Investigating the Universe:

Quine, New Foundations, and the Philosophy of Set Theory

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THESIS

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Dedicated to Paul R. Morris, a first teacher in life, and to Burton S. Dreben, a first teacher in philosophy.

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SPM

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LIST OF ABBREVIATIONS

NF	The Set Theory, New Foundations
PM	The Logical System of Principia Mathematica
Z	Zermelo Set Theory
ZF	Zermelo-Fraenkel Set Theory
ZFC	Zermelo-Fraenkel Set Theory with the Axiom of Choice

SUMMARY

Most broadly conceived this dissertation is concerned with the a priori in mathematics. Since mathematical knowledge seems to make no appeal to empirical circumstances and carries an air of necessity with it, philosophers have long taken it as a paradigm case of a priori knowledge. I will argue against this claiming that mathematics at its foundations, that is, set theory, is exploratory and experimental, developed pragmatically along lines similar to the natural sciences. More specifically this dissertation is concerned with the set theory New Foundations (NF), developed by W. V. Quine, and the philosophy surrounding it. The argument will have both historical and philosophical components, and though they are often related, I have done my best not to mix the two, though no doubt my philosophical and historical inclinations have at times influenced each other.

The dissertation consists of six chapters, some more closely linked than others, but all concerned with this general theme of set theory as a naturalistic endeavor. In the first section of this dissertation, I present a brief history of set theory through the discovery of the paradoxes, and I consider the main proposals offered by Zermelo and Russell for resolving them. Chapter one focuses on the work of Georg Cantor and his founding of set theory. Here, I look at the motivations for set theory in giving an account of the infinite and review the major early results of set theory. In chapter two, I turn to the crisis in set theory brought about the set-theoretic paradoxes, particularly in the work of Bertrand Russell. Two important points arise here. First, to the extent that there was any single shared conception of a set at the founding of the theory, it was the idea that a set is the extension of a predicate, i.e. is the collection of things that a predicate is true of. Second, that after the paradoxes the success of a set theory was largely judged by its ability to explain the infinite and serve as a framework for reconstructing accepted mathematics. This is a central idea throughout the dissertation in arguing that set theory is largely shaped by pragmatic concerns of theory building. In the final chapter of this section, I present Quine's NF as combining the insights of both Zermelo and Russell for resolving the paradoxes.

The second section of the dissertation contains the philosophical core. In chapter four, I argue first that by examining Quine's early technical logical work as a reaction to Russell, we gain great insight into the origins of Quine's more general naturalistic and broadly pragmatic outlook as exemplified in his later philosophical work. In looking at Quine's publications from the 1930's into the early 1950's, it is striking how much of this work concerns technical issues in logic and set theory but neglects what might be thought as the major philosophical views he is now most remembered for. I aim to bring out how these early technical endeavors are intimately intertwined with his later more obviously philosophical aims. In chapter five, I then turn to consider some arguments in favor of the iterative conception of set so often exemplified by the axioms of ZF and perhaps most notably championed by George Boolos. Currently, the iterative conception is preferred to the extent of its being taken as the single "correct" conception of a set. Against this, I argue that this preference actually rests largely on pragmatic grounds that support NF equally well. Thus, I aim to show that mathematics in its foundations develops in ways much more akin to the natural sciences and as such, is not the paradigm of a priori knowledge philosophers have often thought it to be.

In the final chapter, I continue with the arguments of the previous two turning to consider some of the more technical details of NF, particularly its disproof of the axiom of choice, in order to show that it satisfies the criteria offered for judging a set theory successful, that is, that it provides an account of the infinite and serves as a framework for reconstructing mathematics within it. In fact, in these concluding chapters I aim to bring out that as set theory itself has developed as a subject for mathematical investigation—and is thus no longer limited to providing a foundation for other branches of mathematics—NF with its "big sets" such as the universal set may be more consonant with current mathematical thought regarding set theory. A general aim throughout this dissertation is to make the technical details of NF, and its place in set-theoretic research generally, more accessible to philosophers, as well as to mathematicians and computer scientists. The theory has received a fair amount of attention by researchers in various fields, but so far much of this research remains isolated from the mainstream of philosophical and mathematical research—an unfortunate situation given its potential to elucidate very general issues in the philosophy and foundations of mathematics.

I. CANTOR AND THE ORIGINS OF SET THEORY

In the following chapter, I lay out the origins of set theory with a particular focus on the work of Georg Cantor (1845-1918) up to the discovery of the set theoretic paradoxes around 1900. I have not attempted to make any original contribution to the already existing literature on the history of set theory. My aim here in only to provide context for the chapters that will follow. To this end I have relied heavily on the work of Akihiro Kanamori, Joseph Dauben, and José Ferreirós.[1] Also, there are philosophically relevant issues here, but I will hold off explicit treatment of them until later chapters of this dissertation. The primary discussion of the major trends in the philosophy of set theory will come in part II.

Set theory arose as a mathematical discipline, though often deeply connected to the metaphysical views of it founders, during the nineteenth century rigorization of analysis. Since the beginnings of the calculus with Leibniz and Newton, the notion of a function had been gradually expanded from analytic expressions to arbitrary correspondences. Euler made the first great expansion in the eighteenth century by introducing methods relating to infinite series. In the following century, however, troubles arose surrounding such unrestricted use of functions and the related notions of convergence and continuity.[2]

Cauchy and Weierstrass eliminated these difficulties by further articulating convergence and continuity and replacing infinitesimals with the notion of a limit in terms of the now familiar epsilon-delta language. The result of their work not only eliminated the apparent flaws in the calculus, but it also restored a deductive rigor to mathematics absent since Euclid. Making sense

[1] Akihiro Kanamori, "The Mathematical Development of Set Theory from Cantor to Cohen," *The Bulletin of Symbolic Logic*, vol. 2, number 1, (March 1996), pp. 1-71; Joseph Warren Dauben, *Georg Cantor: His Mathematics and Philosophy of the Infinite*, (Princeton: Princeton University Press, 1979); José Ferreirós, *Labyrinth of Thought: A History of Set Theory and its Role in Modern Mathematics*, (Boston: Birkhäuser Verlag, 1999).
[2] Kanamori, "Development," pp. 1-2. For a brief account of such difficulties with the calculus see Joan Weiner, *Frege in Perspective*, (Ithaca: Cornell University Press, 1990), pp. 23-6.

of these new functions in terms of an infinite series was only achieved through their careful specification by means of deductive methods. Cantor emerged from this tradition that restored proof as the focal point of mathematics thus furthering its greater abstraction and generality.[3]

His first important result came in 1870 in proving a uniqueness theorem for trigonometric series: if a trigonometric series converges everywhere to zero, then all of its coefficients are equal to zero. He generalized this result in his "On the Extension of a Theorem of the Theory of the Trigonometric Series" [1872][4] to obtain the result: For a collection of real numbers P, let P' be the collection of limit points for P, and let $P^{(n)}$ be the result iterating this operation n times. If a trigonometric series converges everywhere to zero except on a P where for some n, $P^{(n)}$ is empty, then all of its coefficients are equal to zero. In considering collections of real numbers specified by an operation—a variation on the idea that sets are the extensions of concepts—he put himself on the path to set theory.[5]

In addition to this work, Cantor made another important move now familiar in contemporary set theory; he built up the real numbers from collections of rationals. While Dedekind's cuts are perhaps now the most familiar construction of the reals from rationals, Cantor construed the reals as what he called "fundamental sequences" of rationals, or what are now often called "Cauchy sequences."[6] As Dauben explains, Cantor saw a logical error in previous attempts to construct the real numbers in that these constructions had in some sense presupposed the very objects they were supposed to define. The common mistake was to equate

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[3] Kanamori, "Development," p. 2.
[4] Georg Cantor, *Gesammelte Abhandlungen: Mathematische und Philosophischen Inhalts*, Ernst Zermelo, ed. (Hildesheim: Georg Olms Verlagsbuchhandlung, 1962), pp. 92-102.
[5] Kanamori, "Development," p. 2; On the relationship between sets as the extensions of concepts and the specification of a set by an operations see José Ferreirós, *Labyrinth of Thought*, p. 143; see also George Boolos, "The Iterative Conception of Set," in *Logic, Logic, and Logic*, ed. Richard Jeffrey, (Cambridge: Harvard University Press, 1998), pp. 13-4; I will return to this point in more detail in Part II.
[6] The fundamental sequences are defined by the same property Cauchy used for his criterion of convergence; see Ferreiros, p. 128.

some arithmetic sum with a preexisting real. Cantor's idea was to show that the sequences themselves could do the work of the reals.[7]

Following Weierstrass, Cantor began with the collection of rationals A, and then provided a foundation for all further concepts of number in terms of an infinite sequence of rationals $\{a_n\}$. He first defines a fundamental sequence as follows: an infinite sequence of rationals

$$(1) \qquad a_1, a_2, \ldots, a_n, \ldots$$

is fundamental if there exists an integer N such that for any positive rational value of ε, $|a_{n+m} - a_m| < \varepsilon$, for any m and for all $n > N$. He then observes that if $\{a_n\}$ is a fundamental sequence then it has a definite limit b, though Dauben carefully points out that in saying this Cantor does not mean to presuppose an actual limit b of $\{a_n\}$. At this point, he only says that b is a definite symbol associated with the sequence $\{a_n\}$. Next, he defines the ordering relations between fundamental sequences: if we associate $\{a_n\}$ with b and $\{a_n'\}$ with b′, then if for all n greater than some arbitrarily large N, $a_n – a_n' < \varepsilon$, then $b = b'$; if $a_n – a_n' > \varepsilon$, then $b > b'$; and if $a_n – a_n' < -\varepsilon$, then $b < b'$. Thus, a fundamental sequence with limit b can bear one of three relations to a rational number a, which can itself be construed as the constant fundamental sequence $\{a\}$; either $b = a$ or $b < a$ or $b > a$. From this discussion, Cantor concludes, "From these and the definitions immediately following it follows that if b is the limit of the sequence (1), then $b - a_n$ becomes infinitely small as n increases, whereby, *incidentally*, the designation 'limit of the sequence (1)' for b finds a certain justification."[8]

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[7] Dauben, *Cantor*, p. 37.
[8] Ibid., p. 38; Cantor, [1872], p. 93 (Dauben's translation).

Finally, Cantor extends the arithmetic operations from the collection of rationals A to the collection B of new numbers b: given the numbers b, b′, and b′′ in B respectively associated with the fundamental sequences $\{a_n\}$, $\{a_n'\}$, and $\{a_n''\}$, we define the following operations

$$b + b' = b'' \quad \text{as} \quad \lim(a_n + a_n' - a_n'') = 0$$

$$b \cdot b' = b'' \quad \text{as} \quad \lim(a_n \cdot a_n' - a_n'') = 0$$

$$b / b' = b'' \quad \text{as} \quad \lim(a_n / a_n' - a_n'') = 0.$$

Having thus extended these arithmetic operations to the new objects of B, Cantor now refers to these objects b as numbers rather than symbols and as such completes his construction of the reals out of the rationals.[9] Kanamori notes three particularly important aspects of Cantor's construction of the reals in influencing his development of set theory. The construction in terms of fundamental sequences led him to more explicitly consider infinite collections, to view them as unitary objects, and to allow for arbitrary possibilities of such objects. But it was Cantor's next move—to prove that the reals are uncountable—that led to his full-blown development of transfinite set theory. "Set theory was born on that December 1873 day when Cantor established that the collection of real numbers is uncountable," Kanamori remarks, "and in the next decades the subject was to blossom through the prodigious progress made by him in the theory of ordinal and cardinal numbers."[10]

During this period Cantor began considering infinite iterations of his operator P' where

$$P^{(\infty)} = \cap_n^{\infty} P^{(n)},\ P^{(\infty+1)} = P^{(\infty)\prime},\ P^{(\infty\cdot 2)},\ \ldots,\ P^{(\infty\wedge 2)},\ \ldots,\ P^{(\infty\wedge\infty)},\ \ldots,\ P^{(\infty\wedge\infty\wedge\wedge\infty)},\ \ldots.$$

He also began to investigate infinite collections and real numbers and infinite enumerations as such. Combined, these moves led him to the basic concepts used in the study of the continuum

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[9] Ibid., pp. 38-9.
[10] Kanamori, "Development", p. 3.

and to the formulation of the transfinite numbers.[11] His first major result in this direction was to prove that the set of reals is uncountable. The proof first appeared in print in his 1874 "On a Property of the Totality of All Real Algebraic Numbers".[12] Cantor first establishes that the algebraic numbers are countable, where a real number ω is algebraic if there exists a positive integer n, and integers $a_0, a_1, \ldots, a_n$, $a_n \neq 0$, such that

$$a_n\omega^n + a_{n-1}\omega^{n-1} + \cdots + a_1\omega + a_0 = 0.$$

He then proceeds by reductio to show that for any countable sequence of reals, every interval contains a real not in the sequence:

Let the set of reals be countable. Then each real ω can be sequenced by indexing them with natural numbers n:

$$\omega_1, \omega_2, \omega_3, \ldots, \omega_n, \ldots.$$

Now, given an interval (α, β) a subset of reals, it is possible to find at least one real number η such that η fails to be listed as an element of the sequence. To find such an η, let $\alpha < \beta$ and pick the first two numbers α', β' of the above sequence in the interval (α, β). These form another interval (α', β'). Then continue this procedure to yield a sequence of nested interval through (α^n, β^n) where α^n, β^n are the first two numbers of the sequence in the interval $(\alpha^{n-1} \ldots \beta^{n-1})$. There are two possibilities to consider:

If the number of constructed intervals is finite, then at most only one additional element of the sequence could lie in the interval (α^n, β^n). By choosing any real number η in the interval (α^n, β^n) not equal to the possible number from the sequence, we find a real not listed in the sequence.

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[11] Ibid., p. 3.

[12] Georg Cantor, "On a Property of the Totality of All Real Algebraic Numbers," in *From Kant to Hilbert: A Source Book in the Foundations of Mathematics*, vol. II, ed. William Ewald, (Oxford: Clarendon Press, 1996), pp. 840-43.

If on the other hand the number of constructed intervals is not finite, since the sequence α, α', …, α^n, … is bounded in (α, β), it has an upper limit α^∞, and similarly, the sequence β, β', …, β^n, … has a lower limit β^∞. There are two further cases to consider. If $\alpha^\infty < \beta^\infty$, then as in the finite case any real number η in the interval $(\alpha^\infty, \beta^\infty)$ would be the real not listed in the above sequence. If, however, $\alpha^\infty = \beta^\infty$, then $\eta = \alpha^\infty = \beta^\infty$ and η could not be listed as an element of the above sequence. Let $\eta = \omega_\rho$. For n a sufficiently large index, ω_ρ would be excluded from all the intervals nested within (α^n, β^n). But by virtue of the construction, η must lie within (α^n, β^n). Contradiction, and hence, the collection of real numbers is uncountable.[13]

In his next publication "A Contribution to the Theory of Manifolds" [1878], Cantor began to focus more directly on bijective mappings between sets.[14] He defined two sets as having the same power if and only if there is a bijective mapping between them. Whereas his earlier result on the uncountability of the reals showed where such a mapping failed to hold, he now looked to see where these mappings did hold. It was these investigations that led him to fully develop the mathematics of the transfinite.[15] At the end of his investigations, however, he had only been able to find two powers of infinite sets and conjectured that every set of reals has the first power, that is, is countable, or has the power of the continuum. His attempts to solve this early version of the Continuum Hypothesis pushed his work forward leading him to a more arithmetical approach in studying sets of real numbers as well to questions of set existence as such. At least where Cantor is concerned, Kanamori observes that "Set theory had its beginnings not as some abstract foundation for mathematics but rather as a setting for the articulation and

[13] I have followed Dauben pp. 51-2 in presenting Cantor's proof, but Kanamori p. 6 also sketches it but in less detail.
[14] Cantor, *GA*, pp. 119-33.
[15] Kanamori, "Development", pp. 4-5.

solution of the *Continuum Problem*: to determine whether there are more than two powers embedded in the continuum."[16]

Cantor's next major work, one of the most important in presenting his developed set theory, *Foundations of a General Theory of Manifolds: A Mathematico-Philosophical Investigation into the Theory of the Infinite* [1883], commonly referred to as *Grundlagen* introduced his theory of ordinal numbers and the notion of well-ordering.[17] His idea was to shift focus away from the infinitely indexed operator P' used in his paper on the trigonometric series and to turn his attention to the indexes themselves, what became his ordinal numbers. Here, he also made a notable notational change in moving from the symbol '∞' of the potential infinite to 'ω', the final letter of the Greek alphabet, to represent the infinite as a completed whole. In another terminological shift representing his focus away from subsets of real numbers and to abstract set theory, Cantor stopped speaking of point-manifolds and instead talked of sets. The major achievement of the *Grundlagen*, as these changes indicate, was to single out the transfinite numbers as both an autonomous and systematic extension of the finite numbers. Without this move, Cantor would have had no way to progress beyond the finite, and as such, research in abstract set theory and on the continuum would have stopped.[18]

Traditionally, mathematicians had treated the infinite as a variable increasing beyond all limits or decreasing to arbitrary smallness. In being arbitrarily large or small, however, the infinite was only potential, and the finite remained the primary notion. The idea, in a way, was that there was always some finite number that would be large or small enough available for the task at hand. Absolute, or completed, infinities were thought to be incoherent. Cantor aimed to

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[16] Ibid., p. 5.
[17] Georg Cantor, *Foundations of a General Theory of Manifolds: A Mathematico-Philosophical Investigation into the Theory of the Infinite*, in Ewald, vol. II, pp. 878-920.
[18] Kanamori, "Development", p. 5; Dauben, *Cantor*, p. 96.

show in the *Grundlagen* that this was in fact not the case. A succession of actually infinite numbers could be developed with identifiable and determinate number theoretic properties making these transfinite numbers just as legitimate as any other number system.[19]

Prior to this work, Cantor had no simple definition of the powers beyond the denumerable infinite. The least power among the infinite sets was the countable set of natural numbers. His new transfinite number would allow for a natural definition of powers beyond this countable infinity. To this end, he observed that the natural numbers resulted from the repeated addition of units to yield the sequence 1, 2, 3, …. This he calls *the first principle of generation*, the successive generation of finite ordinal numbers by successive addition. This class, the number class (I), had no largest element, but there was nothing to stop Cantor from introducing a new number ω expressing the natural order of the entire set (I). This number is the first number following the entire sequence of finite numbers in the set of natural numbers; it is the first transfinite number. But then there was also no reason not to apply the first principle of generation again to yield additional transfinite numbers $\omega, \omega + 1, \omega + 2, \ldots, \omega + n, \ldots$. Again, this sequence had no largest element, but Cantor then introduced another new number 2ω to represent this entire sequence in its natural order. And again he applied the first principle of generation to yield the new sequence $2\omega, 2\omega + 1, 2\omega + 2, \ldots, 2\omega + n, \ldots$.

This process then led him to characterize what he called *the second principle of generation*. He explained that ω could be thought of as a limit approached by the sequence of natural numbers but never reached by it in the sense that ω is the first whole number after all the finite numbers in the set of natural numbers. So his second principle of generation stated that if a sequence of numbers has no greatest element, then a new transfinite number can always be generated as the least number greater than all those in the sequence. And so the successive

[19] Ibid., p. 96.

application of the two principles of generation always allowed for the possibility of generating a new number in succession to those previously generated numbers. Once he had this second principle, he was able to define his second number class (II) as the collection of all numbers α formed from the two generating principles in a definite increasing succession where all numbers proceeding α constitute a set with the same power as the first number class (I). He also indicated a third principle, the principle of limitation, that was to allow him to proceed to even higher classes of numbers, though he did not do much to develop this idea.[20]

In summarizing the significance of this work on the first and second number classes, Dauben notes the important difference mentioned above between the transfinite numbers and Cantor's earlier introduction of infinite symbols. Previously he focused on derived sets of the second species and treated the transfinite symbols attached to his operator P' as mere indices to identify and distinguish among the derived sets themselves. The transfinite numbers, however, were themselves independent numbers, and this was necessarily so. Cantor aimed to use these very numbers in his further investigations into the powers of sets and the continuum. Hence, he could not define the transfinite numbers in terms of the very sets he wished to study. They required an independent formulation so that they could be applied to the study of point sets and their powers. The transfinite numbers were to be understood as having an independent claim on reality equal to that of any of the other numbers.[21]

Another crucial aspect of the development of set theory introduced in *Grundlagen* was the well-ordering principle, which Cantor described as "the law of thought that says that it is always possible to bring any *well-defined* set into the *form* of a *well-ordered* set—a law which seems to me fundamental and momentous and quite astonishing by reason of its general

[20] Ibid., pp. 97-8. We will see in Chapter 2 how Cantor avoided the paradoxes, Burali-Forti being the relevant one in this case.
[21] Ibid., pp. 97-8.

validity."[22] As Kanamori points out, this principle can be understood as part of the unity Cantor saw between the finite and transfinite numbers; just as the finite numbers can be well-ordered so can the transfinite numbers.[23]

Cantor introduces the notion of a well-ordered set early in *Grundlagen* stating that

> A well-ordered set is a well-defined set in which the elements are bound to one another by a determinate given succession such that (i) there is a first element of the set; (ii) every single element (provided it is not the last in the succession) is followed by another determinate element; and (iii) for any desired finite or infinite set of elements there exists a determinate element which is their immediate successor in the succession (unless there is absolutely nothing in the succession following all of them).[24]

These well-ordered sets were essential to his investigations into the transfinite numbers and in particular, in distinguishing finite from infinite sets. Here, he introduced the important concept of a numbering [Anzahl], where a numbering expressed the ordering of the elements of a given set. Later, he would identify these numberings as the ordinal numbers. In drawing this connection between well-ordered sets and their numberings, Cantor was also able to further the transfinite numbers' claim to having objective reality. As Dauben explains the objective reality of the transfinite numbers came from the existence of well-ordered set whose order could be expressed by associating them with a number from the various transfinite number classes. To this end, Cantor aimed to show that for any countably infinite well-ordered set, there was always a number of the second number class (II) that uniquely represented its ordering.[25]

To illustrate this he considered the denumerable set (α_ν), which can be well-ordered, for example, in any of the following ways:

$$(\alpha_1, \alpha_2, \ldots, \alpha_\nu, \alpha_{\nu+1}, \ldots)$$

$$(\alpha_2, \alpha_3, \ldots, \alpha_{\nu+1}, \alpha_{\nu+2}, \ldots, \alpha_1)$$

$$(\alpha_3, \alpha_4, \ldots, \alpha_{\nu+2}, \alpha_{\nu+3}, \ldots, \alpha_1, \alpha_2)$$

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[22] Cantor, *Foundations*, p. 886.
[23] Kanamori, "Development", p. 6
[24] Cantor, *Foundations*, p. 884.
[25] Dauben, *Cantor*, p. 101.

