [Friedman, 2001], pp 242 – 243.

[26] [Friedman, 2001], p 244. Original emphasis.

[27] [Friedman, 2001], p 244. Additionally, Friedman notes that Carnap's championing of formal logical methodology in philosophy led, rather tragically, to the widespread opinion that his overall philosophical project – the transformation of philosophical problems into purely pragmatic choices in language planning – is a failure. Though we disagree over the effect that (at least some of) the criticisms leveled at Carnap's view have on it, about which more will be said in section 6.2 below, I agree with Friedman that Carnap is widely dismissed in virtue of them in contemporary analytic philosophy. This is an issue which I will return to in section 6.3 below.

whether it applies to a given number or not, then we ought pick a language which is constructed along the lines of Carnap's Language I. Returning to the case at hand, that is, to deciding between classical and intuitionist mathematics, what we will need to do is to construct a language that allows us to investigate the consequences of adopting each proposed language for mathematics without prejudging the issue. The metalanguage so constructed need not be the same one in which we might, for example, investigate whether the object-theories are consistent. This is because the task at hand has changed. As we noted above, Tolerance runs all the way up the linguistic hierarchy on the wide reading, and therefore so does task relativity. Friedman's argument assumes that there needs to be a single metalanguage, chosen once and for all; for him, it not only serves as a place to stand when considering questions about a particular language, it is also supposed to simultaneously be a perspective from which we adjudicate all disputes over the form that a language should take. But, this insistence on a single metalanguage for these disparate tasks is a kind of absolutism that is at odds with Tolerance.[28]

There is another way in which Friedman's argument does not hit its mark. As he points out, there must be a single metalanguage from which we prove facts about our object-language(s), as for example their completeness or their consistency. This metalanguage must have certain resources in order for the proofs to go through, as we have noted several times. We then use the results of this investigation of these languages in order to come to a determination of which of the two to adopt as the language of our first-order mathematical theorizing. Friedman claims that this prejudices the decision of which object-language to adopt because we have made essential use of resources in the metalanguage that the proponent of intuitionism must not accept as valid. It is this last step that is problematic from Carnap's perspective; it relies on a notion of validity *simpliciter* which he rejects. On Carnap's picture, inferences are only valid (or invalid) relative to a particular language, and so the fact that one treats an inference as valid in one language does not entail that one must treat it as valid in every language. Though this point is obvious in the case of different object-languages, as the case at hand of classical and intuitionistic logics illustrates, it is somewhat more subtle when examining the case of an object-language and its metalanguage. However, Carnap's view is that metalanguages are constructed in just the same way as object-languages are. This means, as we emphasized above, that just as with object-languages, the validity of inferences in metalanguages are language relative. So, accepting certain inferences for the task of investigating the consequences of adopting a language does not thereby commit one to the unlimited validity of those inferences. That is, the intuitionistically inclined Carnapian can still entertain the notion that inferences like double negation elimination and unrestricted universal quantification are invalid in our mathematical reasoning quite independently of accepting them for investigating the consequences of rejecting those inferences on our mathematical practice.

In this section, we have developed a reading of Carnap's Tolerance which we dubbed the 'wide' reading. According to it, as I argued, there are no constraints

[28] Creath makes a similar point in his [Creath, 1991].

whatsoever on the construction of languages, and therefore no constraints on what languages we can consider as potential languages to adopt for formalizing some practice. We showed that this wide reading is the most plausible way to understand Carnap's intended meaning for the Principle of Tolerance. Finally, we turned our attention to a worry raised by Michael Friedman, namely that Tolerance is self-undermining due to the prejudicial nature of the metalinguistic considerations necessary to assess proposed linguistic frameworks. However, I argued that this worry misses its mark because it fails to appreciate just how through going Carnap's Tolerance really is, to wit, in precisely the way that the wide reading makes clear.[29] It was essential to detail the failure of this line of criticism of my reading of Carnap before moving to the main critiques because otherwise my answer would have simply reintroduced the problem, albeit slightly farther up the linguistic hierarchy. With this problem out of the way, however, we return our attention in the remainder of this chapter to the other criticisms of Tolerance which were raised in chapter 5 above.