$(\alpha_1, \alpha_3, \ldots, \alpha_2, \alpha_4, \ldots)$.

Cantor states that two well-ordered sets have the same numbering, or are similar, if they can be put into a one-to-one correspondence in such a way that preserves their respective orderings. So if α_n comes before α_m in the ordering of one set, then in the other set the respective corresponding elements α_n' and α_m' must be ordered so that α_n' comes before α_m'. And such correspondences are always uniquely determined. So he observed that given any two well-ordered sets, he could use the succession of natural numbers plus the transfinite numbers to identify similar well-ordered sets. Given any number α of the first or second number class, when taken together with all or its preceding elements, the numbering of all similar well-ordered sets is given by α uniquely. For example,

$$(\alpha_1, \alpha_2, \alpha_3, \ldots, \alpha_\nu, \alpha_{\nu+1}, \ldots)$$

$$(\alpha_2, \alpha_1, \alpha_4, \ldots, \alpha_{\nu+1}, \alpha_\nu, \ldots)$$

$$(1, 2, 3, \ldots, \nu, \ldots)$$

all have the same numbering ω. Similary,

$$(\alpha_2, \alpha_3, \ldots, \alpha_\nu, \ldots, \alpha_1)$$

$$(\alpha_3, \alpha_4, \ldots, \alpha_{\nu+1}, \ldots, \alpha_1, \alpha_2)$$

$$(\alpha_1, \alpha_3, \ldots; \alpha_2, \alpha_4, \ldots)$$

have the distinct numberings $\omega + 1$, $\omega + 2$, and 2ω, respectively.[26]

Cantor also employed his distinction between number and numbering to bring out differences between finite and infinite sets. For finite sets, their numbering remains the same regardless of their ordering, whereas for infinite sets, there are sets of the same power with different numberings and so with different well-orderings. The numberings, then, are dependent

[26] Ibid., p. 102.

on the ordering of the elements so that, in general, different orderings produce different numberings, although they have the same number of elements. In addition, there is also a correlation between the number of elements in a set and the number of numberings the set could yield depending upon its ordering. For example, Cantor considered the sets of the first number class (I) given in a determinate order. The numberings of these sets, so long as they are well-ordered, always corresponded to numbers of the second number class (II). Also, conversely, given any number α of (II), any set of (I) could be ordered so that its numbering would correspond to α. Analogous results hold, as well, for sets of higher power.[27]

As indicated above, when focusing only on finite sets, the concepts of power and numbering coincided; different orderings of finite sets did not produce different powers. Since for finite sets power was independent of ordering, a finite set of n elements just had power n. But for infinite sets the distinction between power and number was significant. Every number α of (II) indicated a unique ordering of elements, but any such set with α as its numbering was always denumerable. Here a connection remained between power and numbering in that any denumerable well-ordered set had a uniquely determined numbering α where α belonged to the second number class (II). The properties of well-ordered sets had the result of joining these various notions of transfinite numbers, numberings, and number classes together into a unified theory. The concept of numbering merely generalized the notion of counting. For Cantor, this further supported his view that the transfinite numbers were just as legitimate as their finite counterparts.[28]

Kanamori claims that Cantor's interest in the well-ordering principle and the ordinal numbers, that is, the numberings, was directly linked to his attempts to solve the continuum

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[27] Ibid., p. 102-03.
[28] Ibid., p. 103.

problem. The transfinite ordinals provided him with a framework for his two primary approaches to the problem, one through power and the other through definable sets of real numbers. His approach through power focused on the first and second number classes, the natural numbers and the set of countably infinite ordinals respectively, though he also gave some indication that these classes could be continued to a third number class and beyond. His major result here was to prove that the number class (II) was uncountable, and that any subset of (II) is either countable or of the same power as (II). As such, the second number class (II) had precisely the property Cantor sought for the real numbers reducing the continuum hypothesis to the assertion that the set of real numbers and the second number class were of the same power. He could not, however, seem to find a definable well-ordering of the real numbers and so could not find a correlation between these two sets.

Cantor had not yet developed his famous diagonal method of proof for showing (II) uncountable. Instead, in section 12 of *Grundlagen* he presented an argument similar to that given earlier in his proof that the reals are uncountable. Following Kanamori, we sketch the proof as follows. Let s be a countable sequence of countable ordinals with a least element a. Let a′ be a member of s, if any, such that a < a′; similarly for a′ < a″, and so on. No matter how long this process continues, the supremum of such elements is not a member of s. Hence, the second number class (II) is uncountable.[29]

Although of less importance to my investigations in the following chapters, as Kanamori observes, Cantor also continued to pursue the continuum problem through definable sets of real numbers, an approach that evolved from his earlier work on the trigonometric series. As noted his earlier "symbols of infinity" used in the analysis of the P' operator had become by the time of

[29] Kanamori, "Development", p. 6; Cantor, *Grundlagen*, pp. 909-10.

the *Grundlagen*, the ordinals of the second number class. Here, he studied P' for uncountable P and defined the important concept of a *perfect set of real numbers*, a set of real numbers which is non-empty, closed, and contains no isolated points. Then in his 1884 "On Infinite Linear Point-Manifolds" he proved that any uncountable closed set of real numbers is the union of a perfect set and an uncountable set.[30] A set A of real numbers has the perfect set property if and only if A is countable or has a perfect subset. So in particular, he established that closed sets have the perfect set property. In showing that any perfect set has the power of the continuum he was able to establish that the continuum hypothesis held for closed sets, that is, every closed set is countable or has the power of the continuum. This again reduced the continuum problem, here, to the question of whether there is a closed set of real numbers that has the power of the second number class (II).[31]

In the years following the *Grundlagen*, Cantor continued to be unable to solve the continuum problem by searching for direct correlations between the set of real numbers and the ordinals. This led him to a more general approach to size and order that would take into account the continuum. To this end, he introduced the notion of a *cardinal number* replacing the earlier terminology of 'power'. He also went beyond the study of well-orderings to the more general notion of linear order types, and he took on a view of well-defined sets as being given together with a linear ordering of their members. Order types and cardinal numbers resulted from the successive abstraction from a set M to its order type M-bar and then to its cardinal M-double-bar. As he describes the process relating to cardinal number of a set, it is "the general concept which, by means of our active faculty of thought, arises from the aggregate M when we make abstraction of the nature of its various elements m and of the order in which they are given." For

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[30] Cantor, *Gesammelte*, pp.210-46.
[31] Kanamori, "Development", pp. 6-7.

ordinals the process is similar, but we do not also abstract away the ordering of the set. The process is hazy and carries with it mentalistic overtones to which we will return in chapter 2.[32] It was in this context that Cantor put forward in his 1891 "On an Elementary Question in the Theory of Manifolds" his famous diagonal argument establishing that for any set M the collection of functions from M into a two-element set is of higher power, or cardinality, than M itself.[33]

As we have seen, Cantor had already proved in 1874 the existence of uncountable sets. This proof however relied on the existence of irrational numbers. His 1891 aimed to establish this result with much greater generality, though it would employ extremely powerful methods and would lead him to an ascending and limitless hierarchy of transfinite powers. In particular, the proof relied upon full-impredicativity in its appeal to arbitrary functions, or equivalently, arbitrary subsets.[34] Irrational numbers were themselves still controversial mathematical objects and so did not help to convince the wider mathematical community of the reality of his even more controversial transfinite set theory. Cantor's diagonal method allowed him to avoid all talk of point-sets, irrational numbers, or any specific objects at all. In this way, he could achieve a complete generality where the elements involved in the proof were themselves unquestionable.

The diagonalization proof relied on two elements m and w. Cantor used these to consider a collection M made up of elements $E = (x_1, x_2, \ldots, x_n, \ldots)$, where each x_n was either m or w. So, for example, the elements E might look like any of the following:

$E^{I} = (m, m, m, m, \ldots)$,

$E^{II} = (w, w, w, w, \ldots)$,

$E^{III} = (m, w, m, w, \ldots)$.

[32] Cantor, *Grundlagen*, pp. 86, 112.
[33] Kanamori, p. 7; Cantor, "On an Elementary Question in the Theory of Manifolds", in Ewald, vol. II, pp. 920-22.
[34] Ibid., p. 7.

He then claimed that any such collection M was uncountable: if $E_1, E_2, \ldots, E_\nu, \ldots$ is any simply infinite sequence of elements of the set M, then there is an element E_0 in M which is not equal to any E_ν. To prove his claim, he first gave a countable listing of elements E_μ where each $a_{\mu,\nu}$ was either m or w:

$$E_1 = (a_{11}, a_{12}, \ldots, a_{1\nu}, \ldots),$$

$$E_2 = (a_{21}, a_{22}, \ldots, a_{2\nu}, \ldots),$$

$$\ldots$$

$$E_\mu = (a_{\mu 1}, a_{\mu 2}, \ldots, a_{\mu\nu}, \ldots)$$

$$\ldots.$$

This defined a new sequence $b_1, b_2, \ldots, b_\nu, \ldots$ where each b_ν was either m or w, but $b_\nu \neq a_{\nu\nu}$. This sequence of b_ν yielded a new element $E_0 = (b_1, b_2, \ldots, b_\nu, \ldots)$ in M, where $E_0 \neq E_\nu$ for any index ν. No matter what element E_ν was considered, E_0 always differed from it at the ν-th coordinate. Therefore, there was always an element left off of a countable listing of elements of M, and hence, M was uncountable.[35]

Cantor the proceeded to show how his diagonal method could be applied to particular infinite sets by considering the set L of the linear continuum, the set of real numbers on (0, 1), and then showing that the set M of single-valued functions f(x) with only values 0 or 1 for any x in (0, 1) was of greater power than L. M was clearly greater than or equal to L in power since it contained a subset equal in power to L. For example, the set N of functions f(x) on (0, 1) equal to 0 everywhere except at a single point x_0 where $f(x_0) = 1$ was such a subset. So Cantor just had to show that L and M were in fact not equal in power. If M and L are equal in power, then it must be possible to establish a one-to-one mapping between them. That is, there must be a

[35] Dauben, *Cantor*, p. 166; Cantor, "On an Elementary Question", pp. 920-21.

function $\varphi(x, z)$, where for every value z there must be an element f(x) in M such that $f(x) = \varphi(x, z)$. And conversely, for every element f(x) in M given by $\varphi(x, z)$, there must be a unique z such that $f(x) = \varphi(x, z)$. This is impossible as shown by the diagonalization method. Consider the function g(x) having only values 0 or 1, but where $g(x) \neq \varphi(x, x)$, for any given x. Hence, g(x) is in M, but z could not be determined in such a way that would yield g(x) from $\varphi(x, z)$ since $\varphi(z_0, z_0)$ is never equal to $g(z_0)$. Therefore, M is of greater power than L.[36]

From his new proof of the existence of uncountable sets, Cantor provided a simple way for showing that the ascending sequence of powers of well-defined sets had no maximum. Indeed, he took this to be the true significance of the proof:

> This proof is remarkable not only because of its great simplicity, but more importantly because the principle therein can be extended immediately to the general theorem that the powers of well-defined manifolds have no maximum, or what is the same thing, that for any given manifold L we can produce a manifold M whose power is greater than that of L.[37]

Furthermore, Cantor took this infinite ascending sequence of higher and higher powers as a generalization of the notion of a finite cardinal and as such, having equal status as the finite numbers. As he remarked,

> The ‘powers’ represent the unique and necessary generalization of the finite ‘cardinal numbers’. They are none other than the actual-infinite cardinal numbers, and they have the same reality and determinateness as the others, except that the law-like relations among them—their ‘number theory’—is in part of a different sort than in the domain of the finite.[38]

For Cantor—looking at the history of mathematics—the rational, irrational, and complex numbers had been accepted because of their utility and consistency. Each of these number systems was a consistent generalization of prior less comprehensive concepts. The concept of power, then, was the most comprehensive and natural of all.[39]

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[36] Dauben, *Cantor*, p. 167; Cantor, “On an Elementary Question”, pp. 922.
[37] Ibid., pp. 921-22; Dauben also quotes this passage on p. 166.
[38] Cantor, “On an Elementary Question”, pp. 922.
[39] Dauben, *Cantor,* p. 167.

Cantor's final major work in set theory was his 1895/1897 *Contributions to the Founding of the Theory of Transfinite Numbers*, commonly known as the *Beiträge*.[40] This work summarized his progress in set theory to this point, while also clearly revealing a gap left open by his inability to settle the continuum problem. In part I, he presented his post-*Grundlagen* research on cardinal numbers and the continuum posing the question of cardinal comparability; whether for all cardinals a and b, either a = b, a < b, or b < a. He promised a proof at some point later but never provided one. It finally came as a consequence of Zermelo's 1904 Well-Ordering Theorem. Putting comparability aside, Cantor first turned to defining the addition, multiplication, and exponentiation of cardinal numbers. He also introduced the now standard aleph notation for infinite cardinals taking $\aleph_0$ as the cardinality of the set of natural numbers, the first infinite cardinal number. He, then, further observed that $2^{\aleph\text{-nought}}$ is the cardinality of the set of real numbers and easily established, by way of his cardinal arithmetic, the one-to-one correspondence between **R** and $\mathbf{R}^{\mathbf{n}}$ since $(2^{\aleph\text{-nought}})^{\aleph\text{-nought}} = 2^{\aleph\text{-nought} \cdot \aleph\text{-nought}} = 2^{\aleph\text{-nought}}$. Finally, he presented his theory of order types taking η as the order type of the rationals, a countable dense linear order without endpoints, and θ as the order type of the reals, a perfect linear order with a countable dense set.[41]

In part II of *Grundlagen*, Cantor further developed his views on well-orderings construing their order types as the ordinals and proving, by way of order comparisons of well-ordered sets, that they are comparable. He then presented ordinal arithmetic as just a special case of the arithmetic of order types. He also paid some attention to the properties of the second number class (II) and defined its cardinal number as $\aleph_1$. In his final sections, Cantor turned to

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[40] Georg Cantor, *Contributions to the Founding of the Theory of Transfinite Numbers*, trans. Philip E.B. Jourdain, (New York: Dover Publications, Inc., 1955).
[41] Kanamori, "Development", p. 8.

ordinal exponentiation in the second number class defining this operation by transfinite recursion and using it to establish his famous normal form theorem. He concluded with a discussion of the numbers satisfying the condition $\varepsilon = \omega^{\varepsilon}$, the so-called epsilon numbers.[42]

With the first part of the *Beiträge* dealing primarily with cardinal numbers and the continuum and the second part focusing on ordinal numbers and well-orderings, the two parts remained distinct and lacking unity in their subject matter. Kanamori states that this was not just an oversight but the indication of a serious split between the areas of Cantor's developing set theory. For example, nowhere in part I does he state, even as a special case, his major 1891 result that $a < 2^{a}$. Instead the succession of transfinite cardinals are taken as the alephs defined as the powers of the sets of their predecessor ordinals. But then in part II, he never mentions any aleph beyond $\aleph_1$, nor that the continuum hypothesis can be stated as $2^{\aleph\text{-nought}} = \aleph_1$. Furthermore, he establishes ordinal comparability but is unable to reduce cardinal comparability to it. Kanamori concludes, "Having ushered in arbitrary functions through cardinal exponentiation Cantor had introduced an irreconcilable tension into his view that all sets are well-ordered, and there was little point to developing the theory of higher alephs with the assurance of their gauging all the cardinal numbers."[43]

The 1904 International Congress of Mathematicians at Heidelberg, however, signaled a turning point. Julius König presented a proof of the claim that $2^{\aleph\text{-nought}}$ was not an aleph, meaning that the continuum was in fact not well-orderable. Although, by the next day, Zermelo had shown König's proof to be faulty, Cantor was shaken with worries that the continuum might escape the very number context he had built to analyze it. This however was not the only

[42] Ibid., p. 9.
[43] Ibid., pp. 8-9.

problem looming for Cantor's set theory. By this time, the paradoxes of set theory were also threatening, and it is to this topic that I turn to next.

II. TROUBLE IN PARADISE: RUSSELL, ZERMELO, AND THE SET-THEORETIC PARADOXES

I now turn to consider the set-theoretic paradoxes primarily in their historical context, though as we will see, this cannot be done entirely in isolation from the philosophical context. There has been much recent scholarship in this area attempting to sort out the rather confusing history of how the paradoxes arose, when they were discovered, and by whom. Much of what I aim to do in this chapter is to synthesize this work so as to present a unified history of the paradoxes and to make clear how much recent scholarship has overturned some of the traditional and widespread misconceptions concerning the history of the paradoxes. But I also aim to bring out that construing the early history of set theory as being entirely mathematical or entirely philosophical presents a misleading view. In a certain sense, I take it that either approach has philosophical motivations. To give some illustration of this, I mention a recent trend in the history of set theory which emphasizes its mathematical development over and above its philosophical origins. In some ways this emphasis has helped in presenting a more accurate view of set theory's development, especially where the paradoxes are concerned. But in reading this literature we get the sense that some of this emphasis is being shaped by, perhaps unconscious, philosophical motivations reminiscent of a sort of positivism.

To give some indication of this trend consider the following comment from the mathematical logician Gerald Sacks, a fairly early illustration of the sort of mathematical attitude I am suggesting as having philosophical consequences for how we are to think about set theory:

> [F]oundational activity is of interest in some limited spheres. For example, when there were difficulties in set theory, a few small changes were needed to straighten things out. There were difficulties, but not paradoxes; there was no need to write *Principia Mathematica* to straighten them out…. Since there have not yet been any substantial paradoxes, there has not yet been any need for a wider sphere of foundational activity. For example, there were confusions, not paradoxes, in late nineteenth and early twentieth century mathematics, despite all claims to the contrary."[44]

[44] Gerald Sacks, "Remarks Against Foundational Activity", in *Selected Logic Papers*, (London: World Scientific, 1999), p. 219.

As we will see something like this attitude is also implicit in much recent historical work surrounding the set-theoretic paradoxes, which leads to a historical account that privileges the mathematical approach to set theory and downplays the philosophical. For example, Akihiro Kanamori often seems to disparage the likes of Russell for being overly troubled by the paradoxes, but praises the mathematicians for quickly moving past them, if they even acknowledge them at all:

> [F]rom a logical point of view Russell … became exercised with paradox. He had arrived at Russell's Paradox in late 1901 by analyzing Cantor's diagonal argument applied to the class of all classes, a version of which is now known as Cantor's paradox of the largest cardinal number…. Russell's Paradox famously led to the tottering of Frege's mature formal system, the *Grundgesetze*…. The mathematicians did not imbue the paradoxes with such potency. Unlike Russell who wanted to get at everything but found that he could not, they started with what could be got at and peered beyond.[45]

Such emphasis has its place in correcting the historical record since the more standard interpretation has overemphasized the philosophical roots of set theory and the paradoxes. But a danger arises in this revised interpretation as well. Too much emphasis on the mathematical approach to set theory and with it the view that mathematical set theorists were not greatly troubled by the paradoxes leads to the misguided idea that there were always two notions of a set, the mathematical, and the philosophical or logical. While I want to concede the point that mathematicians often reacted differently to the paradoxes than more philosophically minded logicians, I will stress that all researchers in set theory had to come to terms with the paradoxes and that it was only in doing so that the two conceptions of set emerged. Both mathematical and philosophical logicians clarified the notion of a set in reaction to the paradoxes. To the extent that there was any notion of a set prior this crisis, it was roughly the view that a set is the extension of a predicate or concept, a view with a long tradition in the realm of philosophy.

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[45] Kanamori, "Mathematical Development", p. 13.

I

The history of set theory and the discovery of the paradoxes is not an easy one to unravel. On what has been called "the standard account", Cesare Burali-Forti published the first of the set-theoretic paradoxes in 1897—that of the greatest ordinal, which has become known as the Burali-Forti Paradox.[46] So Jean van Heijenoort writes in his introduction to Burali-Forti's paper, "The paper is the first published statement of a modern paradox. It immediately aroused the interest of the mathematical world, and it provoked lively discussions in the years that followed its publication. Dozens of papers dealt with it, and it gave a strong impulse to a reexamination of the foundations of set theory."[47] However, more recent work from Gregory Moore and Alejandro Garciadiego on the history of the paradoxes raises doubts about van Heijenoort's claim.[48] Indeed, they conclude that Russell was primarily responsible for discovering and disseminating the Burali-Forti paradox, as well most of the other set-theoretic paradoxes.