6.2 Arguments from Incompleteness Revisited

Throughout this thesis, up until the previous section, I have claimed that a particular reading of the Principle of Tolerance can help Carnap avoid certain criticisms of his view. In the section just above, I developed the 'wide' reading of Tolerance, and argued that it is the most faithful to Carnap's intent. In this section, I make good on the other half of the promise, and it will be our task to make clear, in the case of each of the criticisms considered in chapter 5 above, how the wide reading of Tolerance does the work of forestalling the apparent problems. There will be, broadly speaking, two strategies employed. The first strategy will be to show that in virtue of the way the Principle is written, the objection is simply based in a misunderstanding of Carnap. The second will mirror the approach I took above with Friedman's revenge-style objection, that is, I will point out that the objector has failed to read the Principle of Tolerance in the wide way, and detail the way in which reading it in that fashion prevents the objection from hitting its target. We will conclude, then, that Carnap's position is safe from worries centered on the Principle.

6.2.1 Potter, Crocco, Blanchette, and the Applicability Objection

In their two critiques of Carnap, which we examined in sections 5.2.1 and 5.2.3, we saw that Crocco and Blanchette both think that considerations which run along the lines that Gödel advanced show that mathematics, if conceived of in Carnap's fashion, cannot be applied. Before we move to addressing this problem head on, we will first give a brief reminder of Carnap's view of mathematics. Then, we will note why the failure of applications of mathematics would be a problem for Carnap's view. Finally, we will argue that this problem is based

[29] A similar response to an earlier work of Friedman's ([Friedman, 1988]) was made by Devidi and Solomon in their [Devidi and Solomon, 1995].

in a misunderstanding of Carnap's view, which will be brought out by some of his comments on the supposed opposition between logicism and formalism.

As Carnap casts matters in the *IA*, one of the principles he took from his study of Frege is that both logic and mathematics (or, logic in the narrower and in the wider senses, respectively) are analytic.[30] It arises as a kind of 'formal auxiliary' of the rules we lay down for the language in question. As Carnap puts the point in his essay "Formal and Factual Science",

> All of logic including mathematics, considered from the point of view of the total language [of science], is thus no more than an auxiliary calculus for dealing with synthetic statements. *Formal science* has no independent significance, but is an auxiliary component [of the total language] introduced for technical reasons in order to facilitate linguistic transformations in the *factual sciences*.[31]

What Carnap means by saying that logic is merely a 'formal auxiliary' is simply that it arises as a kind of byproduct of the rules of a language. But, as we saw, the problem with this view arises when we consider just how the transformation of factual sentences is supposed to happen. The worry, put bluntly, is that if all of mathematics is supposed to be analytic, then there cannot be any *applied* mathematics, since, at least on the view that Crocco and Blanchette offer, what it is to be applied is to have some synthetic component. By Carnap's lights, if there is a synthetic component to a sentence, then the whole of the sentence is synthetic, and would therefore not be mathematical, properly speaking.[32] But if, as we have just seen, the purpose of including the formal sciences in the total language is to facilitate our factual sciences, as Carnap claims in the quotation above, then it seems that his position is untenable. The solution to this worry is found in the way he treats the relationship between the purely logical portion of the language, and the total language.

In section §84 of *LSL*, entitled "The Problem of the Foundation of Mathematics", Carnap discusses the debate between logicists and formalists. As he diagnoses it, the disagreement centers on whether or not the mathematical foundation needs to give an account of the meaning of mathematical talk in every context, including applied ones.[33] From the point of view of our present inquiry, that is into whether or not there can be such a thing as applied mathematics on the Carnapian account, this debate appears especially pressing. Carnap's

[30]For the distinction between the two senses of logic, see [Carnap, 1953a], pp 124 – 125. It is worth reiterating that, while Frege thought that it was only logic and arithmetic that were analytic, Carnap followed Russell in thinking it was all of mathematics – i.e. including geometry – that could be reduced to logic and so were analytic.

[31][Carnap, 1953a], p 127.

[32]This follows almost directly from Carnap's definition of content, and from his Theorem 34g.5 in *LSL*. See [Carnap, 1937], p 120.

[33]Oddly, the example of the applied inference that Carnap uses, which is to conclude that "In this room now there are two people present" on the basis of the sentence "Charles and Peter are in the room now and no one else", is invalid; we would additionally need to know that Charles and Peter are distinct people ([Carnap, 1937], p 326. This example is particularly amusing in Carnap's case since his good friend Hempel was known as both Carl and Peter). While this is an odd oversight, I do not think that it diminishes his point.

solution is typically tolerant. He claims that the logicists and the formalists are simply talking past each other, and that when their claims are suitably clarified they are seen to be compatible:

> The logicist requirement only appears to be in contradiction with the formalist one; this apparent antithesis arises as a result of the ordinary formulation in the material mode of speech, namely, "an interpretation for mathematics must be given in order that it may be applied to reality". [...] The *requirement of logicism* is then formulated [in the formal mode of speech] in this way: *the task of the logical foundation of mathematics is not fulfilled by a metamathematics* (*that is, by a syntax of mathematics*) *alone, but only by a syntax of the total language, which contains both logico-mathematical and synthetic sentences.*[34]

This is Carnap's overall strategy for overcoming the problem of applications, but it does not yet tell us just how this strategy is to be put into practice. On the previous page, however, there is a hint of what he has in mind. There he says that some of the rules of formation for a language must, if we are to have the possibility of applying our mathematics, set out how to use mathematical symbols in synthetic sentences.[35] Of course, it is possible on Carnap's view to have a language where one cannot do any applied mathematics because it lacks this type of rule. Their inclusion or exclusion is just one more choice we must make when setting out a language. To sum up then, the objection is based in a misunderstanding of the view, namely one whereby it is that thought that it is constitutive of what it is to be mathematics *at all* to have no content. But this is just a misreading combined with a kind of dogma about absolute definitions. Once the dogma is removed, however, we see that the objection is essentially terminological. The question is just whether to include synthetic sentences which contain mathematical symbols in 'mathematics' or not. But this is precisely the kind of decision which Carnap thinks has to be made by conventional agreement, and does not constitute a flaw in the view. However, there is a further problem that arises when considering the possibility of applying the Carnapian picture which we now turn to, namely that due to Michael Potter.

As we saw in section 5.2.2, Potter has argued that given the way that Carnap sets up his linguistic frameworks, he runs into dire consequences in two ways. In chapter 4 we detailed the route that Carnap took to get around the issues that the discovery of the incompleteness phenomenon might have had for his view, namely abandoning the use of a recursive consequence relation in his languages. Potter argues that if one does this, then while one can regain provable completeness, it is done at the cost of its graspability by speakers. That is, in cases where the consequence relation is not effective, it appears mysterious how any finite beings could ever come to use such a language. If, instead, Carnap uses a recursive consequence relation, he lands squarely on the other

[34][Carnap, 1937], p 327. Original emphasis.
[35][Carnap, 1937], p 326.

horn of Potter's dilemma. There, as we saw, the fact that a particular linguistic framework has any synthetic sentences at all becomes a matter of experimental fact, about which we could be mistaken. This is due to the absence, and indeed impossibility of a consistency proof, without which we have nothing better than inductive assurance that the linguistic framework we use is consistent. If it is inconsistent, then *every sentence* of the language will be analytic, as we saw, because they will all follow from the rules of the framework alone. Moreover, if a sentence is analytic, then it does not refer to the world. So, if we can have only inductive certainty, at best, as to whether or not every sentence in a particular language is analytic, then we can have only inductive certainty that the sentences in that language refer to the world. In the remainder of this subsection, we tackle the two horns of this dilemma, each in turn.