The first part of Cantor's "Contributions to the Founding of the Theory of Transfinite Numbers" published in 1895 left open the question of the comparability of ordinals, that is whether for any two ordinals α and β either $\alpha < \beta$, $\beta < \alpha$, or $\alpha = \beta$.[49] Burali-Forti states the aim of his paper as answering this question negatively. Nowhere in the paper does he give any prominence to the statement of the paradox that would later bear his name. Where we do see some hint of this paradox, as Moore and Garciadiego note, is in the proof itself of his central

[46] Cesare Burali-Forti, "A Question on Transfinite Numbers," in *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*, ed. Jean van Heijenoort, (Cambridge: Harvard University Press, 1967), pp. 104-11.
[47] Ibid., p. 104.
[48] Gregory H. Moore and Alejandro Garciadiego, "Burali-Forti's Paradox: A Reappraisal of its Origins," *Historia Mathematica* 8 (1981), pp. 319-50; Alejandro Garciadiego, *Bertrand Russell and the Origins of the Set-theoretic "Paradoxes"* (Boston: Birkhäuser Verlag, 1992). They note that van Heijenoort was far from the only historian to hold this view citing, among others, the similar attitudes of I.M. Bochenski, Nicolas Bourbaki, Morris Kline, and William and Martha Kneale; see p. 320 and pp. 19-20 of these works respectively. I use the terminology "the standard account" following them. In much of what follows on the emergence of the paradoxes I rely of Moore and Garciadiego for the historical details, though I do not always follow them in the conclusions they draw from them.
[49] Georg Cantor, *Contributions to the Founding of the Theory of Transfinite Numbers*, trans. Philip E.B Jourdain, (New York: Dover Publications, Inc., 1955). The 1895 article is contained in pp. 85-136; the follow-up 1897 article is on pp. 137-208.

claim.[50] To establish this claim, he first defines a set A as perfectly ordered if (1) it has a first element; (2) every element with a successor has an immediate successor; and (3) for x in A, either x has no immediate predecessor or x has a predecessor y with no immediate predecessor and there are only finitely many elements of A between x and y. Furthermore, he took a set to be well-ordered if it satisfied only conditions (1) and (2), and so every well-ordered set was also perfectly ordered. Next, he assumes the comparability of ordinals and proceeds to show by reductio that this fails to hold. First, he defines "No" as "the order type for a perfectly ordered class", and then proves that if a is the order type of a perfectly ordered class then $a + 1 > a$.[51] And then by letting Ω be the order type of all perfectly ordered classes, he proves that if b is an order type of a perfectly ordered class, then $b \leq \Omega$. Now this is enough for him to show that comparability fails. Substituting Ω for "a" and $\Omega + 1$ for "b", he obtains $\Omega + 1 > \Omega$ and $\Omega + 1 \leq \Omega$, a contradiction. Hence, comparability does not hold for order types, or the ordinal numbers in particular.

But as Moore and Garciadiego point out, Burali-Forti had confused Cantor's definition of a well-ordered set by omitting his third clause, which states that any finite or infinite set of elements of A which has a successor must also have an immediate successor.[52] Burali-Forti recognized his mistake in reading the 1897 continuation of "Contributions" where Cantor proved comparability for ordinals. In response, Burali-Forti published the short note "On Well-Ordered Classes" acknowledging that while every well-ordered class is perfectly ordered, the converse does not hold. Hence, Burali-Forti thought there was no conflict between the two opposing

[50] Moore and Garciadiego, p. 323.
[51] Here I use "class" interchangeably with "set", as many of the founders of set theory, including Russell, did. Where the distinction between sets and classes becomes significant, I will indicate it.
[52] Moore and Garciadiego, p. 323; Cantor, *Grundlagen*, p. 884.

results that he and Cantor had obtained.[53] But what is particularly interesting here with regard to the standard history of the paradoxes is that nowhere does Burali-Forti claim to have found a more general problem with Cantor's set theory. The only contradiction he discovers is the sort one would expect to see in any reductio proof.[54] He never made the further step often attributed to him that Cantor's theory itself somehow went astray collapsing into paradox. Indeed, neither did anyone else it seems until 1902. According to Moore and Garciadiego, Giulio Vivanti, who wrote the abstracts for Burali-Forti's papers, nowhere mentions the discovery of any sort of paradox. Furthermore, again contrary to the standard account of the paradoxes, there was no general outpouring of interest in these papers. In fact, until Russell, Vivanti's abstract appears to be the only reference to Burali-Forti's 1897 papers.[55]

In tracing the emergence of the Burali-Forti paradox at the hands of Russell, Moore and Garciadiego point to Russell's early Hegelianism as its source writing, "Russell began to search for paradoxes in mathematics much earlier than is usually recognized. His predisposition to invent such paradoxes had its roots in the philosophical antinomies of Kant and Hegel, both of whom deeply influenced his early development as a philosopher."[56] While it may indeed be true that Russell was more predisposed to or adept at finding paradoxes because of his philosophical background, it seems overly strong to say that he was intentionally searching for them. Moore and Garciadiego offer only the following passage from an unpublished 1896 essay to support their claim:

> The present article lays no claim to originality. From Zeno onwards, the difficulties of continua have been felt by philosophers, and evaded, with even subtler analysis, by mathematicians. But is seemed worth while to collect and define … some contradictions in the relation of continuous quantity to number, and

[53] Moore and Garciadiego, p. 323; Cantor, *Contributions*, pp. 150-51; Cesare Burali-Forti, "On Well-Ordered Classes", in van Heijenoort, pp. 111-2.
[54] In fairness to van Heijenoort, it should be pointed out that he also observes that Burali-Forti uses the paradox as part of a reductio ad absurdum argument to show that ordinal comparability fails to hold.
[55] Moore and Garciadiego, pp. 323-4.
[56] Ibid., p. 324.

> also to show, what mathematicians are in danger of forgetting, that philosophical antinomies, in this sphere, find their counterpart in mathematical fallacies."[57]

So while Russell was clearly sensitive to contradictions, we do not get the sense here that his aim in the philosophy of mathematics was to discover paradoxes. Instead, I would like to emphasize that Russell came upon the set-theoretic paradoxes, including the one that would bear his name, by considering some fairly intuitive, but unfortunately contradictory, features we might think the notion of a set to have. This aspect in the discovery of the paradoxes will be important throughout much of what follows in both this and later chapters. For it brings out that the foundational crisis in mathematics was a battle of intuitions about sets and that its resolution ultimately came down to largely pragmatic considerations. The paradoxes showed that a single notion of set incorporating all of the intuitive features both mathematical and philosophical logicians initially thought sets to have was impossible.

Russell's first published gesturing at paradox came from his explorations of Cantor's work rather than from Burali-Forti's. In 1896 he published a review of a book by French philosopher Arthur Hannequin that had criticized as contradictory Cantor's view of the continuum. Endorsing Hannequin's view, Russell wrote,

> For Cantor's second class of numbers, by which he hopes to exhaust continua, begins with the first number larger than any of the first class; but in the first class (the ordinary natural numbers) has no upper limit, it is hard to see how the second class is ever to begin. Cantor's attempts, indeed, seem to have proved, more conclusively than ever, that no legitimate extension of number can suffice for the adequate treatment of continua.

He then describes this difficulty as leading Hannequin to contradiction in the form of Kant's second antinomy that an indivisible element should necessarily be divisible, or every composite substance both is and is not divisible. "This is only our old friend, Kant's second antinomy," he

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[57] Bertrand Russell, "On Some Difficulties of Continuous Quantity", in *The Collected Paper of Bertrand Russell*, vol. 2, Nicholas Griffin and Albert C. Lewis, eds. (New York: Routledge, 1990), p. 46.

concludes, "but it acquires a new force by the proof of its inherence in mathematical method."[58] Cantor's set theory continued to weigh on Russell's mind, and by 1899 he had developed the antimony of the infinite number, described in an outline of *Principles of Mathematics*:

> Chapter VII. Antinomy of Infinite Number. This arises most simply from applying the idea of a totality to numbers. There is, and is not, a number of numbers. This [and] causality are the only antinomies known to me. This one is more all-pervading…. No existing metaphysic avoids this antinomy.[59]

He was now very close to discovering what would become Cantor's paradox of the largest cardinal, and it was in this context that he also came upon the Burali-Forti paradox.

Initially, Russell described what he thought to be errors in Cantor's work writing on December 8, 1900 to the French philosopher Louis Couturat,

> I have discovered a mistake in Cantor, who maintains that there is no largest cardinal number. But the number of classes is the largest number. The best of Cantor's proofs to the contrary can be found in Jahresb. D. deutschen Math. Ver'g., I, 1892, pp. 75-8 [Cantor 1891]. In effect it amounts to showing that, if u is a class whose [cardinal] number is α, the number of classes included in u (which is 2^{α}) is larger than α. The proof presupposes that there are classes included in u which are not individuals [members] of u; but if u=Class, that is false: [for] every class of classes is a class.[60]

Russell took "Class" as denoting the class of all classes, so if u is the class of all classes, then there could not possibly be a class with a greater number of classes as members. All classes are members of Class, and it was Cantor's mistake not to acknowledge this and see the consequences.[61] As Moore and Garciadiego observe, Russell did not yet draw out the conclusion from the set of all sets that there was a contradiction in Cantor's theory but only that Cantor's theorem—that the cardinality of the set of all subsets of u is strictly greater than the cardinality of u itself—is not generally applicable, specifically in the case of the universal class. It was not

[58] Bertrand Russell, "Review of *Essai critique sur la hypothèse des atmomes dans la science contemporaine*", in *Collected Papers*, vol. 2, p. 37; Moore and Garciadiego, pp.324-25.
[59] Quoted in Moore and Garciadiego, p. 325.
[60] Quoted in ibid., pp. 325-6.
[61] As we will see Cantor did not allow for a class of all classes. Some commentators have taken this to be a result of Cantor having already discovered the paradox of the greatest cardinal. At the very least, this does not seem entirely clear-cut. Cantor held a variety of philosophical and religious views that may have led him to deny the existence of a universal class whether it led to contradiction or not.

until he discovered his own paradox of the class of all classes which are non-self-membered that he was led to the other paradoxes of set theory.

It was however in this context that Russell first became aware of Burali-Forti's argument against comparability of ordinals. Included in his response to Russell's comments on Cantor, Couturat reported on Burali-Forti's result that comparability fails for ordinals, remarking that "[h]is reasoning is more specious than convincing." But he did not stop here, going on also to question Russell's own class of all classes wondering "whether one can consider the class of all possible classes without some sort of contradiction."[62] Russell responded agreeing with Burali-Forti's result adding that he suspected comparability also failed for cardinals, though he offered no reason for either claim. In contrast he did not back away from his class of all classes arguing against Couturat,

> If you grant that there is a contradiction in this concept, then the infinite always remains contradictory, and your work as well as that of Cantor has not solved the philosophical problem. For there is a concept Class and there are classes. Hence Class is a class. But one can prove (and this is essential to Cantor's theory) that every class has a cardinal number.

But Russell added that no contradiction arose with regard to the largest cardinal since Cantor's theorem did not apply to the class of all classes. He offered no further explanation of this claim, and Couturat continued to question the concept Class asking in his January 27, 1901 response, "Is the class Class determined, closed so to speak, in such a way as to possess a cardinal number?" Russell replied that it was well-defined since for any x it either belonged to Class or not. He further remarked that he had now received and read Burali-Forti's papers.[63]

Russell arrived at his own paradox by May 1901, which he discovered, according to his account in *Principles*, "in the endeavour to reconcile Cantor's proof that there can be no greatest cardinal number with the very plausible supposition that the class of all terms…has necessarily

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[62] Moore and Garciadiego, p. 326.
[63] Ibid., p. 327-28.

the greatest possible number of members," a term being "[w]hatever may be an object of thought, or may occur in any true or false proposition, or can be counted as *one*…."[64] Moore puts forth this date for the discovery based upon an early manuscript of *Principles* where in Chapter III Russell composes the first extant account of his paradox:

> The axiom that all referents with respect to a given relation form a class seems, however, to require some limitation…. We saw that some predicates can be predicated of themselves. Consider now those (and they are the vast majority) of which this is not the case. These are the referents (and also the relata) in a certain complex relation, namely the combination of non-predicability with identity. But there is no predicate which attaches to all of them and to no other terms. If it is predicable of itself, it is one of those referents by relation to which it was defined, and therefore, in virtue of their definition, it is not predicable of itself. Conversely, if it is not predicable of itself, then again it is predicable, and therefore again it is predicable of itself. This is a contradiction, which shows that all referents considered have no common predicate, and therefore do not form a class…. It follows from the above that not every definable collection of terms forms a class defined by a common predicate.[65]

Here we see not only Russell's first statement of the paradox, but also, we get the first indication of where its resolution will ultimately rest. In observing that "the axiom that all referents with respect to a given relation form a class" needs limiting, Russell gives up the traditional conception of what a class is—that every predicate or concept determines a class. As we will see, this idea of a set was found throughout the emerging discipline of set theory including that work of Cantor. Locating the source of the paradox in the comprehension principle was to change the development of set theory in crucial ways and in particular was to give rise to the philosophical debates over set theory that we will primarily be concerned with in many of the following chapters. Moore also notes that Russell surprisingly does not also state the paradox in terms of classes despite of his own later admission that he had discovered his paradox by considering Cantor's theorem and the cardinality of the class of all classes.[66] But perhaps this omission is not quite so strange given Russell's preference for an intensional basis for his logic. The paradox

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[64] Bertrand Russell, *Principles of Mathematics*, 2nd ed., (London: George Allen and Unwin Ltd., 1937), p. 101. The first edition of *Principles* appeared in 1903. He defines the terms on p. 43.
[65] Gregory H. Moore, "The Origins of Russell's Paradox: Russell, Couturat, and the Antinomy of Infinite Number", in *Essays on the Development of the Foundations of Mathematics*, ed. Jaakko Hintikka, (Dordrecht: Kluwer Academic Publishers, 1995), pp. 233-4.
[66] Ibid., p. 234.

stated in terms of predicates non-predicable of themselves would for him rock the entire foundation of everything else that he had constructed from it, including the theory of classes, and perhaps even more devastating, of mathematics in general.

Russell's reaction, however, was not immediately one of concern. It seems that that he reported the paradox to no one else—except Peano, who never responded—until June of 1902 when he communicated it to Frege. Frege, in return, responded,

> Your discovery of the contradiction caused me the greatest surprise and, I would almost say, consternation, since it has shaken the basis on which I intended to build arithmetic. It seems, then, that transforming the generalization of an equality into an equality of courses-of-values (§ 9 of my *Grundgesetze*) is not always permitted, that my Rule V (§ 20, p. 36) is false, and that my explanations in § 31 are not sufficient to ensure that my combinations of signs have a meaning in all cases. I must reflect further on the matter. It is all the more serious since, with the loss of my Rule V, not only the foundation of my arithmetic, but also the sole possible foundations of arithmetic, seem to vanish.[67]

Moore reasonably suggests that Russell was not initially disturbed by his paradox because he thought there must be some simple solution to it. To support this view, Moore turns again to Russell's correspondence with Couturat, quoting Russell from September 29, 1902, "When my book began to be printed, I believed I could avoid these contradictions, but now I see that I was mistaken, a fact which greatly diminishes the value of my book."[68] It was only in receiving Frege's pessimistic response that Russell came to see the devastating consequences of his discovery. He looked upon Frege with extremely high regard, and when he could offer no simple resolution, Russell could no longer regard this as a minor difficulty to be solved only in time. For Russell, the foundational crisis had arrived.

From this point, matters became worse. Moore and Garciadiego point to Russell's recognition of the seriousness of his own paradox as the source of the other set-theoretic paradoxes. Reconsidering the work of Burali-Forti and of Cantor yielded his discovery of the two paradoxes that would eventually bear each of their names. In a 1902 article on well-ordering,

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[67] Gottlob Frege, "Letter to Russell", (1902), in van Heijenoort, pp. 127-8.

[68] Moore, "Russell's Paradox", p. 234-5; see also Moore and Garciadiego, pp. 328-9.

Russell began his attempts at reconciling both the work of Burali-Forti and Cantor, now accepting Cantor's proof of the comparability of ordinals but doubting his claim that every set can be well-ordered. Russell's response at this point was to grant that every ordinal segment was well-orderable but not that the ordinals as a whole were. In this way, he continued to avoid the conclusion that either of Burali-Forti or Cantor had landed themselves in contradiction.[69]

The 1903 publication of *Principles of Mathematics* saw this situation change. Here, in addition to an entire chapter on his own paradox, Russell for the first time in print put forward both the Burali-Forti and Cantor paradoxes.[70] As already mentioned above, Russell claims to have discovered his own paradox in considering Cantor's theorem. In Chapter X of *Principles* entitled "The Contradiction", he rehearses his paradox in three different forms. He first states it in terms of predicates observing that if x is a predicate, then x may or may not be predicable of itself. Now, assuming "not predicable of itself" is a predicate, if this predicate is predicable of itself then it is not predicable of itself, and if this predicate is not predicable of itself then it is predicable of itself. Since in either case the opposite follows, we have a contradiction. Similarly, Russell says that a class-concept may or may not be a term of its own extension, and so considering the class-concept "class-concept which is not a term of its own extension", again from either case, the opposite follows. Finally, he states the contradiction in terms of classes remarking that here it "appears even more extraordinary." He begins by stating that a class as one, that is, a class treated as a single entity such as the human race, may be a term, or element, of itself as many, that is, a class treated as a plurality of object such as all humans. On this view, then, the class of all classes is a class; similarly, the class of all terms that are not men is not a

[69] Moore and Garciadiego, pp. 329-30.
[70] Some scholars have traced the Cantor paradox to the 1899 Cantor-Dedekind correspondence, but, as we will see, this is not entirely accurate. Cantor does produce a contradiction there, but much like Burali-Forti, only in the service of a reductio proof. It was Russell who turned these contradictions into a more general foundational crisis for mathematics.

man. In both such cases, the class as one is not a member of itself as many. He now poses the crucial question, “Do all the classes that have this property [being a class, which as one, is not a member of itself as many] form a class? If so, is it as one a member of itself as many or not?” Assume that it is as one a member of itself as many. But then it would have to be a many to satisfy the requisite property, so it is not. But then if it is not as one a member of itself as many, then it does satisfy the requisite property, so it is a member as one of itself as many. Thus, in either case the opposite follows, and Russell has arrived at the first published version of his class paradox.[71] His earlier version stated in his letter to Frege was the now more familiar version stated in terms of the class of all classes which are not members of themselves.

In diagnosing the paradox, he again turns to the comprehension principle, the axiom that any propositional function in one free variable determines a class. He observes that either this principle or the principle that every class can be treated as a single term must be false but sees no fundamental objection to rejecting either. “But having dropped the former…,” Russell asks, “Which propositional functions define classes which are single terms as well as many, and which do not? *And with this question our real difficulties begin*.”[72] This, as we will continue to see, is indeed the crucial issue to set theory in the early twentieth century and in a sense, remains to this day one of the crucial issue for set theory in its more philosophical aspects.

Having spelled out his own paradox, Russell now returned to the work of Burali-Forti and Cantor seeing for the first time that their views also lead him to paradox, though of a less general nature than his own. Russell first turns to consider Burali-Forti remarking that there appears to be a problem with the order type of the whole series of ordinals. We can easily prove that every segment of this series is well-ordered, and so it is natural to suppose that the whole

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[71] Russell, *Priciples*, p. 102.
[72] Ibid., pp. 102-3; my emphasis.

series is also well ordered. The type of this series would be the greatest of all the ordinals since the ordinals less than a given ordinal form in order of increasing magnitude a series whose type is the given ordinal. But every ordinal can be increased by the addition of one, so there can be no greatest ordinal. From this contradiction, Russell correctly remarks, Burali-Forti concluded that for any two given ordinals, comparability does not in general hold. Russell also observes—though he does not indicate whether he takes the perfectly ordered and well-ordered set to coincide or whether he reapplied Burali-Forti's original argument for perfectly ordered sets to Cantor's well-ordered sets—that Cantor proved exactly the opposite.[73] And here Burali-Forti's paradox was born.

Russell finds no weakness in Cantor's proof, and instead questions a premise of Burali-Forti that the whole series of ordinals is well-ordered. From the well-ordering of every segment of ordinals, Russell claims that the well-ordering of the whole series does not follow. Since this claim of the well-ordering of the entire series of ordinals appears incapable of proof, and rejecting it avoids Burali-Forti's paradox, Russell follows this path and at least for the moment takes this to be the successful resolution of the paradox of the largest ordinal.[74] What Russell does not see is that Cantor nowhere explicitly admits a class of all ordinals, and without this there is no contradiction. What drives Russell to the paradox is his perhaps commonplace view that the ordinals themselves should form a class. Had there been no paradox, it seems no more surprising to have a class of all ordinals than it is to have a class of all naturals or reals. After all, much of Cantor's urgings for the acceptance of the transfinite relied on that fact that these numbers behaved in ways very similar to the already familiar sorts of numbers.

[73] Russell, *Principles*, p. 323.
[74] Russell, *Principles*, p. 323.

In introducing his final set-theoretic paradox, Russell turns to the founder of set theory himself considering Cantor's investigations into the cardinal numbers. He begins by remarking that the common objections to infinite numbers, classes, series, and the infinite in general as self-contradictory are groundless, but more serious problems related to his own paradox of non-self-membered classes remain. This is not a problem with the infinite as such but only with certain large infinite classes. The difficulty is as follows. Cantor proved that there is no greatest cardinal number: if u is a class, then the number of classes included in u is greater then the number of terms, or elements, in u. Or, in other words, for any cardinal α, $2^{\alpha} > \alpha$. This, as we have seen, is Cantor's theorem. But Russell observes that certain classes appear to have as many terms as possible, such as the class of all terms, the class of all classes, or the class of all propositions. So it seems, to Russell, that Cantor's proof must make some assumption that does not hold for such cases since definite contradictions arise in applying Cantor's reasoning to such cases. Indeed, such difficulties arise generally for any case that deals with the class of all entities or any equally numerous class. Russell admits that in light of this problem we might be tempted to think that the totality of things, or the whole universe of existing entities, is an illegitimate totality, inherently contradictory to logic. "But it is undesirable to adopt so desperate a measure," he counters, "so long as hope remains of some less heroic solution."[75]

Russell first presents versions of both Cantor's 1874 proof and of his 1897 proof that there is no greatest cardinal. Although he finds the first proof specious, he remarks of the second—which includes the famous diagonal argument—that it "appears to contain no dubitable assumption. Yet there are certain cases in which the conclusion seems plainly false."[76] First, he considers the class of all terms assuming that every constituent of a proposition is a term so that

[75] Ibid., p. 362.
[76] Ibid., pp. 363-7.

the classes are only some among the terms. And conversely, since there exists for every term, a class consisting only of that term, there is a one-to-one correlation of all terms with some classes. Then by the Schröder-Bernstein theorem, there is an equal number of classes and terms. Now admitting the class of all objects of whatever kind, the classes of objects will be only some among the objects. But according to Cantor's argument, there are more classes of objects than there are objects. Hence, contradiction. Russell then goes on to spell out the contradiction similarly in terms of the class of all propositions and then in terms of the class of all propositional functions.[77] In all of these cases where we have a class of objects that is presumably the same size as the class of objects, we are able, by Cantor's argument, to produce a class of greater size. And here we have for the first time in print Cantor's paradox.