While it is certainly true that Carnap gives up using a recursive consequence relation, it is far from obvious that this has the dire consequences that Potter claims it does for speakers' grasp of a language. In order to see precisely why, it will be instructive to remind ourselves of the claims in the central passage of this portion of his critique. He says,

> The competence we ascribe to someone whom we credit with an implicit grasp of a finite set of rules is nonetheless a finite competence: it should be describable in finite terms. To say that the grasp is implicit is only to recognize that someone can be unaware of the exact limits of their own competence. If the competence in question cannot be finitely described at all, on the other hand, it is hard to see why it should be thought explanatory to describe it as a grasp of a set of rules at all.[36]

However, this description of Carnap's position misstates it badly. What Carnap gives us by characterizing a consequence relation for a linguistic framework is not an account of what it is that speakers of that language grasp, which would be an exercise for psychology, but rather a theoretical *description* of meaning in the language at hand. That is, it would be just confused to characterize Carnap's position as giving a list of rules that one must grasp in order to cognize a particular language. Rather, what he is doing is giving a theoretical reconstruction of meaning (in this case understood as synthetic consequences) in that language in terms of rules, some of which are infinitary. So much, then, for the first horn of the dilemma; what can be said about the other?

On the second horn of Potter's dilemma, we see that in the absence of a consistency proof we can have no more than inductive confidence that our language refers to the world. That is, in the case where a language turns out to be inconsistent, every sentence is analytic, and so has no content. So, Potter concludes, we must always be suspicious that our language fails to refer, no matter how successful we think we have been in the past.[37] However, this representation of how reference is supposed to work on Carnap's account is not accurate. Recall, as we presented it in chapter 4, Carnap's view is that

[36] [Potter, 2002], pp 274 – 275.

[37] See the passage quoted in section 5.2.2 above, or at [Potter, 2002], p 277.

certain sentences called ‘protocol sentences’ are the ones which refer to the world, and they do so in virtue of their testability. However, a sentence is deemed a protocol sentence because of the logical form it has, not because of its content. In Carnap’s words,

> 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.[38]

So, we see clearly that content plays no role whatsoever in determining whether a given sentence is a protocol sentence or not. The rules for determining the form of the protocol sentences are just as freely chosen as all the other rules of the language. This means that whether a particular sentence is accepted by a community of speakers or not – that is, whether they agree to the rules of a particular language or not – is what determines whether that sentence refers. As Carnap puts the point,

> There is in the strict sense no refutation (falsification) of an hypothesis; for even when it proves to be L-incompatible with certain protocol-sentences, there always exists the possibility of maintaining the hypothesis and renouncing acknowledgment of the protocol-sentences.[39]

So, let us return the case that Potter is worried about, namely where we are unable to give a consistency proof for our language. In the case where the language is consistent, then reference to the world is safe and unproblematic. If the language is inconsistent, and the speakers of the language know it, then they know that, while it might have appeared otherwise in the past, every sentence of the language has the same (empty) content, and so they are unable to say anything. Nonetheless, there can still be ‘contact with the world’, as Potter puts it, because what that amounts to is simply acceptance or rejection of protocol sentences by a linguistic community. Since sentences are protocol sentences because of the particular form they have, then even if the language is inconsistent there will still be sentences with the right form, and so there is still at least the possibility of acceptance.[40] In the case where the language is inconsistent, and where the speakers of this language are *unaware* of this fact, and so every sentence is analytic, there can still be reference. Some sentences of the language will have the form of protocol sentences, and even though they are analytic, they can still be either accepted or rejected by speakers of the language.[41] In this way, then, the lack of a consistency proof need not

[38] [Carnap, 1937], p 317. Original emphasis.

[39] [Carnap, 1937], p 318.

[40] Rejection, however, is a more difficult problem because every sentence of the language will be provably L-true, and so there will be some difficulty in giving a rationale for ever rejecting a proposed protocol sentence.