Russell closes his discussion of these paradoxical results by looking at some attempted correlations between such large classes and the class of all terms. Russell claimed that he was led to the discovery of his own paradox by considering Cantor's argument.[78] Taking a closer look at the attempted correlation between terms and classes helps to elucidate how this might have occurred. Russell looks at all terms and says if x is not a class, we should correlate it with its singleton class ιx. But if x is a class, we just correlate it with itself. Then according to Cantor's argument, the class which should be omitted from the correlation is the class w of all non-self-membered classes. This being a class, however, should mean that it correlates with itself. But w is the contradictory class of Russell's paradox since it both is and is not a member of itself. Hence, we see how Cantor's argument again leads to a contradiction and in particular how Russell was led from considering Cantor to the discovery of his own paradox.[79]

[77] Ibid., pp. 366-7; Russell's objects are a slightly broader class than the terms in that they also include pluralities (*Principles*, p. 55 fn.).
[78] Ibid., p. 101; Bertrand Russell, *My Philosophical Development*, (New York: Routledge, 1993), p. 58.
[79] Russell, *Principles of Mathematics*, p. 367.

Russell concludes these investigations observing that though Cantor's argument leads to contradictions where such large classes are concerned, he can find no missteps in Cantor's original proof. The only solution he sees at this point is to deny that there are any true propositions concerning all objects or propositions. "Yet the latter, at least, seems plainly false," he thinks, "since all propositions are at any rate true or false, even if they have no other common properties. In this unsatisfactory state, I reluctantly leave the problem to the ingenuity of the reader."[80] Though for Cantor, as we will see, it seemed plainly true that there is no universal class. I turn now to consider how these difficulties were in fact dealt with by Russell and others.

II

I first turn to Cantor, who is interesting both as the founder of set theory and as someone who may in fact never have been confronted by such problems as Russell introduced. For while Russell saw it as obvious that such large classes as the class of all classes and the class of all objects should exist, Cantor—for is own philosophico-theological reasons—saw this as equally implausible. Much of this comes out in his correspondence with Dedekind, which has often been erroneously cited as the source of the Cantor paradox. But as we will see, on Cantor's view of a class, we are unable to derive such paradoxes. Instead, much like Burali-Forti, the only contradictions Cantor derives are the sorts one would ordinarily and unsurprisingly expect to find in reductio proofs.

The first occurrence of Cantor's paradox has often been traced to his 1899 correspondence with Dedekind, but this interpretation presents two problems—the one just mentioned in the previous paragraph and the other that Cantor's paradox was not publicly known before 1932 when the relevant pieces of the Cantor-Dedekind correspondence were published.

[80] Ibid., p. 368.

As we have seen, Russell was ultimately responsible for the contradiction that would bear Cantor’s name. There are, however, still interesting and relevant aspects of this correspondence for the set-theoretic paradoxes, and in particular, for understanding how they were resolved.

Cantor initiated his 1899 correspondence with Dedekind the end of July writing that he would like them to be regular correspondents so that he may have Dedekind’s views on certain fundamental questions of set theory.[81] In particular, Cantor wrote on August 3 to Dedekind presenting his proof that every transfinite set has a definite cardinal and then concluding that the system (tav) of all alephs is the system of all transfinite cardinal numbers, that is, all transfinite cardinals are alephs. But before proving this result, Cantor introduces what he sees as a crucial distinction, that between consistent and inconsistent multiplicities. He explains that he begins with the notion of a definite multiplicity (or system or totality) of things but then observes that the assumption that a multiplicity’s elements “are together” can lead to a contradiction in certain cases. So contrary to our original thought, such a multiplicity cannot be conceived as a unity, or “one finished thing”, and these multiplicities he says are the absolutely infinite or inconsistent multiplicities. He observes “[a]s we can readily see, the ‘totality of everything thinkable’, for example, is such a multiplicity; later still other examples will turn up.”[82] We should note, briefly for now as it is a point I will often return to, that not everyone did readily see this. Russell, of course, did not and thought it completely commonsense that there is a collection of all things thinkable and the desire to preserve such an intuition is what led him to the paradoxes. What we will see is that the systemization of set theory has largely been and remains a battle of competing intuitions about what sets are like.

[81] Georg Cantor, Richard Dedekind, and David Hilbert, “Cantor’s Late Correspondence with Dedekind and Hilbert”, in *From Kant to Hilbert: A Source Book in the Foundations of Mathematics*, vol. II, William Ewald, ed., (Oxford: Clarendon Press, 1999), pp. 923-40.
[82] Ibid., pp. 931-2.

In contrast, Cantor also observes that there are collections where the elements can be thought as “being together” without contradiction and so can be gathered into “one thing”. These he says are the consistent multiplicities or sets. He then goes on to note that if two multiplicities are equivalent, then they are both either sets or inconsistent, and every submultiplicity of a set is also a set. Also, for any set of sets, the elements of these sets also form a set.[83]

Using this distinction between consistent and inconsistent multiplicities, Cantor now begins his proof that every transfinite cardinal is an aleph. First he considers the system Ω of all ordinals noting that he had earlier proved that any two ordinals are comparable.[84] This multiplicity when naturally ordered according to magnitude forms a well-ordered sequence

$$0, 1, 2, 3, \ldots, \omega_0, \omega_0 + 1, \ldots, \gamma, \ldots$$

in which every number is the order type of the preceding sequence of elements. Now he shows that Ω is not a consistent multiplicity. Assume Ω is consistent. Then since it is well-ordered, it corresponds to some ordinal δ greater than all the ordinals in Ω. But δ is also in Ω since Ω contains all the ordinals. So we have $\delta < \delta$, a contradiction. Therefore, Ω is an inconsistent, or absolutely infinite, multiplicity.

Cantor then goes on to explain that since the similarity of well-ordered sets also yields their equivalence, that is, they have the same cardinality, to every ordinal γ there corresponds a definite cardinal $\mathfrak{c} = \bar{\gamma}$, where $\bar{\gamma}$ is the general notion applying to all ordered sets similar to γ. That is, $\mathfrak{c}$ is the cardinal of any well-ordered set with type $\bar{\gamma}$. As we saw in chapter one, he denotes the cardinals corresponding to the transfinite ordinals by “$\aleph_n$”, aleph, the first letter of

[83] Ibid., p. 932.
[84] Cantor actually considers Ω', which he takes as the ordinals plus zero. For simplicity, I will just take the ordinals to include zero.

the Hebrew alphabet. He explains here to Dedekind that the system of ordinals γ corresponding to one and the same cardinal $\mathfrak{c}$ forms “a number class”, $Z(\mathfrak{c})$. In every such number class there is a least ordinal γ_0, and there is also a first ordinal γ_1 falling outside this number class $Z(\mathfrak{c})$ such that the condition $\gamma_0 < \gamma < \gamma_1$ is equivalent to the fact that γ belongs to $Z(\mathfrak{c})$. Therefore, every number class is a definite segment of the sequence Ω. Finally, he notes that certain ordinals of the system Ω each by itself form a number class. For example, to the finite ordinals

$$0, 1, 2, 3, \ldots, \nu, \ldots,$$

there corresponds the finite cardinals

$$\overline{0},\ \overline{1},\ \overline{2},\ \overline{3}, \ldots,\ \overline{\nu}, \ldots.$$

Now, let ω_0 be the least transfinite ordinal and let $\aleph_0$ be the cardinal corresponding to it, so that $\aleph_0 = \overline{\omega_0}$. So $\aleph_0$ is the least aleph and determines the number class $Z(\aleph_0) = \Omega_0$. The ordinals α of Ω_0 satisfy the condition $\omega_0 < \alpha < \omega_1$ and are characterized by it. ω_1, then, is the least transfinite ordinal whose cardinal does not equal $\aleph_0$. So $\omega_1 = \aleph_1$, and $\aleph_0 \neq \aleph_1$, the next greater aleph, and so on. He concludes by observing that among the transfinite ordinals of Ω, there is a least ordinal to which no $\aleph_\nu$ corresponds, where ν is finite, which he denotes “$\omega_{\omega 0}$”. This yields an aleph $\aleph_{\omega 0}$, which is the next greatest cardinal after all the $\aleph_\nu$. This process of forming alephs and number classes of Ω “that correspond to them is *absolutely* limitless.”[85] The system of all alephs he denotes by “ ”, tav, the twenty-second letter of the Hebrew alphabet, and he can now show that it too is an inconsistent, or absolutely, infinite totality. He observes that the system of all alephs ordered according to magnitude,

$$\aleph_0, \aleph_1, \ldots, \aleph_{\omega 0}, \aleph_{\omega 0+1}, \ldots, \aleph_{\omega 1}, \ldots,$$

[85] Cantor-Dedekind-Hilbert, p. 933-4; Cantor’s italics.

forms a sequence similar to the system Ω and so, is also inconsistent.[86]

Cantor's final result in this letter answers the question "are there transfinite cardinals not contained in the system ?" He answers negatively based upon the inconsistency of Ω and . If we define a multiplicity V and assume no cardinal corresponds to it, then V must be inconsistent. By the assumption, Ω must be projectible into V, so there must be a submultiplicity V' of V equivalent to Ω. V', then, is inconsistent since Ω is, so the same must hold of V. Therefore, he concludes, every consistent transfinite multiplicity must have a definite aleph as its cardinal, and so all transfinite cardinals are contained in the system . And from this he also concludes that his 1895 assertion that comparability of cardinals was correct.[87]

In an editorial note to this letter, Zermelo observed a weakness in Cantor's proof, namely that he had not proved that Ω was in fact projectible into every V lacking an aleph as its cardinal. Something like an intuition of time seemed to be at play here with Cantor continuing a process of successively and arbitrarily matching elements of V with the ordinals of Ω. But this, however, assumes that the elements of V will each be used only once in the process, and such a process, Zermelo remarks, "goes beyond all intuition". What Cantor needed was Zermelo's axiom of choice, so that he could make a simultaneous choice of elements from V to define V'.[88] It was this axiom that eventually allowed Zermelo to prove what Cantor had wanted.[89]

This weakness in Cantor's proof here, however, does not obscure my main concern. What is important to note is that, first, while something like Cantor's paradox does appear in this letter to Dedekind, it is all part of a reductio proof. Cantor's paradox did not result in a

[86] Ibid., p. 934.
[87] Ibid., pp. 934-5.
[88] Ibid., p. 935, fn. b.
[89] Ernst Zermelo, "Proof that Every Set can be Well-ordered" (1904) in van Heijenoort, pp. 139-41. Due to controversy over his used of the axiom of choice, he reproved the result, again using AC, in 1908 also contained in van Heijenoort as "A New Proof of the Possibility of a Well-ordering", pp. 183-98.

foundational crisis because this contradiction was what Cantor intended to prove for reductio so that he could obtain his results about the transfinite cardinals. But then why did Russell react so differently to the paradoxes? And this is the second important point in Cantor's letter. In his distinction between consistent and inconsistent totalities, Cantor had a response to the paradoxes before any foundational crisis arose. But what were the grounds for such a distinction? It is not exactly clear when Cantor introduced it. One possibility is that he did in fact first discover the set-theoretic paradoxes and then introduced it as an ad hoc solution to them. If this is the case, Cantor's solution to the paradoxes was very much on par with the other solutions offered later. As we will see in the following chapters, they were all in a sense introduced on pragmatic grounds with the aim of preserving as much of Cantor's set theory as possible. There does not, however, seem to be much evidence for this as an account of Cantor's thinking on the paradoxes.

Another possibility, and one that would be much more welcome to set theorists who think there is a single correct, or intended, notion of a set, is that Cantor in fact thought there was something inherent to what a set is, that called for such a distinction. There does seem to be some evidence for this view. But it again will not be especially satisfying to most set theorists. Joseph Dauben has shown that Cantor worried greatly over whether his views of the infinite were consistent with Catholic dogma—whether knowledge of the infinite was to claim the kind of knowledge that only God could have. The acknowledgement of absolutely infinite totalities alleviated all such worries since their inconsistency showed that we did not in fact have knowledge of such infinities. Human understanding was limited to infinities that fell short of Ω or   or the system of all things thinkable.[90]

[90] Dauben, chapters ten and eleven. He provides various references for Cantor's philosophico-theological views of set theory, but for his published views see especially "Mitteilungen zur Lehre vom Transfiniten" in Cantor, *Gesammlelte,* pp. 378-439.

While the possibly ad hoc nature of such a distinction between consistent and inconsistent totalities did not bother the likes of Hilbert and Dedekind (Cantor did not reveal the religious underpinnings to them), the unclarity of it did. Cantor had introduced this distinction as early as September 26, 1897 in a letter to Hilbert communicating an early version of his proof that all transfinite cardinals are alephs. Indeed, as mentioned above, while most scholars trace Cantor's paradox to the 1899 letter to Dedekind, this letter to Hilbert seems to be the earliest evidence of something like Cantor's paradox. Here he wrote, we are first to observe that the totality of all alephs "cannot be conceived as a determinate, well-defined, *finished* set."[91] Otherwise, this totality would be followed in size by a determinate aleph which would both belong and not belong to the totality hence yielding a contradiction. Much as in his letter to Dedekind, he then uses this contradiction in a reductio proof to show that all transfinite cardinals are alephs. While we do not have Hilbert's responses to Cantor, Cantor's October 2, 1897 response suggests that Hilbert did not fully grasp the distinction Cantor wished to make. He begins his letter by immediately observing that Hilbert overlooked the characteristic of being a finished set. He then goes on to again explain his proof remarking:

> One must only understand the expression 'finished' correctly. I say of a set that it can be thought of as *finished* (and call such a set, if it contains infinitely many elements, 'transfinite' or 'super-finite') if it is possible without contradiction (as can be done with finite sets) to think of *all its elements as existing together*, and so to think of the set itself as *a compounded thing for itself*; or (in other words) if it is *possible* to imagine the set as *actually existing* with the totality of its elements.

He then notes that "the 'transfinite' coincides with what has since antiquity been called the 'actual infinite'."[92] He explains that what he intends by a set is "an 'assembling together' [which] is only possible if an '*existing together*' is *possible*." He concludes by contrasting this with the absolutely infinite sets:

[91] Cantor-Dedekind-Hilbert, p. 926; Cantor's italics.
[92] Ibid., p. 927; Cantor's italics.

> Infinite sets such that the *totality* of their elements cannot be thought of as 'existing together' or as a 'thing for itself' … and that therefore also *in this totality* are absolutely not an object of further *mathematical* contemplation, I call '*absolutely infinite* sets', and to them belongs the 'set of all alephs'.[93]

Whether Hilbert ever understood Cantor's distinction, we do not know, but we do know that Dedekind was equally puzzled by it.

In his response to Cantor's proof that every transfinite cardinal was an aleph, Dedekind wrote,

> You will certainly sympathize with me if I frankly confess that, although I have read through your letter of 3 August many times, I am utterly unclear about your distinction into consistent and inconsistent; I do not know what you mean by the 'coexistence of all elements of a multiplicity', and what you mean by its opposite. I do not doubt that with a more thorough study of your letter a light will go on for me; for I have great trust in your deep and perceptive research.[94]

As we saw in chapter one with regard to his views of cardinal numbers and abstraction, Cantor was not afraid to bring certain mentalistic elements into his set theory. And much like Frege, the precise mathematical minds of Dedekind and Hilbert were unable to comprehend what exactly Cantor's analogy came to—how it was to yield objects precise enough for mathematical contemplation.[95] There seem to be both physicalistic and mentalistic aspects of Cantor's view of a set. He speaks of collecting together elements into a set as a sort of pseudo-physical process that works much like the collecting together of any other group of physical objects. So long as we can imagine such a process, we have an existing together of element and so a consistent totality, or set. However, a group of physical objects existing together is much easier to make sense of than say the cardinal or ordinal numbers. For physical objects we might just stand them all together, and then we see that they can in fact exist simultaneously together. Having actually done just such a thing many times, it is not much harder to imagine such a process when we literally cannot bring these objects all together to one place. But then why would something

[93] Ibid., p. 928; Cantor's italics.
[94] Ibid., p. 937.
[95] For a defense of Cantor's views see William Tait, "Frege Versus Cantor and Dedekind: On the Concept of Number," in *The Provenance of Pure Reason: Essays in the Philosophy of Mathematics and Its History,* (New York: Oxford University Press, 2005), pp. 212-51.

analogous fail for all of the cardinals? It is not immediately obvious why we cannot imagine these objects existing together, as Cantor says we cannot. The set of all cardinals turns out inconsistent on Cantor's view, and so in a sense maybe we cannot in fact think of these objects as existing together. But it is unclear why the analogy with physical objects breaks down. For those like Dedekind, Hilbert, and Russell, who were not already inclined towards Cantor's view, the analogy of collecting things together in the mind does not help.

Looking at Cantor's published views of what he considered a set yield no further information as to what principle decided between consistent and inconsistent totalities. In his *Contributions* of 1895 he defines a set as "any collection into a whole M of definite and separate objects m of our intuition or our thought."[96] So here again, he relies on this apparently mentalistic collecting together. Earlier, in a footnote to the *Grundlagen* of 1883, Cantor did elaborate some this idea of a collecting together:

> Theory of manifolds. I use this word to designate a very broad theoretical concept which I have hitherto used only in the special form of a theory of geometric or arithmetical sets. In general, by a 'manifold' or 'set' I understand every multiplicity which can be thought of as one, i.e. every aggregate of determinate elements which can be united into a whole by some law.

So here again he appeals to an ability of out thought as determining which sets exist. But he does give us something further in this passage in stating that the sets are those aggregates whose "elements …can be united into a whole by some law." Ferreirós suggests that Cantor has something like the comprehension principle in mind here, that sets are determined as the extensions of concepts or predicates. In using "Inbegriff" for a collection, Cantor draws the connection between a concept (Begriff) and a set. The rule or law by which the set is determined is the property that must hold of an object for it to be an element of the set. Indeed, Ferreirós convincingly argues that this notion of a set runs throughout the early history of set theory

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[96] Cantor, *Contributions*, p. 85.

resulting from the founders' shared philosophical education and provides a somewhat unified view of what constituted a set prior to the paradoxes. For them the move to set theory from traditional logic was a natural one, having all been well-educated in Aristotle's logic with its distinction between concepts and their extension. To the extent that there ever was an intended intuitive notion of a set it was as the extension of a concept.[97]

But this somewhat more precise account of what a set is does not make Cantor's distinction any clearer. Comprehension alone does not determine which collections are consistent and which are not. Indeed, it was precisely comprehension that led both Russell and Frege into contradiction. So again we are left only with Cantor's mentalistic intuitions about which sets exist.

Furthermore, even though Cantor did not provide the religious underpinnings of his distinction to Dedekind and Hilbert, it seems unlikely that this would have helped to clear up matters. Certainly, Russell would not have accepted these grounds as the basis for a distinction between the transfinite and the absolutely infinite convinced as he was that the collection of all things thinkable was in fact thinkable. What we see arising in all this is a dispute of intuitions over what sets are like. What was needed was some definite criterion for set existence which did not lead to contradictions in the theory. This is what Zermelo and Russell provided.

In 1908 Zermelo presented an axiom system for set theory that was to retain everything of value in the theory while excluding the paradoxes.[98] Moore has argued that Zermelo's real concern in this paper was to present rigorously the principles required for proving his famous but, at the time, controversial well-ordering theorem.[99] Cantor had claimed it as a law of thought that

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[97] Ferreiros, pp. 48-53, 263-7.
[98] Ernst Zermelo, "Investigations into the Foundations of Set Theory I", in van Heijenoort, pp. 199-215.
[99] Gregory H. Moore, "The Origins of Zermelo's Axiomatization of Set Theory", *Journal of Philosophical Logic* 7, (1978), pp. 307-29.

every set can be well-ordered, but such a claim did not appear self-evident to everyone.[100] Zermelo finally proved this claim in 1904 generating a great deal of controversy among mathematicians and mathematically informed philosophers by his explicit use of the axiom of choice, a principle which he rightly claimed was implicitly used throughout mathematics. Attempting to settle this dispute, he then reproved his theorem in 1908.[101] He quickly followed this result with the publication of his axiom system for set theory. Moore has argued that Zermelo's real purpose in this paper was to make explicit the principles used in his well-ordering proof and thus to further quell the debates over the axiom of choice. While there does seem to be some truth to this claim, Moore's emphasis of it encourages the skewed view discussed at the outset of this chapter—that the paradoxes were purely the concern of philosophers and that mathematicians were not troubled by them. This then further encourages the view that there were always two conceptions of a set, one mathematical and the other logical, or philosophical, where only the latter ended up in contradiction. As we just saw in the case of Cantor, this issue was not so clear cut, and as we will soon see, the situation was similar for Zermelo. Since its inception, set theory has been inextricably intertwined with philosophy throughout its history.

While it is true, as Moore notes, that Zermelo does not spend nearly as much time considering the paradoxes as Russell does in his proposed solution, he does set out his paper with the aim of removing such difficulties from the theory. What is in fact more striking in light of Moore's reinterpretation is that Zermelo nowhere mentions the proof of his well-ordering theorem. In opening "Investigations", he first indicates what set theory as a mathematical discipline is, stating:

> Set theory is that branch of mathematics whose task is to investigate mathematically the fundamental notions of 'number', 'order', and 'function', taking them in their pristine, simple form, and to develop

[100] Cantor, *Grundlagen*, p. 886.
[101] Zermelo, "New Proof".

> thereby the logical foundations of all arithmetic and analysis; thus it constitutes an indispensable component of the science of mathematics.[102]

He then immediately turns to the threat of the paradoxes:

> At present, however, the very existence of this discipline seems to be threatened by certain contradictions, or 'antinomies', that can be derived from its principles—principles necessarily governing our thinking, it seems—and to which no entirely satisfactory solution has yet been found.[103]

This passage raises some interesting points. First, it runs clearly contrary to Moore's claim that the true purpose of Zermelo's axiom system was the exposition of the well-ordering theorem rather than a solution to the paradoxes. And more generally, it shows one of the leading mathematical set theorists of his day placing their resolution at the center of his concerns. His use of the phrase "seems to be threatened", however, may suggest that he thought this threat only an apparent one. But it we can also read this as an indication of Zermelo's solution. Given that he believes his axioms to now exclude the paradoxes, the threat is indeed now only a seeming one. Yet, he then would also count his very own solution to be among others which have not been entirely satisfactory. But this too can be explained by what he goes on to say about earlier notions of set.