[41] This is still a somewhat strange picture. For example, it entails that in this latter case, speakers of a language are confused about the meaning of their words. To them, it appears as if the sentences they utter have some content, when in fact they do not since they are all analytic. While this state of affairs is odd, it is nonetheless the view, and the worry that Potter has raised does not affect it in the way that he claims.

undermine our confidence in our ability to refer to the world on Carnap's view. Before moving on to consider the final two criticisms of Carnap's position, what remains to be done in this subsection is to spell out the way in which Tolerance, and in particular the wide reading of the Principle, underlies the answers I have given on Carnap's behalf so far.

The applications of Tolerance in the reply I gave to Potter's critique are relatively straightforward, and so we will not belabor them overly much. In the first horn of his dilemma, and even granting that languages with non-recursive consequence relations are not cognizable by finite beings like us, we noted that Carnap's project was not to give a psychologically adequate description of speakers' grasp of languages. Rather, we said it was to provide a theoretical account of meaning, namely as sets of non-analytic consequences. Because the account is purely theoretical, it need not be constrained in the ways that Potter claims it should. Indeed, the lack of antecedent constraints is precisely the wide reading of Tolerance. The story is similar with the other horn of the supposed dilemma: it is precisely Tolerance that underlies Carnap's view on protocol sentences, as we saw in chapter 3. What is distinctively wide about the use made of the Principle in the response to Potter is somewhat removed from the discussion of applications and content, however. It is the move made at the beginning to a consequence relation that is non-effective. As I argued above, in section 6.1.1, the freedom to use whichever consequence relation seems best is part and parcel of the wide reading. So, summing up, the wide reading of Tolerance, along with some reflection on the role of protocol sentences, allows Carnap to sidestep the worries over applicability, and over reference. We turn now to the critiques based in either circularity, or self-refutation.

6.2.2 Gödel and Putnam, Verificationism and Incompleteness

In this section, our goal will be to answer the remaining two criticisms of Carnap's view that we discussed in chapter 5. The response will have four parts. In the first, we will give a brief overview of the two arguments. The second part will argue that, while these two arguments might look quite different, they rely on the same faulty assumption, and we will show how this assumption is undermined by wide Tolerance. In the third section, we will consider a rejoinder, due to Matti Eklund, to the solution offered in section two. In the final part, we will argue that Eklund's position not only has not made any progress in showing a weakness in Carnap's view, but also runs roughshod over the very heart of that view. We will conclude, as we did in the previous section, that if we understand Carnap as having employed the wide reading of Tolerance, then his view is unaffected by the various arguments that have been deployed against it.

In section 5.2.4, we set out the first of the criticisms we will examine in this section, namely that made by Hilary Putnam in a footnote to his lecture "Philosophers and Human Understanding". There he argued that if Carnap was to maintain the Principle of Tolerance, then he had to assume the VCM in the background. If not, then it was possible for there to be facts of the matter concerning which language we should adopt. However, the VCM was supposedly a free postulation made on the basis of Tolerance; that is, it was a stipulation that, according to Putnam, Carnap laid down as a preference, not as an absolute requirement. So, if Putnam is right, then the justifications for both the VCM and the Principle are viciously circular. The second critique of Carnap's view that we will take up in this section is Gödel's. In his 1953 draft paper, he argued that if the truths of logic and mathematics are supposed to be analytic and content-less, then they cannot entail any synthetic sentences (as that is what it is to have content on Carnap's picture). This demand amounts, however, to the demand for the truths of logic to be consistent, for if they were not then they would entail everything, and in particular they would entail some synthetic sentences. But, by the incompleteness theorems, there is no possibility of giving a proof of a language's consistency in that very language in the case of languages strong enough to be interesting.