From Russell's paradox especially, he explains, it seems no longer possible for each logically definable notion to have a set or class as its extension, as had been long thought in the philosophical tradition. Furthermore, Cantor's original definition of a set as "a collection, gathered into a whole of certain well-distinguished objects of our perception or our thought" must also be restricted. What is unsatisfactory about other proposed solutions, including his own, is that they do not preserve these rather simple and intuitive notions of set. These earlier definitions of sethood have not "been successfully replaced by one that is just as simple and does

[102] Zermelo, "Investigations", p. 200.
[103] Ibid., p. 200.

not give rise to such reservations."[104] Accepting this situation that our intuitions about sets have led us astray, he concedes

> There is at this point nothing left for us to do but to proceed in the opposite direction and, starting from set theory as it is historically given, to seek out the principles required for establishing the foundations of this mathematical discipline. In solving the problem we must, on the one hand, restrict these principles sufficiently to exclude all contradictions and, on the other, take them sufficiently wide to retain all that is valuable in this theory.[105]

What Zermelo acknowledges, continuing our ongoing theme, is that after the paradoxes set theory has become a largely pragmatic endeavor. The aim in light of the paradoxes is to restrict the notion of set sufficiently to exclude contradiction but to maintain enough of the theory so that it can continue its purpose as a foundation for all mathematics. No solution to such paradoxes will be as intuitive as the notion of a set as the extension of a concept, but this is the situation we must accept if set theory is to go on at all. And in this sense no solution will be entirely satisfactory; there will always be tradeoffs about what to save from our intuitions about sets.

Zermelo now goes on to present his axiom system attempting to show how he can reduce Cantor's set theory to a few definitions and seven axioms, which appear independent of each other. To indicate that his axioms do in fact preserve a reasonable amount of Cantor's theory, he then develops the theory of cardinals and mentions that in a later paper he will do the same for the ordinals. He admits, however, that he has not been able to give a rigorous proof of their consistency, "though this is very essential." Instead, he will merely indicate how his axioms resolve the known paradoxes in hopes that this will provide a useful beginning towards a consistency proof.[106] His axioms are as follows:

I) Axiom of Extensionality: two sets are equal if they have the same elements.

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[104] Ibid., p. 200.
[105] Ibid., p. 200.
[106] Ibid., pp. 200-1. Heinz-Dieter Ebbinghaus in his *Ernst Zermelo: An Approach to his Life and Work,* (New York: Springer: 2007), p. 77-8, criticizes Moore's interpretation from the side of consistency observing that this was crucial to Hilbert's program in the philosophy of mathematics, so much as I have been urging, Zermelo's axiomatization, again, is not a purely mathematical undertaking.

II) Axiom of Elementary Sets: there exists an empty set; for every set there exist a set with only that set as its member; and for all sets a and b, there exists a set with only these two members.

III) Separation Axiom: if a propositional function φ(x) is definite for all elements of a set M, M has a subset M_φ containing just the element for which φ(x) is true.

IV) Power Set: for any set M, there is a set P(M) containing all the subsets of M.

V) Union: to every set M there corresponds a set union of M that contains exactly all the elements of the elements of M.

VI) Choice: if M is a set with elements all distinct from the empty set and are mutually disjoint, its union includes at least one subset having exactly one element in common with each element of M.

VII) Infinity: there exists an infinite set.

It is the separation axiom, he explains, that resolves the paradoxes. Unlike the comprehension principle, sets can no longer be defined independently by an axiom. Instead, they are always separated out as subsets of sets already given. As such, contradictory notions such as "the set of all sets" or "the set of all ordinals" are excluded from the theory. The defining condition of such subsets must always be definite if the fundamental relations of the domain, meaning the membership relation, by means of the axioms and the logical laws, determine whether or not φ holds or not for a given value x. This last restriction banishes in addition to the set-theoretic paradoxes, the semantic paradoxes such as the Liar and Richard's paradox.[107] It should be noted that the idea of a propositional function's being definite would continue to be refined over the next twenty years or so until it would come to mean "definable by means of first-order logic".

Having explained separation, Zermelo next shows in his Theorem 10 how Russell's paradox now becomes the theorem that there is no set of all sets: every set M possesses at least one subset M_0 that is not an element of M. First, observe "x is a member of x" is a definite

[107] Zermelo, "Investigations", pp. 201-4.

property. Now let M_0 be a subset of M such that x is a member of M_0 if and only if x is not a member of itself. Then M_0 is not a member of M. Suppose M_0 is a member of M_0. Then M_0 is a member of M and M_0 is not a member of M_0 by the construction of M_0. This is equivalent to M_0 is a member of M_0 if and only if M_0 is not a member of M_0. Contradiction. Hence, M_0 is not a member of M. So the domain itself is not a set thus excluding Russell's paradox and turning it into a theorem.[108] I now turn to consider Russell's own solution to the paradoxes.

As early as the *Principles* Russell proposed solving the paradoxes with his theory of types, a sort hierarchy of propositional functions that would prevent such membership conditions as self-membership and hence excluding from the theory his own paradox, among others.[109] His proposal of types in the *Principles*, however, was tentative and between 1903 and 1908 he explored a number of other approaches towards resolving the paradoxes before coming back to types in 1908.[110]

Russell begins his "Mathematical Logic as Based on the Theory of Types" remarking that this system of logic recommended itself to him because of its ability to resolve certain paradoxes, in particular Burali-Forti's of the largest ordinal. "But the theory in question seems not wholly dependent on this indirect recommendation," he argues,

> it has also, if I am not mistaken, a certain consonance with common sense which makes it inherently credible. This, however, is not a merit upon which much stress should be laid; for common sense is far more fallible than it likes to believe.[111]

It is striking how similar Russell's approach is to Zermelo's. Both start from the paradoxes and motivate their proposed solutions by their success in resolving them, rather than on grounds of, say, capturing the notion of what a set really is. They each recognize that it was our intuitions

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[108] Ibid., p. 203.
[109] Russell, *Principles*, pp. 103-5 and Appendix B.
[110] This is particularly true of his 1905 "On Some Difficulties in the Theory of Transfinite Ordinals and Order Types", in *Essays in Analysis*, Lackey, ed., (London: George Allen and Unwin, Ltd., 1973), pp. 135-64.
[111] Bertrand Russell, "Mathematical Logic as Based on the Theory of Types", in van Heijenoort, p. 153.

about sets that led the theory astray and so, can no longer serve as the sole guide in developing the theory. For both Russell and Zermelo what determines the success of their axiomatizations is their ability to avoid the paradoxes while still including enough of Cantor's original theory to serve as a foundation for mathematics. Again, the theme of pragmatism continues to run strong in resolving the set-theoretic paradoxes.

Unlike Zermelo, Russell next gives a detailed account of the various paradoxes among them the liar, Burali-Forti, and his own. Moore attaches much significance to this in his interpretation of Zermelo arguing that Zermelo's lack of such an extended discussion, leaving out in particular the "philosophical but non-mathematical conundrum" of the liar paradox, indicates that his primary aim was not to solve the paradoxes.[112] But as we have seen, Zermelo does take the paradoxes as his starting point. He is explicit that he will not probe the more philosophical questions, but he does, however, "hope to have done at least some useful spadework hereby for subsequent investigations in such deeper problems," among these the origins of the axioms, their general validity, and their consistency.[113] Russell does choose to probe such questions presenting the various paradoxes in some detail so as to show that they share "a common characteristic".[114] Perhaps, as Moore suggests, this difference is accounted for by Russell's philosophical background, but given Zermelo's self-professed interest in the paradoxes it does not seem we can say that his axiomatization is unconcerned with them. The difference in presentation may just be a difference in the disciplines' writing styles with mathematical papers often written in a succinct style and judged on the mathematical results they

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[112] Moore, "Origins", p. 323-4. It should be noted that Zermelo did mention some of the other semantic paradoxes indicating that he, like Russell, did not yet draw a distinction between the logical, or set-theoretic paradoxes, and the semantic paradoxes as Ramsey eventually did. See F. P. Ramsey, "The Foundations of Mathematics," and "Mathematical Logic," both in *The Foundations of Mathematics and Other Logical Essays*, ed. R. B. Braithewaite, (New York: Humanities Press, 1950).

[113] Zermelo, "Investigations", pp, 200-1.

[114] Russell, "Theory of Types", p. 154.

present, unlike philosophy papers, where presenting motivations is encouraged and often of primary importance.

The common feature Russell discerns in all of these paradoxes is "self-reference, or reflexiveness." So, for example, when Epimenides the Cretan asserts that all Cretans are liars and that all other statements made by them are lies, this assertion itself must fall within the scope of Epimenides' statement. Similarly, for all classes x to belong to the class w if and only is x is non-self membered, this condition must apply to w itself. Russell indicates, "In each contradiction something is said about *all* cases of some kind, and from what is said a new case seems to be generated, which both is and is not of the same kind as the cases of which *all* were concerned in what said."[115] He then proceeds through the paradoxes showing how this works in each case concluding, "Thus all our contradictions have in common the assumption of a totality such that, if it were legitimate, it would at once be enlarged by new members defined in terms of itself."[116] And thus he establishes the general principle, a version of his vicious circle principle, that

> "Whatever involves all of a collection must not be one of the collection", or conversely: "If, provided a certain collection had a total, it would have members only definable in terms of that total, then the said collection has no total."[117]

The theory of types will be such a theory that excludes such collections so making such phrases as "all propositions", "all properties", or "all classes" meaningless.

Russell defines a type as the range of significance of a propositional function, that is, the collection of arguments for which the function has values, or is true or false. Otherwise, the resulting proposition is simply nonsense. Whenever an apparent variable, that is, a bound

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[115] Ibid., p. 154; Russell's italics.
[116] Ibid., p. 155.
[117] Ibid., p. 155. For a more detailed account of this principle and its motivations in Russell see Peter Hylton, "The Vicious Circle Principle," in *Propositions, Functions, and Analysis: Selected Essays on Russell's Philosophy*, (Oxford: Clarendon Press, 2005), pp. 108-14.

variable, occurs in a proposition, its range of values forms a type. The paradoxes then are avoided by the vicious circle principle as implemented by the theory of types: "no totality can contain members defined in terms of itself," or more technically, "whatever contains an apparent variable must not be a possible value of that variable." Russell's typing of the objects of the universe forces it so that any expression containing an apparent variable must be of higher type than the possible values of that variable. Thus the apparent variables in an expression determine the type of the expression.[118]

Russell elaborates his theory by explaining that any proposition containing an apparent variable is a "generalized proposition", and any proposition with no apparent variables is an "elementary proposition". A generalized proposition presupposes the other propositions from which it is obtained by the process of generalization. Hence all generalized propositions presuppose elementary propositions. In an elementary proposition we distinguish one or more terms from one or more concepts, where the terms are what we regard as the subject of the proposition and the concepts are the predicates or relations asserted of the terms. The terms of an elementary proposition, Russell calls the "individuals", and these form the lowest type. In practice, however, he remarks that it is unnecessary to know which objects belong to the lowest type, or even to know, in a given context, whether the lowest type are individuals. All that ultimately matters are the relative types of the objects in a given expression. So the lowest type in any context may be taken as the individuals, and all that is essential to them is the way in which the other types are generated from them as follows.[119]

In generalizing the individuals in an elementary proposition, we generate a new proposition, and this will be a legitimate procedure so long as no individuals are propositions,

[118] Ibid., pp. 162, 163.
[119] Ibid., p. 163-4.

which is the case because propositions are essentially complex whereas individuals lack all complexity. So for example, given the propositional function "x is red", where "x" is taken as an individual variable, we may generalize on "x" to get the new proposition "$(\forall x)$(x is red)", or "Every individual is red". And since the individuals cannot be propositions, generalizing on them in this way does not lead to reflexive paradoxes. The elementary propositions along with the propositions containing only individual variables as apparent variables, Russell calls "first-order propositions", and these propositions form a new set of objects that can also be generalized so generating the second type. Generalizing now on the variables of the second type, we generate the second-order propositions, which now also serve as quantifiable variables so forming the third type, and so on.

Now, we can see how the typing removes the reflexive paradoxes. Epimenides' statement now becomes, for example, "All first-order propositions asserted by me are false." Since this is a proposition about first-order propositions, it is itself a second-order proposition and in fact, true. It does not assert any true first-order proposition, and so does not yield a paradox as before. In general, the $(n + 1)^{th}$ logical type consists of order n propositions, and these propositions contain as apparent variables propositions of order no higher than $n - 1$. Since the types are mutually exclusive, it is impossible to generate the reflexive paradoxes.[120]

In practice, Russell explains that it is easier to work with a hierarchy of functions rather than propositions.[121] So a function that takes only individuals as arguments, thus always yielding a first-order proposition, is a first-order function. A function involving only first-order functions or propositions as apparent variables is a second-order function and so on. Furthermore, he calls a function of one variable which is of order next above its argument a "predicative function"; this

[120] Ibid., p. 164.
[121] For a discussion of this point see Peter Hylton, "Russell's Substitutional Theory," in *Propositions Functions, and Analysis: Selected Essays on Russell's Philosophy*, (Oxford: Clarendon Press, 2005), pp. 83-107.

is the same also for propositions of several variables. Hence, the type of a function is determined by the type of its values and the types of its arguments. He further explains this hierarchy by denoting first-order functions of an individual variable x as "φ!x". Since no first-order function contains a function as a variable hence forming a well-defined totality, we can generalize on φ. Then any proposition with φ as apparent variable and no apparent variable of higher type is a second-order proposition. If such a proposition contains and individual variable x, then this proposition is not a predicative function of x but it is of φ written "f!(ψ!ŵ)". So f is a predicative function and its values form a well-defined totality, and so we can also generalize on f. This defines the third-order predicative functions, which are such that have third-order propositions as values and second-order predicative functions as arguments and so on.[122]

Although the type theory blocks the paradoxes, a new problem arises. The success of the theory is to be judged on its capacity for providing a foundation to mathematics, but as it stands, it generates very little of mathematics. As we have seen, the typing restrictions exclude such expressions as "all propositions" or "all properties of x". We can only make such assertions when restricted to a particular order. But if a finite number is defined as it usually is as a number that possesses all properties possessed by zero and by successors of all numbers possessing these properties, then the type theory says we must confine our talk to properties of a particular order. But if we confine this statement to all first-order properties of numbers, then we cannot infer that this definition holds for all second-order properties. So for example, we cannot prove that if m and n are finite numbers, then m + n is a finite number since, by the above definition, "m is a finite number" is a second-order property of m. So from "m + 0 is finite, and that if m + n is

[122] Russell, Theory of Types, p. 165.

finite, so is m + n + 1," we cannot conclude by induction "m + n is a finite number," leaving much of mathematics impossible.[123] Russell's solution is to introduce the axiom of reducibility.

What Russell needs for mathematics is a way of reducing the order of a propositional function without affecting the truth or falsity of its values, and so simulating talk of all properties. He solves this problem with what he takes as the "common sense" reason for accepting classes. He explains, given any propositional function of any order φx, we assume this to be equivalent for all arguments x to the statement "x belongs to the class α". This is a first-order statement, which makes no mention of all functions of such-and-such a type, with the only practical advantage of the original statement being that it is of first-order. There is no other particular advantage in assuming outright the reality of classes (Russell instead defines classes contextually from his intensional propositional functions). In fact, Russell states,

> I believe the chief purpose which classes serve, the chief reason which makes them linguistically convenient, is that they provide a method of reducing the order of a propositional function. I shall, therefore, not assume anything of what may seem to be involved in the common-sense admission of classes, except this: that every propositional function is equivalent, for all its values, to some predicative function.[124]

Much as with Zermelo's system and the basic system of types, the axiom is introduced on pragmatic grounds. Like Zermelo, Russell wants to preserve as much of set theory as mathematics requires while still excluding the paradoxes. In neither case, are they motivated by something they see as some feature they see as the essence of sets or classes. The paradoxes showed that commonsense and our intuitions about sets or classes were not a reliable guide in developing the theory.

Russell goes on to elaborate the new axiom explaining that it is to hold of any function regardless of the type of its argument. So if we let φx be a function of any order of an argument

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[123] Ibid., p. 167.
[124] Ibid., p. 167.

x, which itself may be of any type, then by the axiom of reducibility, there is a predicative function φ!x equivalent to it. This Russell thinks is weaker than the usual assumption of classes, and it retains as much of classes as needed for mathematics but not so much as to regenerate the paradoxes. The axiom allows statements about "all first-order functions" or "all predicative functions" to do the work that statements about "all functions" previously did. "The essential point," he observes, "is that such results are obtained in all cases where only the truth or falsehood of values of the functions concerned are relevant, as is invariably the case in mathematics."[125] The axiom, as such, turns Russell's intensional theory of propositional functions into the extensional theory of classes. Yet, there may be a worry that with the axiom, the paradoxes return. Russell argues that this is not the case since in the case of the paradoxes either something beyond the truth or falsity of the values of the function is concerned or the expression occurs that remains unmeaningful even after the axiom is applied.[126]

Russell concludes the paper remarking more specifically on how his theory deals with some of the paradoxes. With regard to his own he explains that since we can identify classes with functions, no class can be a member of itself due to the hierarchy of functions. Members of the class, then, are the arguments of a function, and arguments of a function are of lower type than the function itself. And this forces Russell to give up his strongly held earlier view that there must be a class of all classes, regardless of how intuitive it might have initially seemed. Instead, the theory of types gives him a class of all classes of type t. Since this class is of the next higher type, it is not a member of itself. "Thus," he concludes,

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[125] Ibid., p. 168.

[126] Ibid., p. 168. For an excellent technical exposition of this point with regard to the heterological paradox, and of the theory of types in general, see Alonzo Church, "Comparison of Russell's Resolution of the Semantical Antinomies with that of Tarski", *The Journal of Symbolic Logic*, vol. 41, no. 4 (Dec. 1976), pp. 747-60, especially pp. 752-4.

> although the above primitive propositions [his basic laws of the system] apply equally to all types, they do not enable us to elicit contradictions. Hence in the course of any deduction it is never necessary to consider the absolute type of a variable; it is only necessary to see that the different variables occurring in one proposition are of the proper relative types.[127]

The idea encapsulated here is that of typical ambiguity. It is an important one to which we will return in the next chapter on Quine's system, New Foundations (NF).

The type theory similarly eliminates Cantor's paradox of the largest cardinal. Just as the typing restrictions now generate a class of all classes for each type, they also generate a largest cardinal for each type, the cardinal of the whole type. But as is consistent with Cantor's theorem, such a cardinal is always surpassed in size by the cardinal of the next type since the type n + 1 contains all subsets of the type n. Furthermore, since there is no way to add classes of different type so as to get a cardinal of all objects of whatever type, there is no greatest cardinal.[128]

Finally, Russell examines Burali-Forti's paradox concluding much as with the other two that there is no totality of all ordinals. Again, his theory yields an ordinal for any given type, but higher types will always have greater ordinals, which since they are of different types cannot be added together into a single all encompassing ordinal. Only in a given type can all the ordinals be arranged by order of magnitude into a well-ordered series, and such a series will then have an ordinal that is of higher type than the ordinals in the series. He concludes, much like Cantor had initially and then Zermelo, that

> It is impossible to complete the series of ordinal, since it rises to types above every assignable finite limit; thus although every segment of the series of ordinals is well-ordered, we cannot say that the whole series is well-ordered, because the "whole series" is a fiction. Hence Burali-Forti's contradiction disappears.[129]

Surprisingly, in light of Moore's interpretation of the differences between Zermelo and Russell's approaches to the paradoxes, Russell closes his paper much as Zermelo began:

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[127] Ibid., p. 172.
[128] Ibid., p. 179. Applying a singleton function allows us to raise the type of a class without changing its cardinality so that classes of different cardinality can be added together, but this holds only for a finite number of cardinals; there is no infinite singleton function. The situation is similar for ordinals.
[129] Ibid., p. 181-2.

> The theory of types raises a number of difficult philosophical questions concerning its interpretation. Such questions are, however, essentially separable from the mathematical development of the theory and, like all philosophical questions, introduce elements of uncertainty which do not belong to the theory itself. It seemed better, therefore, to state the theory without reference to philosophical questions, leaving these to be dealt with independently.[130]

It seems then that where set theory is concerned the line between philosophy and mathematics remains a difficult one to draw as we will continue to see in the next chapter concerning Quine's approach to the paradoxes in the system of New Foundations.

[130] Ibid., p. 182. In fairness to Moore, it should be noted that in a somewhat later paper he attributes the discovery of Russell's paradox to the important intertwining of mathematics and philosophy. He remarks, "Mathematics and philosophy have interacted in many ways, but in the twentieth century mathematicians have often reacted with suspicion when philosophers had something to say about mathematics. The origins of Russell's Paradox provide a case study of how traditional philosophical concerns led to genuine mathematical progress," in "Origins of Russell's Paradox," pp. 234-5.

III. QUINE'S NEW FOUNDATIONS

We now turn to consider the system of set theory put forward by W.V. Quine in his 1937 paper "New Foundations for Mathematical Logic".[131] New Foundations (NF), as this system has become known, emerged from Quine's thinking through the systems of both Russell and Zermelo preserving something like the type restrictions of type theory along with the unrestricted variables of Zermelo's set theory. The system is especially notable, and perhaps also controversial, in proving the existence of such "big" sets as the set of ordinals, the set of cardinals, and even the universal set. In this chapter, I lay out the system and discuss how it avoids the paradoxes. I also begin to examine, from the perspective of his work in logic and foundations, the sort of philosophical mind Quine was, which will be the primary focus of the next chapter.

Quine opens his article observing that Russell and Whitehead's *Principia Mathematica*[132] provides substantial evidence that all of mathematics is translatable into logic. In light of Gödel's incompleteness theorem, however, no axiom system is sufficient for deducing all of mathematics. Still, the core of mathematics, as in *Principia*, serves as some reasonable standard for judging any system to be sufficient for mathematics, and so it is this standard Quine applies for judging NF. For all of Zermelo, Russell, and Quine, the standard of success for any given formulation of set theory is simply whether the system captures some reasonable amount of mathematics. As far as logic goes, Quine admits that the logic required for mathematics is a good deal stronger than traditional logic:

> It must be admitted that the logic which generates all this is a more powerful engine than the one provided by Aristotle. The foundations of the Principia are obscured by the notion of propositional function, but, if we suppress these functions in favor of the classes and relations which they parallel, we find a three-fold

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[131] W.V. Quine, "New Foundations for Mathematical Logic", *The American Mathematical Monthly*, vol. 44:2, (1937), pp. 70-80.

[132] Alfred North Whitehead and Bertrand Russell, *Principia Mathematica*, 2nd. ed., 3 vols., (Cambridge: Cambridge University Press, 1925-27).

> logic of propositions, classes, and relations. The primitive notion in terms of which these calculi are ultimately expressed are not standard notions of traditional logic; still they are of a kind which one would not hesitate to classify as logical.[133]

So we have Quine clarifying exactly what he will count as a logical foundation for mathematics; it is logic inclusive of set theory. But in accepting such a position, he knowingly gives up much of the epistemological aims that the reduction of mathematics to logic was supposed to accomplish. Quine's concern is not to somehow rest mathematics on a more certain foundation than mathematics itself. Set theory, for him as we will later see, is foundational in that it unifies diverse bodies of mathematics into a single framework of logic and set theory.[134] This theme will be important for understanding what Quine's philosophy of set theory is, which we will deal with more thoroughly in the following chapters. For now, we merely observe that these departures from traditional aims are present in some of his earliest work in foundations. This is not, however, the only point of interest here in beginning to flesh out Quine's philosophical stance. We also see him in this passage quickly dismissing Russell's basic notion of a propositional function on grounds of its obscurity and instead accepting classes outright. The central Quinean themes of clarity and simplicity are present already at this early stage, and in recognizing the obscurity of the notion of a propositional function, we can see already the beginnings of Quine's attack on philosophical accounts of meaning and so also on the analytic-synthetic distinction. We see here the core of Quine's philosophy as emerging from his logical concerns.

The system of NF itself contains a much smaller number of basic notions than that of

[133] Ibid., p. 71.

[134] This point is also interesting in light of later disputes over the logical status of higher-order logic. We see here that Quine was already in a position at this early stage to draw a distinction between first- and higher-order logic on grounds of their respective claims to logicality. In a way, what we see here is that the label "logic" was never of great importance for Quine. Rather, what was important was making clear what it was we were will to count as logic and being honest about what sorts of assumptions we are building into attributing the label "logic". Among such assumptions, as in the case here, is the mathematical strength of such theories.

Principia Mathematica (PM) employing just membership, "$\in$" (epsilon); alternate denial, "$|$" (not both); universal quantification, "$(\forall x)$", and an infinite number of variables "x", "y", "z", "x'", "y'", "z'", "x''" and so on. The variables, unlike those of PM, range over all objects whatsoever; there is no stratification of the universe into different levels, or types. From these basic notions, he then defines the usual logical connectives of negation, conjunction, disjunction, conditional, biconditional, and the existential quantifier, along with the various set-theoretic relations such as subset, "$\subseteq$"; union, "$\cup$"; and intersection "$\cap$". In remarking that the formula "$(x \in y)$", or "x is a member of y", only makes sense where y is a class, he further simplifies the system by adopting the convention that, where y is an individual, we may interpret such formulas as "x is the individual y". As such, every individual x is just equal to its unit class, or singleton, $\{x\}$, but, he adds in a footnote that "this is harmless."[135] Again, we see the broadly pragmatic spirit that has come to characterize Quine's philosophy emerging in his logical work. Where he can simplify the system, unlike and in reaction to Russell, he does so without regret.

Next, Quine introduces the basic rules and axioms, or initial theorems, of NF, among them truth-functional and quantificational axioms, and rules of inference. He also proposes the following two set-theoretic principles:

P1. $(\forall x)(\forall y)(x \subseteq y) \supset ((y \subseteq x) \supset (x = y))$,

the extensionality principle that a class is determined by its members, and

R3. If "x" does not occur in φ, then "$(\exists x)(\forall y)((y \in x) \equiv \varphi)$" is a theorem.

In short, given any condition on y, there is a class x with members y such that this condition holds of the y's. But he then observes, this is of course the inconsistent comprehension principle

[135] New Foundations, p. 71. This convention is indeed harmless, as shown by Scott, but it also results in the surprisingly very different system of NFU, distinguished in particular by Jensen's consistency result. See his "On the Consistency of a Slight (?) Modification of Quine's NF," *Synthese* 19, (1969), pp. 250-63.

yielding Russell's paradox when we take φ as the condition "(y ∉ y)". As we saw in the previous chapter, one way of resolving the paradoxes was to adopt types so that the universe is stratified into levels and the variables are restricted to ranging over objects of a particular level. (α ∈ β), then is only a formula if the values of β are of type n + 1 and the values of α are of type n. In all other cases, (α ∈ β) is not true or false but meaningless.[136]

Quine, however, focuses his attention on an observation we saw Russell himself make in the previous chapter that

> In all contexts the types appropriate to the several variables are actually left unspecified; the context remains systematically ambiguous, in the sense that the types of its variables may be construed in any fashion conformable to the requirement that "∈" connect variables only of consecutively ascending types. An expression which would be a formula under our original scheme will hence be rejected as meaningless by the theory of types only if there is no way whatever of so assigning types to the variables as to conform to this requirement on "∈." Thus a formula in our original sense of the term will survive the theory of types if it is possible to put numerals for the variables in such a way that "∈" comes to occur only in contexts of the form "n ∈ n + 1."[137]

While Russell had already noticed this feature of types, Quine takes the idea more seriously and follows it through to its conclusion. It is inessential to resolving the paradoxes that the universe actually be stratified into levels; what the typing restriction does is provide a syntactic test for which formulas actually determine classes, these are the so-called "stratified formulas". "New Foundations", in this sense, as he later remarked, emerged from the theory of types.[138]

Quine first observes some unnatural consequences of Russell's type theory. Although we saw Russell clinging desperately to a universal class, his resolution of the paradoxes in type theory ultimately forced him to give up this idea in favor of a series of, what Quine describes as, quasi-universal classes, a class of all objects of the type directly below. So there is a class of all individuals, of all classes of classes of individual and so on, but there is no single class V that

[136] Ibid., p. 77.
[137] Ibid., p. 78.
[138] W.V. Quine, "The Inception of New Foundations", in *Selected Logic Papers*, enlarged ed., (Cambridge: Harvard University Press 1995), p. 287.

contains all objects regardless of type. Similarly, there is no single null class Λ but, again, one for each type. Types has the further odd consequence that classes no longer have absolute complements. A class –x does not contain all non-members of x but rather only those non-members of x at the next lower type than x. So we see the Boolean class algebra no longer applies to classes in general but only to classes within particular types. Furthermore, these classes are then reduplicated within each type, and the same holds also for the usual calculus of relations. Finally, he notes that even arithmetic is reduplicated in each type so that the numbers fail to be unique, there being for each type a number 0, 1, 2, 3 and so on. He concludes, "Not only are all these cleavages and reduplications intuitively repugnant, but they call continually for more or less elaborate technical maneuvers by way of restoring severed connections."[139] By taking Russell's insight of typical ambiguity seriously, Quine resolves the paradoxes while avoiding these unhappy consequences: "Whereas the theory of types avoids the contradictions by excluding the unstratified formulas from the language all together, we might gain the same end by continuing to countenance unstratified formulas but simply limiting R3 explicitly to stratified formulas."[140] The hierarchy of types is then removed so that variables stay unrestricted in range and the logical language allows for all formulas in the original untyped sense. The notion of a stratified formula remains a part of the system only the following revised version of R3:

R3'. If φ is stratified and does not contain "x", then "$(\exists x)(y)((y \in x) \equiv \varphi)$" is a theorem.

Here we also get a glimpse of a further reason for rejecting the theory of types that Quine was only to emphasize in his later account of how he came upon "New Foundations". There he explained that he disliked the arbitrary grammatical restrictions of type theory. As mentioned

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[139] Ibid., p. 78-9.
[140] Ibid., p. 79.

above, unstratified formulas, according to type theory, were neither true nor false but meaningless, and as such "[s]eemingly intelligible combinations of signs were banned as ungrammatical and meaningless."[141] The theory of types was, for Quine, too drastic in its resolution of the paradoxes in banning all unstratified formulas, especially since many of them seemed perfectly intelligible. Much of his work up to "New Foundations" was an attempt to legitimize these restrictions so that the banning of unstratified formulas would not be wholesale and without reason.[142] In his first attempt in 1935, he attempted to legitimize the restrictions by finding some more basic array of notions that would allow him to provide contextual definitions of classes and the membership relation. "I hoped," he explained, "to devise contextual definitions that would generate just the formulas that fit the theory of types, while leaving other formulas meaningless in the straightforward sense of not being accounted for by the definitions." The attempt failed, however, as the set-theoretic paradoxes simply shifted to become semantic paradoxes.[143]

At this point the crucial influence shifted from Russell to Zermelo. From him Quine gained the insight "that a meaningful open sentence may or may not determine a class, and that it can be left to the axioms to settle which ones do."[144] So while Russell banned such open sentences as "x is a member of itself" on grounds of their supposed meaninglessness, Quine took the contrary position affirming their meaningfulness but leaving it open that they may not determine a class depending upon which classes the axioms yield. His first step in the direction of New Foundations was the 1936 "Set-theoretic Foundations for Logic" in which he presents a

[141] Quine, "Inception", p. 286.
[142] Ibid., p. 286.
[143] Ibid., pp. 286-7.
[144] Ibid., p. 288.

version of Zermelo’s system but with a modified version of the separation axiom. Zermelo state his original version of separation as

$$(\exists x)(\forall y)(y \in x \leftrightarrow x \in z \wedge Fy).$$

Quine’s idea was to replace ‘$x \in z$’ with ‘$x \subseteq z$’ so as to obtain

$$(\exists x)(\forall y)(y \in x \leftrightarrow x \subseteq z \wedge Fy).$$

While Zermelo’s system required several other set-theoretic axioms to be adequate for the derivation of mathematics, Quine’s modified system, which he called “Γ”, included only the modified separation axiom and the extensionality axiom. As we might be starting to expect, his aim in “Set-theoretic Foundations” was “the presentation of a system Γ which resembles [Zermelo’s] system but is more economical.”[145] Like the systems of Zermelo and Russell, however, in order to derive a substantial amount of mathematics, the system Γ still required additional postulating of the axioms of infinity and choice.

Indeed, this lack of economy was the primary drawback Quine saw in Zermelo’s system since it otherwise avoided the difficulties of types in that it lacked both arbitrary grammatical restrictions and the reduplication of objects at each level of the hierarchy: “Zermelo’s system itself was free of both drawbacks, but in its multiplicity of axioms it seemed inelegant, artificial, and ad hoc.”[146] But both Zermelo’s system and the modified version Γ had another drawback in removing type theory’s reduplication of objects. While types had quasi-universal classes, Zermelo’s system banned this class altogether, and as such, absolute complements were again banned yielding instead only complements with regard to an already given class. In general,

[145] W.V. Quine, “Set-theoretic Foundations for Logic”, in *Selected Logic Papers*, p. 85.
[146] In later chapters we will see more of how Quine revised this view of Zermelo’s set theory in light of the connections he was able to draw between it and type theory. Drawing out these sorts of connections is central to Quine’s philosophy of set theory and to his philosophy in general. He gestures at this move in “Inception”, pp. 287-8. Contrast this with his earlier statement in “New Foundations” about the awkwardness of Zermelo’s system p. 80, fn., which he notably removed from the paper in the form published in *From a Logical Point of View*.

what both systems lacked were big classes.[147] Here we see another aspect of Quine's philosophy of set theory which will emerge more fully in the following chapters. Quine is very much concerned with set theory as an explorative project much like the rest of science. The lack of big classes is not necessarily a drawback for reconstructing mathematics within set theory. Most ordinary mathematics goes on with comparatively small sets, but this lack has become all the more evident as set theory itself has emerged as a subject of mathematical investigation. In exploring how the set-theoretic universe functions we should not prejudice our explorations to only some sets. We should want to know what all sets are like, how they a similar and where they diverge and what sorts of features cause such divergences.[148] Should we want to know what the big sets are like, neither the system of Russell nor of Zermelo can give us any answer.

What Quine wanted was a system free of all the drawbacks of types and Zermelo, "but would be like the theory of types in having a single comprehension principle of class existence, and would admit big classes without restriction to type."[149] Combining the insights of Russell's typical ambiguity with Zermelo's view that the axioms could determine which sets exist gave him precisely this as codified in the system of "New Foundations". In light of Zermelo, "I was able to look to types as a restriction specifically upon classes and not upon language. The purpose of the theory of types was to bar the paradoxes, and this could be done by using it only to say which open sentences are to be taken to determine classes."[150] The structure of the set-theoretic universe, then, was determined by the stratification condition limited only to the comprehension principle R3'. The universe itself was not stratified into a hierarchy of classes as

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[147] Quine, "Inception", p. 288.
[148] This is perhaps very much in the spirit of much current research in set theory concerned with large cardinal axioms. We want to know what the universe is like beyond the sets given by ZFC. Yet NF remains largely neglected in its possible insights into the universe.
[149] Quine, "Inception", p. 288.
[150] Ibid., p. 288.

Russell thought. And now, with the variables reconstrued as general, that is, as ranging over all objects with no restriction of type, Quine "found new strength accruing at every turn, apparently with impunity."[151] New Foundations successfully removed Russell's arbitrary grammatical restrictions while not adding the reduplication of objects back in. Furthermore he regained the universal class and absolute complements as well as other such big classes as the class of cardinals and of ordinals. And finally, he obtained an infinite class without the arbitrary postulation of an axiom of infinity in that the universal class existed and contained within in it each natural number such that for all such numbers m and n, $m \neq n$.[152]

Quine's aim in presenting NF was not to do away with other set theories, to have somehow presented the single "right" conception of a set. Rather his aim was, and would remain, to further set-theoretic research generally by looking at sets from a different perspective, which preserved certain fairly intuitive features of sets that the systems of Russell and Zermelo did not. From Quine's perspective none of these systems discovered anything like the essence of sets so as to zero in on the one and only correct set theory. Rather, just like Russell and Zermelo, Quine's primary purpose was eliminating the paradoxes while still preserving enough of Cantor's original theory for it be mathematically interesting. Any set theory adequate to these aims was one worthy of mathematical, or perhaps we might even say more broadly, scientific, investigation.

But then did NF in fact avoid the paradoxes? Quine remarks in conclusion that he has no proof that such a system is consistent, but just as Zermelo and Russell reasoned about their respective systems, he can see not way of deriving a paradox within it. As he himself makes this

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[151] Ibid., p. 288.

[152] Although it would not be until 1953 that Ernst Specker showed outright that the axiom of infinity was provable in NF. See his "The Axiom of Choice in Quine's New Foundations for Mathematical Logic," *Proceedings of the National Academy of Sciences*, vol. 39, no. 9, (September 1953), pp. 972-975.

connection: "The lack of a consistency proof is no special ground for misgivings, for there is likewise none for the systematization involving the theory of types."[153] Furthermore, his system removes the various unnatural consequences of types. Both the universal class V and the null class Λ are unique while the complement of x, –x, is the class of everything not in x. Thus the Boolean class algebra is restored and similarly for the calculus of relations. Similarly, the reduplications of arithmetic are removed so that the numbers are unique and their laws become generally applicable as a single calculus thus eliminating whatever complicated technical devices type theory required for restoring such severed connections.[154]

In concluding his paper, Quine remarks that NF differs from the original inconsistent theory in preventing the existence of classes defined by unstratified conditions. He adds in a footnote that the systems of his earlier "Set-theoretic Foundations for Logic" along with Zermelo's offer other approaches for resolving the paradoxes. "But these methods entail most of the awkward limitations which are entailed by the theory of types," he argues. "The present method of avoiding the contradictions, if it indeed avoids them, would seem to be the least restrictive method yet suggested."[155] So we see again, Quine's guideline for developing set theory is much like Zermelo's and Russell's—to preserve as much as the original theory unrestricted theory as possible. In this sense, NF does seem an advance over Zermelo's system and theory of types. While both of these versions of set theory are sufficient for reconstructing mathematics, they lack certain sets, the so-called "big" sets in particular. And as we have seen, these other theories include a variety of inelegancies resulting from the way they restrict the notion of set so as to avoid the paradoxes.

[153] Quine, "New Foundations", p. 79.
[154] Ibid., p. 79.
[155] Ibid., p. 80.

So far we have had only Quine's speculations that NF is free from contradiction. It does seem fairly obvious that Russell's paradox is not derivable in the system given that there is no set with the membership condition $y \notin y$. But how exactly the theory avoids the other paradoxes, such as Cantor and Burali-Forti, is less clear. Furthermore, Cantor had arrived at an important result of set theory, that the power set of a set always has greater cardinality than the set itself. It seems we should want to preserve such a result if we are to preserve Cantor's set theory. I now turn to see how in fact the known paradoxes are blocked in NF, and also how the theory preserves an analogue of Cantor's theorem.

Recall from the previous chapter that Cantor's paradox results from the claim that there exists a universal set and Cantor's result that for each set x, the power set of x has greater cardinality than the set x itself. Quine's NF certainly yields a universal set V in its stratified existence claim that $(\exists x)(\forall y)(y \in x \equiv y = y)$.[156] So it seems that such a set should have the greatest cardinality given that it contains all sets. But then by Cantor's theorem the cardinality of the power set of V would be greater than V itself, so landing us back in the contradiction that worried Russell so much. How then does NF block Cantor's paradox? In the first general result about NF, Quine himself showed both how his system blocks this paradox and also preserves an analogue of Cantor's theorem.[157] Although it may seem tedious, it is instructive to follow the details of this proof to see exactly how stratified comprehension works in preventing Cantor's paradox.

Quine states Cantor's theorem as "the converse domain of any one-many relation has a subset which does not belong to the domain." Formally,

[156] Claims using identity are stratified when the objects on each side of the relation are of the same type in a stratified formula. Quine takes identity as defined in terms of the identity of indiscernibles: $x = y =_{df} (\forall z)(x \in z \supset y \in z)$. So we could also replace occurrences of identity with this formula checking the resulting formula for stratification.
[157] W.V. Quine, "On Cantor's Theorem," *The Journal of Symbolic Logic,* vol. 2:3, (September 1937), pp. 120-4.

(1) $(\forall v)[(\forall y)(\forall z)(\forall w)((z, y) \in v \wedge (w, y) \in v \supset z = w) \supset$
$(\exists x)((\forall y)(y \in x \supset (\exists z)(z, y) \in v) \wedge \neg(\exists y)(x, y) \in v)]$.

Here, he renders functions in terms of two-place relations, where a relation Rxy is just a set v with the ordered pair (x, y) as a member. We can then read it as "there is a set x, a subset of the converse domain of R, such that for any object y, if it is in x then it is borne R by something, but the set of such y's bears R to nothing, i.e. is not in the domain of R." Hence, we have that a set always has more subsets than members.

To prove (1), assume v is a one-many relation, where a one-many relation is a relation such that any two members of the domain of the relation v bearing this relation to the same member in the range must be the same object, i.e.,

(2) $(\forall y)(\forall z)(\forall w)((z, y) \in v \wedge (w, y) \in v \supset z = w)$.

That is to say, such a relation is a function, since for any object in the domain, or input, there is a unique object in the range, or output, that it bears the relation to. And by unrestricted comprehension, let x be the set

(3) $(\forall y)(y \in x \leftrightarrow (\exists z)((z, y) \in v \wedge y \notin z)$.

So if $y \in x$, then $y \notin z$, and so $z \neq x$. So from (3) and truth-functional logic,

(4) $(\forall y)(y \in x \supset (\exists z)((z, y) \in v \wedge z \neq x)$.

But then by (2) and truth-functional logic,

(5) $(z, y) \in v \wedge z \neq x \supset (x, y) \notin v$.

Therefore, by (2) and (3) and truth-functional logic,

(6) $(\forall y)(y \in x \supset (x, y) \notin v)$.

And now, from (3) again,

(7) $(\forall y)((x, y) \in v \wedge y \notin x \supset y \in x)$,

which, by truth-functional logic, is equivalent to

(8) $(\forall y)((x, y) \in v \supset y \in x)$.

And so from (8)

(9) $(\forall y)((x, y) \in v \supset (x, y) \notin v)$,

which is equivalent to

(10) $\neg(\exists y)(x, y) \in v$.

And from (3),

(11) $(\forall y)(y \in x \supset (\exists z)((z, y) \in v)$.

Thus, from (2) and the unrestricted comprehension principle, (1) follows establishing Cantor's theorem.

As we have seen, unrestricted comprehension led to the set-theoretic paradoxes. Indeed, from this principle we have seen that we can prove the existence of a universal set, so Cantor's paradox would quickly follow. In Zermelo's system, we can also prove Cantor's theorem, but his restriction on comprehension excludes the existence of a universal set, so there is no reason to worry about the reemergence of the paradox there. But NF, like set theory with unrestricted comprehension, allows for a universal set, so how does it prevent Cantor's paradox?

The proof of Cantor's theorem above follows from comprehension, so it will likewise follow from comprehension in NF if the existence condition is stratified. However, it is not. The instance of comprehension used above was

(3) $(\exists x)(\forall y)(y \in x \equiv (\exists z)((z, y) \in v \wedge y \notin z)$.

But, using the Wiener-Kuratowski definition of ordered pair, and checking for stratification we get

(3') $(\exists x)(\forall y)(y \in x \equiv (\exists z_n)(\{\{z_n\}_{n+1}, \{z_n,y_n\}_{n+1}\}_{n+2} \in v_{n+3} \wedge y_n \notin z_n)$, demonstrating that this is an unstratified condition since y and z must have consecutive subscripts n and n + 1. Therefore, no such set exists in NF and Cantor's paradox is blocked. But it also follows now that Cantor's theorem in its stated form is false, and this may seem to weigh heavily against NF as a live option for formalizing set theory since it now seems to prevent one of Cantor's most important results about set theory. It is central to investigating infinite sets, and so, losing it would remove much of what made Cantor's theory interesting in the first place.