On the surface, these two criticisms are rather different. One appears to concern the justification for the Principle of Tolerance, while the other seems to regard the claim that mathematics, and logic, are analytic and content-less. However, an assumption that is common to both of these arguments is that there is a notion of an empirical fact that stands apart from any linguistic framework. In his paper "Carnap's Principle of Tolerance, Empiricism, and Conventionalism", Ricketts makes this point quite clearly, at least insofar as concerns Gödel's argument:

> Gödel observes that the conventionalist will not be justified in taking the mathematics formalizable in some language to be a contentless auxiliary for science, unless the rules of the language can be shown to be admissible. Gödel's definition of admissibility employs a language-transcendent notion of empirical fact or empirical truth [...] He urges that we are not justified in taking the analytic sentences of a Carnapian language to be conventionally stipulated truths only if the premises needed to establish admissibility are available in advance of the stipulation. This explanatory task arises only in the context of a language-transcendent notion of empirical fact.[42]

To see that Putnam's argument uses the same (unstated) premise takes some work. As Ricketts goes on to argue in his paper, however, the central issue is the way in which Putnam frames the problem. Recall the essential bit of the footnote:

> For the doctrine that no rational reconstruction is uniquely *correct* or corresponds to the way things 'really are', the doctrine that all 'external questions' are without cognitive sense, *is* just the verification principle.[43]

The problematic phrase is "the way things really are". If Putnam's argument is to have any bite against Carnap, then the meaning of this phrase must be

[42][Ricketts, 1994], p 180.

[43][Putnam, 1983], p 191. Original emphasis.

clear. But, this is precisely what Carnap will deny; instead, he argues that it is exactly terms of this sort that are in need of precisification, and moreover that this precisification can only occur by means of a precisely specified linguistic framework. In other words, the claim that Putnam is committed to, namely that there is an unproblematic notion of the way things are apart from a linguistic framework is exactly that language-transcendent notion of empirical fact that Ricketts pointed to. It would seem, then, that both Gödel and Putnam have failed to engage with Carnap on his own terms. However, in a recent paper, which we gestured at in section 5.2.4 above, Matti Eklund has argued that Ricketts' solution is too quick, and it is to this rejoinder that we now turn.

In his paper "Multitude, Tolerance and Language-Transcendence", Eklund argues that Ricketts own notion of language-transcendence is itself in need clarification. Moreover, he argues that there is no way to spell it out such that it at once satisfies a Carnapian view at the same time as being consistent with the way we conceptualize the relationship between languages and the world in contemporary philosophical theorizing. As Eklund puts it,

> If it is a [thesis to the effect that "there is no fact of the matter as to "] that Goldfarb and Ricketts have in mind [. . .] [then] their talk of "language-transcendence" obscures matters. Saying that there is no language-transcendent fact of the matter, as opposed to saying merely that there is no fact of the matter, suggests that although there is no fact of the matter as to what is an empirical fact, *relative to a given language* there is a fact of the matter. But that seems just confused. What language is adopted would appear only to matter to which propositions get expressed; the truth-values of propositions do not change.[44]

Eklund has in mind an interpretation of Carnap like this. Some propositions are true and some false given the way the world is. There is no proposition, however, which says which language should be adopted. This means that we are free to adopt whichever language we see fit. What is different between these languages is what expressive resources they have. So, some languages will have the resources to express more propositions than others will. So, correspondingly, if we adopt a different language, we may have the capacity to express more (or fewer) propositions than we could previously. What will not change, however, is the truth-value of these propositions. On Eklund's interpretation, they do not depend on language for their truth, but rather on the world. However, while this would certainly be a strange reading of Carnap, or possibly itself confused, what it certainly is not is a rejoinder to Ricketts, or, for that matter, to Carnap. All that Eklund has apparently said is that propositions, or sentences, are true or false depending on how the world is. But this is precisely what Carnap, and Ricketts on his behalf, have denied! That is, in order to ensure that a change in

[44] [Eklund, 2010], p 8. Original emphasis. He goes on to say that if we should feel squeamish about propositions-talk, then the entire argument can be reformulated to only use sentence-talk. However, I do not think this small point should bother us insofar as the present inquiry is concerned.

language did not effect a chance in the truth-values of propositions, their truth (or falsity) must be determined by factors that go beyond a particular linguistic framework – to put matters bluntly, there must be a language-transcendent notion of empirical fact in play. So, just as with Putnam and Gödel, Eklund has failed to engage with Carnap, and with Ricketts, on their own terms.