Quine, however, observes that NF does have an analogue to Cantor's theorem. Indeed, it is the same version of the theorem that we can prove for type theory, since there too, the stated version fails in its violation of the typing restrictions. In type theory, relations can be rendered as sets of ordered pairs where the members of the pairs must be of the same type. Mathematics, however, frequently requires pairs with members of different types. Without some way around this, mathematics would be all but impossible in type theory. The solution was to use the singleton operation as a way of raising types. So for example, the pair (x_1, y_2), which is excluded by typing restrictions, can be rendered in type theory in the following way. Again using the Wiener-Kuratowski analysis of the ordered pair, we get $\{\{x_1\}, \{x_1, y_2\}\}$. We then apply the singleton operation to appropriately raise the type so as to get $\{\{\{x_1\}_2\}, \{\{x_1\}_2, y_2\}\}$, which is then the stratified ordered pair $(\{x_1\}_2, y_2)$, a perfectly acceptable statement of type theory.

Similarly, we can now replace the original statement of Cantor's theorem (1) with

(1') $$(\forall v)[(\forall y)(\forall z)(\forall w)((z, \{y\}) \in v \wedge (w, \{y\}) \in v \supset z = w) \supset$$
$$(\exists x)((\forall y)(y \in x \supset (\exists z)(z, \{y\}) \in v) \wedge \neg(\exists y)(x, \{y\}) \in v)],$$

which is derivable in NF since the condition $(\exists z_{n+1})((z_{n+1}, \{y_n\}_{n+1})_{n+2} \in v_{n+3} \wedge y_n \notin z_{n+1})$ is stratified hence allowing for the existence of the required set. However, where (1) states that the subsets of a set cannot be correlated one-to-one with its members, that is, there are more subsets than members, (1') states that a given set has more subsets than singleton subsets, which has the further consequence that that in general there is no function correlating one-to-one members with their singletons. While this last result may seem especially odd, there are many sets for which such a correlation exists. These are the so-called "Cantorian sets" to which we will return in later chapters. So to conclude, what Quine shows is that, for NF, the original version of Cantor's theorem is false, although an analogue of it can be proved in the form that the subsets of a set outnumber the singleton subsets; and there is no correlation in general between the members of set and their singleton sets.

The next major investigation into the consistency of NF came in 1939 with Rosser's "On the Consistency of Quine's New Foundations for Mathematical Logic".[158] Here, Rosser presents a stronger system, *Q*, and shows that none of the usual methods for producing contradictions are possible in this system. *Q* also yields all of Quine's system NF. Therefore, if *Q* is free from contradiction so is NF.

Rosser begins by remarking on Quine's result that Cantor's paradox is not derivable in NF holds similarly for his system *Q*. However, he is able to generalize Quine's findings to show how this result shows the various other set-theoretic paradoxes similarly underivable for both *Q* and NF. Rosser observes that the central point to Quine's proof is his showing that there is no function that takes every set to its singleton. That is, there is no set of ordered pairs (x, y) such that x is equal to y where y is the singleton of x, $\{(x, y) : x = \{y\}\}$. Indeed, the existence of such a set would also yield both the Russell and Burali-Forti paradoxes. For example, consider the following:

$$(\forall R)(\exists \alpha)(\forall x)(x \in \alpha \equiv (\exists y)(x \notin y \wedge \{x\}Ry)).$$

Now, instantiating R by $\{(x, y) : x = \{y\}\}$ yields

[158] J. Barkley Rosser, "On the Consistency of Quine's New Foundations for Mathematical Logic", *The Journal of Symbolic Logic*, vol. 4, No. 1, (March 1939), pp. 15-24.

$$(\exists\alpha)(\forall x)(x \in \alpha \equiv (\exists y)(x \notin y \wedge \{x\} = \{y\}),$$

which gives implies

$$(\exists\alpha)(\forall x)(x \in \alpha \equiv x \notin x),$$

the Russell class. In fact, Rosser concludes that for any unstratified condition φ, we can find a stratified condition ψ with free occurrences of a relation R such that by replacing R with $\{(x, y) : x = \{y\}\}$ makes ψ equivalent to φ. And since relations are defined as classes of ordered pairs, which in turn are just classes of classes, thus depending on stratified comprehension for their existence. Hence, attempts to prove the existence of unstratified relations so as to derive the Russell, Cantor, or Burali-Forti paradoxes fail in NF.[159] So Rosser improves upon Quine's result by showing that this general feature of NF, that is, that there is no general function taking sets to their singletons, blocks the usual ways of generating the set-theoretic paradoxes in NF thus furthering the plausibility of NF's consistency.

While a similar argument holds against the Burali-Forti paradox it is helpful to consider in some detail, as with the Cantor Paradox, how exactly it plays out in NF. Unfortunately, it requires a bit more technical machinery than the discussion of the previous two paradoxes.[160] First, recall that the Burali-Forti paradox is the paradox of the greatest ordinal. Let ON be the set of ordinals which is naturally well-ordered by $\leq$, and let Ω be the order-type of $\leq$ restricted to ON. Since Ω is the type of $\leq$ on the ordinals, Ω is greater than all the ordinals in ON, but Ω is also an ordinal so it must be a member of ON. Hence, Ω is both greater than all the ordinals in ON and one of the ordinals in ON.

[159] Ibid., p. 23.
[160] Rosser does this in his later *Logic for Mathematicians*, (New York: McGraw-Hill Book Company, 1953), pp. 473-5. Here, I primarily follow Randall Holmes's discussion in *Elementary Set Theory with a Universal Set*, Cahiers du Centre de Logique, vol. 10, (Louvain-la-Neuve, Belgium: Academia, n.d.), pp. 109-10. Holmes's presentation is somewhat more accessible.

To begin our discussion of how Burali-Forti plays out in NF, we must first understand which set-theoretic objects the ordinals are in NF. Thomas Forster explains that the cardinals of NF must be Russell-Whitehead cardinals, that is, equivalence classes under equinumerosity, so for example, the cardinal number of a set x is the set of all objects the same size as x. Similarly, the ordinals of NF must be Russell-Whitehead ordinals. So for a well-ordering (X, R), its ordinal number is the set of all well-orderings order isomorphic to it.[161] Recall that every ordinal α has a successor $\alpha + 1$ such that $\alpha + 1 > \alpha$ and that they are naturally well-ordered by the relation $\leq$. For α an ordinal, we define an (initial) segment determined by α, written "$seg_{\leq}\{\alpha\}$", as the set $\{\beta \in NO: \beta < \alpha\}$. In a stratified formula, $seg_{\leq}\{\alpha\}$ is one type higher than α. As with the cardinal numbers, type raising operations will be important if we are to maintain stratification restrictions, so we introduce the operation T. If α is the order-type of (X, R), then $T\{\alpha\}$ is the order-type of $(P_1\{X\}, RUSC(R))$, where $P_1\{X\}$ is the set of singleton subsets of X and RUSC(R) is the set of relational singleton (or unit) subsets of R, that is the set $\{(\{x\}, \{y\}): x\ R\ y\}$. Iterations of the operation T are written "T^n", for n a natural number. A segment $seg_{\leq}\{\alpha\}$, then, is a member of the uniquely determined ordinal $T^2\{\alpha\}$, which is two types higher than α in a stratified formula.

Holmes next sketches a version of the Burali-Forti paradox. Consider the following inductive argument that for all ordinals α, $T^2\{\alpha\} = \alpha$. Let β be the smallest ordinal such that $T^2\{\beta\} \neq \beta$. For each ordinal $\alpha < \beta$, then, $T^2\{\alpha\} < T^2\{\beta\}$. Furthermore, we see that every ordinal less than $T^2\{\beta\}$ must be $T^2\{\gamma\}$ for some $\gamma < \beta$. But then it follows that $T^2\{\beta\}$ is the smallest ordinal greater than all the ordinals $T^2\{\alpha\}$ for $\alpha < \beta$. That is, $T^2\{\beta\}$ is β itself. Hence, it follows

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[161] Thomas Forster, *Set Theory with a Universal Set: Exploring an Untyped Universe*, (Oxford: Clarendon Press, 1992), p. 44.

that there is no such β such that $\beta \neq T^2\{\beta\}$. So for all ordinals α, $T^2\{\alpha\} = \alpha$. Next, we apply this result to the well-ordering $\leq$ itself. We let Ω be the ordinal containing $\leq$. By the above argument, $T^2\{\Omega\} = \Omega$. Since Ω is the ordinal of $\leq$, the ordinal of $\leq$ restricted to $seg_{\leq}\{\Omega\}$ is also Ω. And here arises the contradiction. Since the well-ordering $\leq$ is a strict continuation of $seg_{\leq}\{\Omega\}$, $\leq$ must also include additional ordinals $\Omega + 1$, $\Omega + 2$, and so on greater than the purported greatest ordinal Ω.

Fortunately, Quine's NF does not allow for this argument to go through. Since $T^2\{\alpha\}$ and α are of different types in a stratified formula, $T^2\{\alpha\} = \alpha$ is not stratified and hence, does not determine a set. Since we can only argue by transfinite induction with regard to sets, the above argument then fails because $T^2\{\alpha\} = \alpha$ and unstratified condition. And now we again see Rosser's general point about the singleton function in relation to the set-theoretic paradoxes. There can be no function taking every set to its singleton. If there were, we would then have $T^2\{\alpha\} = T\{\alpha\}$, and $T\{\alpha\} = \alpha$, and thus, $T^2\{\alpha\} = \alpha$ allowing the above argument to go through and thus, yielding the Burali-Forti paradox.

Forster explains the situation slightly differently observing that with every proper initial segment of ordinals we can associate two ordinals:

(1) the least ordinal not in X, call it "$L_1(X)$", and

(2) the order-type of X, call it "$L_2(X)$".

While $L_1(X)$ is bigger than every member of X, this is not obviously true of $L_2(X)$. To derive the Burali-Forti paradox, we need to show by induction on the end-extension relation that $(\forall X)(L_1(X) = L_2(X))$. But in NF, the ordinals $L_1(X)$ and $L_2(X)$) belong to different types. Hence, $(\forall X)(L_1(X) = L_2(X))$ is unstratified, and so the class of counterexamples is not guaranteed to be a set as is required for our induction. In fact it turns out that there are initial segments of X of the ordinals where $L_1(X) \neq L_2(X)$, and so no paradox arises.[162]

As Rosser concludes his own earlier discussion, "[I]t seems to be the case that there is no danger of deriving a contradiction along any of the known lines until one can handle unstratified relations more effectively…."[163] And this echoes Quine's point made earlier—with regard to consistency, we are in no better position in using other versions of set theory. We do not have absolute consistency results for set theory, and so our confidence in its consistency lies in our being unable to derive the paradoxes according to the usual known methods. Consistency for set theory—any set theory—rests upon our not having found a means for deriving the set-theoretic paradoxes, yet.

[162] Forster, *Set Theory with a Universal Set*, pp. 44-46.
[163] Rosser, "Consistency", p. 24.

IV. QUINE'S NATURALISM FROM A LOGICAL POINT OF VIEW

In this chapter I want to address Quine's philosophical origins. It sometimes seems as if his philosophical work did not begin until 1948 with the publication of "On What There Is" and then the seminal 1950 "Two Dogmas of Empiricism", culminating ten years later with his masterpiece *Word and Object*.[164] But by this time, Quine had already amassed a large body of work, though it is strikingly almost entirely within technical areas of mathematical logic.[165] In what follows, I will argue that Quine's more widely known philosophical views actually begin in this logical work going back to his dissertation of 1932.[166] To this point, there has been limited historical research on Quine, the work of Roger Gibson and Peter Hylton being exceptions.[167] But they both focus on Quine as epistemologist tracing much of his work back to Carnap. There is no doubt that Carnap was an important influence on Quine. But in focusing on his early logical work, I want stress the importance of Russell to Quine's development, an influence that has not yet received enough attention for our understanding of his philosophy.

Quine began his formal study of philosophy in 1926 at Oberlin College with an undergraduate degree in mathematics and honors reading in logic and mathematical philosophy. He then quickly moved through graduate school (in just two years) at Harvard. So it is no

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[164] W. V. Quine, "On What There Is" and "Two Dogmas of Empiricism" are collected in *From a Logical Point of View: Nine Logico-philosophical Essays*, rev. ed., (Cambridge: Harvard University Press, 1982); *Word and Object*, (Cambridge: MIT Press, 1960). The two essays originally appeared in 1948 and 1952 respectively.

[165] A quick look at Quine's bibliography makes the point rather quickly; see Ewin Hahn and Paul A. Schilpp, *The Philosophy of W. V. Quine*, expanded ed., (Chicago: Open Court, 1998), pp. 743-64. Out of nearly forty articles through 1950, approximately 25 of them are of a technical logical nature and all of his five books are logic texts. At least another five of the somewhat more philosophical articles are also primarily concerned with issues in logic.

[166] W. V. Quine, "The Logic of Sequences," (New York: Garland Publishing, 1990). A revised version was published two years later as *A System of Logistic*, (Cambridge: Harvard University Press, 1990). The reading I am pursuing here was suggested earlier by Burton Dreben in his "Quine," in Robert B. Barrett and Roger Gibson, eds., *Perspectives on Quine*, (Cambridge, Basil Blackwell, 1990), pp. 82-95. I aim to work out more of the details with a particular focus on Russell's influence.

[167] See for example Roger Gibson, *The Philosophy of W. V. Quine: An Expository Essay*, (Tampa: University of South Florida Press, 1982), and *Enlightened Empiricism: An Examination of W. V. Quine's Theory of Knowledge*, (Tampa: University of South Florida Press, 1992); and Peter Hylton, *Quine*, (New York: Routledge, 2007).

wonder his philosophical origins are somewhat hard to place.[168] He would have had very limited time to engage thoroughly with much philosophy at all. But we do know at least one constant in this period—Russell and Whitehead's *Principia Mathematica*. He learned logic from this text as an undergraduate, and it is also the source of his dissertation and the book version that followed.

At the most general level, the influence that Russell's paradox had on Quine's philosophy cannot be stressed enough. As Quine famously remarked in his 1941 essay on Whitehead,

> But a striking circumstance is that none of these proposals [for coming to terms with the paradoxes], type theory included, has an intuitive foundation. None has the backing of common sense. Common sense is bankrupt, for it wound up in contradiction. *Deprived of his tradition, the logician has had to resort to mythmaking.* That myth will be best that engenders a form of logic most convenient for mathematics and the sciences; and perhaps it will become the common sense of another generation.[169]

Though less vividly, Quine displayed this attitude already in the 1934 *A System of Logistic* remarking that type theory serves its purpose if it blocks the paradoxes.[170] He does not see types capturing anything like the essence of sethood (for Quine there is no essence to be had). It merely restricts set theory enough to prevent contradiction while leaving enough strength in place for it to still serve as a framework for the rest of mathematics. While not stated explicitly, this attitude is certainly present in the dissertation as well. And more importantly, it is an attitude that becomes a constant in his philosophy generally. It is the idea that science is a theory we construct, though as such, no less real and perhaps best captured in the oft quoted remark that "[t]o call a posit a posit is not to patronize it."[171] This idea informs my reading of Quine throughout. I will return to it especially in concluding this chapter where I discuss the philosophical significance of his version of logicism.

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168 Quine's biographical details can be found in various sources including his own autobiography *The Time of My Life*, (Cambridge: MIT Press, 1985) and his abbreviated version in Hahn and Schilpp, op. cit., pp. 2-46; 729-41.
169 W. V. Quine, "Whitehead and the Rise of Modern Logic," in *Selected Logic Papers*, enlarged ed., (Cambridge: Harvard University Press, 1995), p. 27 (my emphasis).
170 W. V. Quine, *System of Logistic*, p. 19.
171 Quine, *Word and Object*, p. 22.

More specifically I will focus on how Quine's naturalism arises out of his engagement with Russell's work in the foundations of mathematics. There are two aspects of Russell's philosophy I will look to. One is his attitude towards the mathematical and the philosophical, particularly as this arises with regard to definitions. No doubt he sees developments in mathematical logic as answering to philosophical problems. But the relationship between the two realms is often vexing. As an example of this I will look to difficulties that arise for him in trying to account for the unity of a proposition. The other aspect I will examine is Russell's difficulties with trying to explain the nature of logic, particularly as brought to the fore by his axiom of reducibility (though type theory and the paradoxes perhaps already raise this issue as displayed in Quine's remarks on Whitehead above). As we will see, logicism for him is part of a more general philosophical argument against Idealism. For it to be successful, mathematics must reduce to something that can reasonable thought to be logic. Reducing mathematics to something that is just more mathematics, for example, will not do the philosophical work he wants logicism to do.

Quine, armed with the insight that common sense is bankrupt, will be able to offer solutions to these problems, or perhaps more accurately, dissolutions of them. He does so by fully committing to Russell's view that technical developments in mathematics can solve philosophical questions. Quine resolves any tension between the mathematical and the philosophical by naturalizing Russell's logic. For him logicism becomes another chapter of mathematics in general, answerable to the best science of his day. I will conclude this chapter remarking that logicism still has philosophical relevance for Quine. But its relevance is now within his revolutionary naturalized philosophy. Logicism serves the philosophical purpose of simplifying and clarifying our understanding of mathematics, and as such contributes to the aim for our conceptual scheme as a whole.

Before I begin, I should mention one guideline of textual interpretation I have followed. I have focused only on the works of Russell we know Quine read in this early period, and primarily on *Principles of Mathematics* and *Principia Mathematica*. In some cases, I have then ignored texts which may have seemed relevant.[172]

I

Russell famously opens *Principles of Mathematics* declaring his twofold aim as first, "the proof that all pure mathematics deal exclusively with concepts definable in terms of a very small number of fundamental logical concepts, and that all its propositions are deducible from a very small number of fundamental logical principles…." And second, "the explanation of the fundamental concepts which mathematics accepts as indefinable," the purpose being "to see clearly…the entities concerned, in order that the mind may have that kind of acquaintance with them which it has with redness or the taste of a pineapple." This second aim, he describes as "purely philosophical".[173] The purpose of such a reduction of mathematics to logic—what has become known as "logicism"—was not somehow to better assure us of the certainty of mathematics, but was rather part of a complex argument against Idealism. Russell aimed to show that, contrary to the Idealists, at least some of our non-metaphysical knowledge did not ultimately lead to contradiction, and this he thought he could do by way of reducing mathematics

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[172] The other Russell texts we can be certain Quine read in this early period are *Marriage and Morals, Skeptical Essays, Philosophy, Our Knowledge of the External World, A-B-C of Relativity*, and *Introduction to Mathematical Philosophy*; see Quine's autobiography, *The Time of My Life*, (Cambridge: MIT Press, 1985), p. 58. The most relevant text I have ignored is Russell's "The Regressive Method of Discovering the Premises of Mathematics," in Douglas Lackey, ed., *Essays in Analysis*, (London: George Allen and Unwin, 1973), pp. 272-83.

[173] Bertrand Russell, *Principles of Mathematics*, p. xv.

to logic.[174] Immediately, we see here one of the most striking and important features of Russell's philosophy—that the technical and the philosophical are intimately entwined.[175] It is a theme we will constantly return to throughout this chapter in seeing how Quine's own philosophy emerged from this entanglement. And in a sense, we will see him as liberating logicism from many of the philosophical burdens Russell placed upon it, though Quine too will have philosophical aims upon which he too brings the technical to bear.

In his attack on Idealism, Russell did not limit his project to specific concerns within the philosophy of mathematics. His purpose was much grander than this, one of general philosophical concern. As he himself explains in his preface to *Principles* the project had its origins in trying to cope with some difficulties in the foundations of dynamics, which led him to consider the philosophy of continuity and the infinite and so also the foundations of mathematics. Logicism in Russell's hands, then, was part of a broader project of providing a consistent account of science generally, and in particular, it showed in contrast to Idealism that a consistent account of absolute space was ready to hand. Truth did not come in degrees as the Idealists claimed but rather, could be shown absolute. This, according to Russell, could be done by way of reducing mathematics to logic.[176]

Also, Russell's logic was not just a technical development that he then brought to bear on Idealism, but rather, the logic itself presupposed a particular metaphysics, a metaphysics which he drew directly from G. E. Moore:

> On fundamental questions of philosophy, my position, in all its chief features, is derived from Mr G. E. Moore. I have accepted from him the non-existential nature of propositions (except as happen to assert existence) and their independence of any knowing mind; also the pluralism which regards the world, both that of existents and that of entities, as composed of an infinite number of mutually independent entities,

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[174] For full details on Russell's emergence from and reaction to Idealism, see Peter Hylton's comprehensive study *Russell, Idealism, and the Emergence of Analytic Philosophy*, (Oxford: Clarendon Press, 1990); and also his "Logic in Russell's Logicism," in his *Propositions, Functions, and Analysis*, pp. 49-82.
[175] Hylton, ibid., emphasizes this aspect throughout his account of Russell's philosophy.
[176] For further detail on this point again see ibid., pp. 179-80.

> with relations which are ultimate, and not reducible to adjectives of their terms or of the whole which these compose. Before learning these views from him, I found myself completely unable to construct any philosophy of arithmetic, whereas their acceptance brought about an immediate liberation from a large number of difficulties which I believe to be otherwise insuperable. The doctrines just mentioned are, in my opinion, quite indispensable to any even tolerably satisfactory philosophy of mathematics, as I hope the following pages will show.[177]

This is an atomistic metaphysics committed to abstract objects—propositions, and the view that truth and falsity are absolute. It is what Hylton call "Platonic Atomism".[178] According to this metaphysics, our knowledge of the world is direct and unmediated. This runs directly contrary to the view of the Idealists who claimed that all of our knowledge was conditioned by various conceptual structures.[179] These structures, they claimed, were objective; they correspond to they way the world actually is. But this is difficult to defend as Hylton explains. If all knowledge is mediated, then the claim itself that the world does in fact correspond to these conceptual structures must also be mediated by other conceptual structures. But then the question returns: what of the objectivity of these other conceptual structures? And so on into an infinite regress. While the Idealists did in fact have responses to such charges, neither Moore nor Russell saw them as sufficient to sustain the view that such mediated knowledge could in fact be objective knowledge. We could not, on the Idealist picture, have genuine true knowledge of how the world really is.