In this chapter, we have examined the criticisms of Carnap's view that were developed in chapter 5. Key to our examination was a particular understanding of the Principle of Tolerance which we called the 'wide' interpretation. This reading of the Principle holds that there are absolutely no constraints on which linguistic frameworks may be proposed, and that there is no fact of the matter about which one must be chosen. There are, of course, pragmatic considerations, as for example consistency or completeness, but these can only be weighed relative to a particular goal. We then showed how each of the criticisms we had previously laid out failed to take adequate account of the freedom this reading of Tolerance ensures. In the case of the worries about the applicability of mathematics, we showed that the objections boiled down to a terminological problem, namely whether synthetic sentences with mathematical symbols were to be counted as mathematical or not. The solution, as we argued, is precisely in Tolerance. That is, it is a matter of decision whether to include sentences like this or not, and the matter is to be decided by investigating the consequences of each approach. We moved from considering worries based in the applications of mathematics, to those whose aim was to show that the Principle is self-refuting, or incoherent. Following Ricketts, we diagnosed these criticisms as relying on a notion of empirical fact that was independent of a linguistic framework, or a "language-transcendent" notion in Ricketts' phrase. However, this notion is not only incompatible with the Carnapian picture, it demonstrates a certain kind of failure to engage properly with that picture. With that, the main philosophical portion of this thesis comes to a close. In what remains, we return to certain reflections begun in the introduction.

6.3 Conclusion

This thesis, broadly speaking, was divided into two parts. In the first, we examined the development of Carnap's philosophical views on logic and mathematics until the publication of *LSL*, or shortly thereafter. We began with the influence of the logicist tradition on his thinking. This came primarily through his interactions with both Frege, from whom Carnap first learned formal logic, and Russell, of whom Carnap made an extended study in the period just after his PhD. Carnap's conviction that the logicist thesis was true was still strong at the time he wrote *LSL*, as we noted. We moved on from discussing this early phase of his thinking to an examination of his time as a member of the Vienna Circle, and in conversation with Wittgenstein. We showed how his logicism combined with Wittgenstein's linguistic conception of logic to form a view which we called 'tautologicism', following some recent literature. We also detailed the way in which his views on science and meaning interacted with his view on logic and mathematics during the debate over the nature of protocol sentences in the late 1920s and early 1930s. There, I showed how Carnap's view, though 'left wing' inside the Circle, was nonetheless still encumbered by a requirement that there be a single language for science along with mathematics and logic. In the following chapter, I argued that a major shift in his position came in the wake of Gödel's discovery of incompleteness, namely the abandonment of the single language approach in favor of one where there could be many languages. The final chapter of the first portion of the thesis was a focused examination of the view that Carnap put forward in *LSL*. As I argued there, the view is the result of the considerations from the previous years of his thinking about how to implement a tautologicist view in a 'many languages' format, combined with a rediscovery of his tolerant attitude. I showed that it was this attitude, which was given its first expression in his writing during the protocol sentence debate but memorably phrased in *LSL* as the Principle of Tolerance, underwrote many of the technical results of the book. I examined a claim by Karl Menger to the effect that the Principle was originally his, but concluded that this did not take adequate account of either Carnap's personal history – in particular the attitude he inherited from his mother – or his use of Tolerance. Most importantly, in that chapter I examined the content of the book, and argued that, broadly speaking it could be viewed as split into two projects, that of developing the many languages method of investigating proposed linguistic frameworks for science, and that of showing, via the construction of two languages (Language I and Language II), that the debates over the foundations of mathematics can be answered by the construction of languages. The central aim of these first three chapters was to track the development of Carnap's view, and to set it out so that the target of the critiques that I examined in the second part of this thesis was clearly visible.

The project in the second part of the thesis, as mentioned above, was somewhat of a departure from the project in the first part. We began by giving a short history of the relationship between Gödel and Carnap in the 1920s and 1930s. We noted that because of the closeness of their association, Gödel would have been one of a very few people who knew Carnap's views on logic and mathematics at that time, and indeed would have been in a better position than almost anyone else to critique those views. With that in mind, we moved on to setting out the critique that Gödel eventually gave in the third version of his draft 1953/59 essay "Is Mathematics Syntax of Language?". We additionally set out four other criticisms due to Gabriella Crocco, Patricia Blanchette, Michael Potter, and Hilary Putnam. In the final chapter, we initially returned to Carnap's Principle of Tolerance. We argued that it should be understood in a particular way, which we dubbed the 'wide' reading of the Principle. According to this reading, we should understand Tolerance to say that there are no antecedent constraints of which languages can be proposed for potential adoption. We then turned our attention back to the criticisms of Carnap's view. We argued that they fell into two camps: those that critiqued the position by arguing that it somehow had difficulties with applied mathematics, and those which critiqued it by arguing that it was either viciously circular or somehow incoherent. However, we went on to diagnose all of these criticisms to rest on a failure to read Tolerance in the wide way. I argued that if we do understand

Tolerance in the wide way, then these criticisms fail to hit their targets. What I have not yet done, is to draw a moral from this failure, and it is this task which we take up in the remainder of our conclusion.

This thesis has in some ways been a conservative one. The story I have been telling in it is, in large part, one that squares with the way we usually understand the history of analytic philosophy, the Vienna Circle, and in particular Carnap's thinking about logic and mathematics in this period. One reason is that, in general, I think our understanding of Carnap's views is correct. There is another sense, however, in which this thesis has been quite radical. This is most easily seen in my discussion in the final chapter of the 'wide' interpretation of Tolerance. There, as I argued above, I presented a reading of the Principle whereby there are no constraints on which languages can be proposed for adoption to accomplish a task. Even those features which might at first appear to be requirements, for example the need for the language to be precisely specified, I showed were not requirements *simpliciter*, but only relative to the task of putting a linguistic framework forward for discussion. That is, even the need for "syntactical rules", in Carnap's phrase, is not an absolute; it is only required if one wants to discuss one's view. This is the point, then, where my interpretation departs from the orthodox account. With these considerations in mind, we turn now to some lessons that can be drawn from the divergence, and success, of my reading relative to the standard one.

The first point that should be made is that the success of the many reevaluations of the major figures in the analytic tradition shows us that there are many details that have been glossed over by orthodox history.[45] Arguing conclusively for the importance of these details is a subject far beyond the scope of this thesis, but we should at least indicate what such an argument might consist in. Broadly speaking, practitioners of history of philosophy can be separated into two groups, though I do not presume that this separation is unproblematic. The first group are those who work on 'getting things right'. Their aim is to correct lacunae in the common understanding of the history of philosophy, and their major focus is on making sure that the understanding of historical philosophers that we eventually come to is faithful to their thinking. The second group are those whose primary goal in doing research into the thinking of historical philosophers is finding inspiration for positions that might be taken up in contemporary debates. Though I take it that either of these approaches are worthwhile, this thesis has primarily been a project in the former camp. That is, one of our goals has been to show certain ways in which the common understanding of Carnap's thinking has been confused. Indeed, the proliferation of criticisms based, in one way or another, on failing to read Tolerance in the wide way shows this much.

However, I further hope that this thesis will serve as a call for a project in the second camp as well. And so it is here, finally, with this call that our story ends. We have shown that, understood properly, Carnap's Principle of Tolerance is not self-refuting, nor is it incoherent. With these impediments removed, before us once again is the boundless ocean of possibilities.

[45]See section 1.1 for examples of the kind of scholarship I have in mind.

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