Against this, Russell, following Moore, denied this central claim of Idealism and instead took our knowledge of the world to be direct.[180] Our perception of the world is of what the world is really like. I speak here of perception in a very broad sense since both Russell and

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[177] Russell, *Principles*, p. xviii.
[178] My account of Russell's metaphysics in the period of his logicism follows that of Hylton. See his *Russell and the Emergence of Analytic Philosophy*, as well as his *Propositions, Functions, and Analysis*, (Oxford: Clarendon Press, 2005). Many of the essays in this volume deal with relevant topics, but here I follow the brief account presented in "The Theory of Descriptions," pp. 189-94.
[179] As Hylton notes, in Kant's case this account brushes over the distinction he draws between intuitions and concepts; p. 190, fn. 13.
[180] The views Russell takes from Moore are put forward perhaps most clearly in Moore's "The Nature of Judgment," in *G. E. Moore: the Early Essays*, ed. Tom Regan, (Philadelphia: Temple University Press, 1986), pp. 59-80.

Moore thought we could have something like perception of ordinary everyday physical objects as well as of abstract objects. Indeed, we have already seen this in how Russell explains the very aim of his logicist reduction, "*to see* clearly…the entities concerned, in order that the mind may have that kind of acquaintance with them which it has with redness or the taste of a pineapple" (my emphasis). Acquaintance is a relation that gives the mind direct unmediated access to what is outside of it.

So among the ideas Russell takes from Moore is that of direct unmediated access to the world, and it is in this context that propositions enter as central to Russell's philosophy. Now returning to the above quoted passage—in line with the objectivity gained by this direct access to the world, Russell then states that propositions are entities independent of any knowing mind. As he understands them, propositions are non-linguistic, non-mental abstract entities and, roughly speaking, the content expressed by a declarative sentence. In making a judgment, we have acquaintance with propositions, which have among their properties those of being true or false. In a true judgment, we have acquaintance with a true proposition, and in a false one, acquaintance with a false proposition. So again, in contrast to the Idealists, truth and falsity do not depend on any sort of mediation by ideas in a mind but rather, results from a direct relationship to a proposition with the requisite property of truth or falsity.

Having stated that propositions are independent of any knowing mind, Russell next introduces the crucial idea of analysis. His view of propositions is atomistic, and it is in this sense that he regards the world as pluralistic and made up of "an infinite number of mutually independent entities". Propositions are complex and so can be analyzed into their basic constituents, which he calls 'terms'. Hylton explains that, in the mind of Russell, this process of analysis is analogous to something like chemical decomposition. For example, the proposition, 'Socrates is wise' analyzes into its two basic components Socrates and wisdom, since, in paradigm cases, the proposition actually contains the objects which it is about.[181] Finally, Russell concludes the quoted passage remarking that it was only after he had adopted Moore's philosophical views that he was able to successfully develop his philosophy of arithmetic. It is at this point, I think, that a certain tension begins to emerge in Russell's philosophy and one which will be crucial to the understanding of Quine I wish to present. This tension is one that emerges between the philosophical and the mathematical in Russell's work. One the one hand he takes mathematical results to be relevant to philosophical problems, and even to solve them. Yet on the other, he still holds that there is some special further task for philosophy to do beyond mathematics. As I will argue, Quine will resolve this tension by fully accepting Russell's view that technical results can solve philosophical problems, but in doing so he will let the further task Russell claims for philosophy go by the wayside. It is in fully committing the idea that mathematics can solve philosophical questions that, I claim, we have the origins of Quine's naturalistic philosophy. In short, we have him taking the best methods of the science of his day—in this case, those of the new mathematical logic—and using them to both clarify and resolve philosophical questions. As this tension most clearly emerges in Russell's talk of philosophical and mathematical definitions, let us now turn to consider them directly.[182]

Russell begins *Principles* explaining that pure mathematics is the class of propositions of the form 'p implies q', where p and q are propositions each containing at least one variable, the same in each proposition, and having no constants but logical constants. The logical constants, he says, are notions definable in terms of implication, the relation of a term to the class of which

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[181] Hylton, *Propositions*, pp. 9-10, 191-2.
[182] Hylton sketches out a similar view in *Russell, Idealism, and the Emergence of Analytic Philosophy*, pp. 233-36, pointing out the anti-metaphysical attitude that one half of the tension leads to. It is this line of thought that I claim to be emerging in Quine.

it is a member, such that, relations, and other further notions involved in propositions of the above form. He also includes the notion of truth, though not itself a constituent of propositions. His aim will be to justify that, in fact, this definition suffices to account for what has traditionally been thought pure mathematics. This definition he then claims is

> not an arbitrary decision to use a common word in an uncommon signification, but rather a precise analysis of the ideas which, more or less unconsciously, are implied in the ordinary employment of the term. Our method will therefore be one of analysis, and our problem may be called philosophical—in the sense, that is to say, that we seek to pass from the complex to the simple, from the demonstrable to its undemonstrable premisses.[183]

So the notion of analysis is crucial, and it is a philosophical notion which leads us to the simple components at the basis of mathematics. Indeed, it is by this method that will aid us in obtaining acquaintance with the indefinables of mathematics "as the necessary residue in a process of analysis".[184] But what exactly is this process of analysis?

On first pass, it seems that analysis is merely the technical reduction of mathematics to logical notions. As Russell explains, it will be through "the labours of the mathematicians themselves" that he will be able to obtain certainty and clarity with regard to his questions concerning the nature of number, infinity, space, time, and motion, and of mathematical inference itself. In reducing such questions to questions of pure logic, we find exact knowledge about mathematics. This, however, is not the complete story. He continues, explaining that previously the philosophy of mathematics had been just as controversial and unprogressive as other branches of philosophy. Philosophy demanded a meaning to mathematics, but mathematics had no answer. Now, Russell claims, mathematics does have an answer "so far at least as to reduce the whole of its propositions to certain fundamental notions of logic." So it might seem again that the reduction of mathematics answers what used to be the concerns of philosophy. It is not uncommon to now think that in its technical reduction logicism has, for

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[183] Russell, *Principles*, p. 3.
[184] Ibid., p. xv.

example, shown us what the natural numbers are. But this is not so for Russell as he then states, "At this point, the discussion must be resumed by Philosophy."[185] Beyond the technical reduction of mathematics to logic, Russell believes there is still some further task for philosophy to carry out. This becomes all the more apparent if we consider his contrasting of two notions of definition—the philosophical and the mathematical.

Russell first introduces this distinction between philosophical and mathematical definition in discussing Peano's logical work:

> It is necessary to realize that definition, in mathematics, does not mean, as in philosophy, an analysis of the ideas to be defined into constituent ideas. This notion, in any case, is only applicable to concepts, whereas in mathematics it is possible to define terms which are not concepts. Thus also many notions are defined by symbolic logic which are not capable of philosophical definition, since they are simple and unanalyzable.[186]

He goes on to explain that in mathematics we define by simply by picking out some fixed relation to a fixed term, which only one term can have. The basic idea is that mathematical definition consists in giving necessary and sufficient conditions for an entity to fall under a concept.[187] He then distinguishes philosophical definitions remarking, "The point in which this differs from philosophical definition may be elucidated by the remark that the mathematical definition does not point out the term in question, and that only what may be called philosophical insight reveals which it is among all the terms there are."[188] So while necessary and sufficient conditions are enough for mathematical definition, philosophical definition requires something more. It seems that the mathematical definition can pick out any particular object from among several satisfying the requisite conditions, but philosophical definition goes deeper, telling us in some sense which objects the defined term *really* is or what its *true* meaning is.[189] Russell has

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[185] Ibid., pp. 4-5.
[186] Ibid., p. 27.
[187] Ibid., p. 26-7, 111; Hylton, *Russell, Idealism, and the Emergence of Analytic Philosophy*, pp. 187-8.
[188] Russell, *Principles*, p. 27.
[189] He makes this point in discussing the analysis of betweeness in section 196 of *Principles*.

this sense of definition in mind when he talks of analysis as the method for carrying out his project, as a method for leading us to acquaintance with the objects of pure mathematics.[190]

Such an understanding of philosophical definition fits well with Russell's later remarks in *Principles* on defining cardinal numbers. Here, he repeats his account of mathematical definition and then explains that "philosophically, the word *definition* has not, as a rule, been employed in this sense; it has, in fact, been restricted to the analysis of an idea into its constituents."[191] So again, we see this idea of decomposition from our earlier discussion of analysis. Philosophical definition, then, is an analysis of an idea into its basic components. This notion of definition leads us to acquaintance with the real reality, a metaphysical reality, if you will, which mathematical definition does not necessarily do.

I say that mathematical definition does not *necessarily* lead us to the sort of acquaintance that philosophical definition does because it seems that it can in some cases. And here I think it starts to become apparent what sort of tension these two distinct notions of definition lead to in Russell's philosophy. Indeed, he continues in this passage stating of the philosophical notion,

> This usage is inconvenient and, I think, useless; moreover it seems to overlook the fact that wholes are not, as a rule, determinate when their constituents are given, but are themselves new entities …, defined, in the mathematical sense, by certain relations to their constituents. I shall therefore, in future, ignore the philosophical sense, and speak only of mathematical definability.[192]

This is at first puzzling if philosophical definition and analysis are to be counted as the same thing and analysis the key method for carrying out Russell's project. And even if the two notions are not supposed to come to the same thing, his earlier remarks on philosophical definition are in no way disparaging and seem to carve out an important role for philosophy. I think what Russell intends here is that philosophical definitions are useless when it comes to discussing the technical, mathematical details of his reduction as he does in this section. He still thinks there is

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[190] Hylton makes this point in *Russell, Idealism, and the Emergence of Analytic Philosophy*, p. 232.
[191] Russell, *Principles*, p. 111.
[192] Ibid., pp. 111-12.

a role for philosophical definition, but it is in reflecting on what the mathematical definition has accomplished that it comes into play. But increasingly, this yields also a tension between the philosophical and the mathematical. Once the mathematical definition has presented the requisite sorts of objects for the mathematics, what further analysis is there to do? What further question is there to answer? As we will see, Russell continues to look at both notions of definition, but increasingly we will see him as taking the mathematical notion to do the work of the philosophical. Yet, he continues to hold on to the philosophical, metaphysical foundation of his project. At best then it is unclear what is left for the philosophical notion of definition to do. And at worst, it leaves Russell with certain irresolvable problems, chief among the unity of the proposition to which we will soon return.

To illustrate this, let us consider further Russell's account of the definition of cardinal number. He defines the number of a class as the class of all classes similar to the given class, where two classes are similar if their members can be put into one-to-one correspondence. What all similar classes have in common, then, is their number.[193] So a number just is a class of a particular sort. Although this definition of cardinal number succeeds mathematically speaking, Russell admits some philosophical worries concerning the connection between classes and predicates. In a previous section, Russell had explained that a philosophical definition is "the analysis of the idea … into constituent ideas," but that this notion "in only applicable to concepts," unlike definition in mathematics.[194] So a philosophical difficulty remains in that he is uncertain whether appropriate concepts can be found to identify with the numbers. He concludes, however,

> For my part, I do not know whether there is any such class of predicates, and I do know that, if there be such a class, it is wholly irrelevant to Mathematics. Wherever Mathematics derives a common property

[193] Ibid., pp. 113-15.
[194] Ibid., p. 27.

> from a reflexive, symmetrical, and transitive relation, all mathematical purposes of the supposed common property are completely served when it is replaced by the class of terms having the given relation to a given term; and this is precisely the case presented by cardinal numbers. For the future, therefore, I shall adhere to the above definition, since it is at once precise and adequate to all mathematical uses.[195]

While Russell sees his project as inherently philosophical, his criterion of philosophical success, as we see here illustrated, is often mathematical.[196]

I take it that this view is fairly constant throughout Russell's work in the foundations of mathematics. Though he makes little explicit use of it in *Principia Mathematica*, it does appear again in the later *Introduction to Mathematical Philosophy*, in a perhaps even stronger form. Here, he remarks again after discussing the notion of similarity relevant to the cardinal numbers, "It follows from this that the mathematician need not concern himself with the particular being or intrinsic nature of his points, lines, and planes, even when he is speculating as an *applied* mathematician."[197] So again, from the mathematical perspective, there appears to be no further question about the essence of points to ask as Russell continues, "It has to be something that as nearly as possible satisfies our axioms, but it does not have to be 'very small' or 'without parts.' Whether or not it is those things is a matter of indifference, so long as it satisfies the axioms."[198] A point is, then, any object satisfying the axioms concerning points, and this is what matters to mathematics. And he concludes, "This is only an illustration of the general principle that what matters in mathematics, and to a very great extent in physical science, is not the intrinsic nature of our terms, but the logical nature of their interrelations."[199] Interestingly, he sees this as not

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[195] Ibid., p. 116.

[196] This same point can be made of Russell's discussion of the definition of 'between' in Chapter XXV, where he concludes that the mathematical definition, with some slight emendations, "give[s] the very *meaning* of between" (Russell's italics). And in section 270, he concludes that "there is no logical ground for distinguishing segments of rationals from real numbers."

[197] Ibid., p. 59; Russell's italics.

[198] Ibid., p. 59.

[199] Ibid., p. 59; my emphasis.

only a feature of mathematics, but one also of the natural sciences—a view that will take front and center in Quine's philosophy.

Russell's account of definition is not the only place where the tension between the mathematical and the philosophical arise. Recall, our discussion of the axiom of reducibility in Chapter two. Here, Russell motivated the distinctions of order and the resulting types by way of his vicious circle principle as applied to propositional functions, but ultimately justified his system on its success in doing away with the paradoxes. The solution is at once both mathematical and pragmatic—mathematical in that it yields technical resolution to the paradoxes and pragmatic in that the criterion of success for his system is that the paradoxes are blocked. I should emphasize also that the solution is highly philosophical in that it is rooted in the metaphysics of propositions, which he brought to bear on his Idealist opponents. In this vein, it is notable that Russell does not give up Platonic atomism as a philosophical position despite initially coming upon his famous paradox in terms of propositional functions. Furthermore, we should observe that, in contrast to Ramsey and Quine (who both distinguished the semantic from the class paradoxes), Russell does not opt for a simple theory of types blocking just the class-related paradoxes, that is, the paradoxes that seem mathematically relevant. Rather, he vastly complicated his universe with the system of orders and types rather than give up his fundamental metaphysics. Since logic presupposed the metaphysics of propositions, a solution to the paradoxes meant eliminating the semantic and class paradoxes all at once.

Recall, though, that once Russell introduced the type distinctions, he still required some way to talk of all properties if his logic was going to be sufficiently strong to yield mathematics. And to this end, he puts forth his axiom of reducibility. The axiom states that for every propositional function there is an extensionally equivalent predicative propositional function, that is, an extensionally equivalent propositional function of the lowest order compatible with its arguments.[200] While Russell clearly takes the existence of propositional functions as part of his logic, reducibility is problematic in its existence claims. Logically speaking there seems to be no reason to assume the existence of the predicative propositional functions the axiom puts forth. It seems a fact about the world we could just as easily reject as accept. This raises a difficulty about the nature of logic for Russell. Indeed, one of Wittgenstein's most persistent criticisms of Russell's logic was questioning the justification for reducibility.[201] The absolute truth of logic functioned as a crucial component of Russell's attack on idealism, and it is hard to argue on purely logical grounds that this axiom is unconditionally true. Furthermore, the axiom does not seem to fit with any traditional characterization of logic as self-evident, a priori, or analytic. Even taking a Quinean stance and saying that logic is just among the truths we hold to most firmly in our web of belief would seem to do little for the logical status of reducibility. So a crucial axiom in Russell's logicism fails in its logicality.

But Russell recognizes this and in fact does not try to ground the axiom in pure logic. Rather, he argues,

> The reason for accepting an axiom, as for accepting any other proposition, is *always* largely inductive, namely that many propositions which are nearly indubitable can be deduced from it, and that no equally plausible way is known by which these propositions could be true if the axiom were false, and nothing which is probably false can be deduced from it. If the axiom is apparently self-evident, that only means, practically, that it is nearly indubitable; for things have been thought to be self-evident and have yet turned out to be false.[202]

Russell's justification for reducibility, then, is wholly pragmatic, relying on how successfully the system in which it is used captures mathematics. In his preface to *Principia* he goes even further

[200] Russell and Whitehead, *Principia*, p. 56.
[201] For example, see Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*, (New York: Routledge, 1990 [1922]), 6.1232-6.1233; also *Cambridge Letters: Correspondence with Russell, Keynes, Moore, Ramsey, and Sraffa*, Brian McGuinnes and Georg Henrik von Wright, eds., (Malden: Blackwell Publishers, 1997), pp. 35, 53-4, 59-61.
[202] Russell and Whitehead, *Principia*, p. 59; my emphasis. Russell's earlier 1907 essay "The Regressive Method of Discovering the Premises of Mathematics," in *Essays in Analysis*, Douglas Lackey, ed., (London: George Allen and Unwin, 1972), pp. 272-83, expands on these views considerably. In keeping only to texts that we know Quine read in his early years I have left consideration of this essay out since it was not published until 1972.

making this point about axioms generally (sounding very much like Zermelo in his 1908 paper; see chapter 2 above) stating that "the chief reason in favour of *any* theory on the principles of mathematics must always be inductive, i.e. it must lie in the fact that the theory in question enables us to deduce ordinary mathematics."[203]

Russell does, of course, mention the self-evidence of an axiom in the above quote but in a way that appears strikingly free of any strong philosophical commitment. Indeed, his use of the terms sounds largely deflationary remarking that it "only means" from a *practical* perspective that a self-evident axiom is "nearly indubitable." In this sense, it differs little from walking out into the rain and declaring that it is raining. Furthermore, self-evidence does not yield any strong justification for an axiom since some apparently self-evident truths have turned out false, specifically Frege's basic law V, the source of Russell's paradox. And regardless of whether reducibility is self-evident, Russell qualifies the inductive reason for accepting an axiom as the *chief* reason for accepting it. So on these grounds reducibility is perfectly acceptable as part of Russell's logical system. But again, the tension in Russell's view between the philosophical and the mathematical becomes apparent. In the service of an argument against Idealism, logicism needs to be a reduction to something we can all agree to label as logic. In particular, logic must be absolutely true. Reducibility makes this highly problematic since it seems just as likely to be true as to be false. As a merely formal technical reduction of mathematics to logic, however, Russell's system succeeds thus making available the pragmatic justification he takes to justify any axiom, including reducibility. As we will see, it is this aspect of Russell that Quine latches on to, noticeably absent is any concern with the questions over the nature of logic.

II

[203] Ibid., p. v (my emphasis). He also expresses a very similar sentiment in the earlier *Principles*, p. xviii.

Quine's philosophy developed both under the influence of and in reaction to Russell. In particular, we can see Quine as taking Russell's logic and stripping it of—what Quine sees as—its excess metaphysical baggage. Indeed, perhaps most striking about Quine's earliest logical work is that there seems almost no philosophy connected to it. He does not discuss the nature of logic, the philosophical payoffs of logicism, or any other such topic we might expect from someone emerging from the philosophy of *Principles of Mathematics* and *Principia Mathematica*. As Dreben emphasizes, Quine begins by just doing logic, not talking about it.[204] Quine's logical beginnings are not part of a larger philosophical argument against Idealism, but I do believe that we can find the philosophy in Quine's early logic by thinking of this work directly in relation to Russell himself. In this section we will see Quine dropping many of the more philosophically extravagant aspects of Russell's philosophy, and in doing so, we will come to understand Quine's philosophical project from within its logical origins. I want to bring how it is that the framework—the naturalistic framework—within which Quine's philosophy takes place over the next seven decades begins to unfold already in his earliest logical works.

The seriousness of Quine's engagement with Russell is demonstrated by his dissertation, "The Logic of Sequences", in which Quine reworked the first 500 pages of *Principia*.[205] While *Principia* may not seem an explicitly philosophical treatise, we have seen its philosophical basis and motivations thoroughly expounded in the earlier *Principles*.[206] Indeed, as Russell and

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[204] Dreben makes this point when considering Quine's logical work against the backdrop of the Harvard school; see his "Quine," pp. 83-4. Another point Dreben has emphasized in interpreting Quine is that Quine begins in the middle of things; see his "In Mediis Rebus," *Inquiry* 37, (1994), pp. 441-7. That Quine does not offer a special philosophical status to logic should be no surprise if we begin in the middle of things.

[205] W. V. Quine, "The Logic of Sequences: A Generalization of Principia Mathematica," (New York: Garland Publishing, 1990). The dissertation was originally completed and submitted on April 1, 1932; Dreben, "Quine," p. 81.

[206] In this way, *Principles* is very much to *Principia* as, for Frege, *Grundlagen* is to *Grundgesetze*. There are also major philosophical shifts between *Principles* and *Principia*, particularly with regard to ontology and Russell's famous intervening work "On Denoting," in *Logic and Knowledge: Essays 1901-1950*, Robert C. Marsh, ed., (London: George Allen and Unwin, 1956), pp. 39-57. For Frege, see *The Foundations of Arithmetic: A Logico-*

Whitehead explain in their preface, *Principia* was originally intended as the second volume to *Principles* but it then took on a life of its own.[207] It is fair to say, then, that *Principia* has a very definite philosophical purpose; this is not just a technical demonstration of how mathematics reduces to logic. This is deep philosophy thoroughly intertwined with the technical apparatus of the new logic. In opening Quine's dissertation, however, we are immediately struck by this sharp contrast with Russell's work. It completely lacks a sketch of any obviously philosophical motivations for the work. It does seem a purely technical exercise. But I contend that this is not so. The philosophy is there if we know where to look. Let us turn to Quine's preface to see how this might go.[208]

He begins, explaining that he views the dissertation as allied with *Principia* but that it is more comprehensive in that it generalizes *Principia's* account of propositional functions and relations. *Principia* has a system of monadic propositional functions to do the work of classes along with a separate theory of dyadic relations. Parallel axioms and theorems must then begiven for each of the two distinct realms. Furthermore, Russell's system does not generalize to n-adic relations; it is impossible to prove theorems in general for n-adic relations without first specifying the value of n. Since dyadic relations are enough for reducing mathematics, this is as far as Russell goes. Quine then explains that this is precisely the sense in which he has generalized *Principia*. By employing a system of sequences, his system allows for classes and relations to be treated singly, not as two independent realms. As a result, he does not need to then reprove parallel theorems for relations once he has done so for classes. Furthermore, such sequences may be of any arbitrary length, so that theorems for relations are proved generally for

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mathematical Enquiry into the Concept of Number, J. L. Austin, trans., 2nd rev. ed., (Evanston: Northwestern University Press,); *Grundgesetze der Arithemetik I/II*, (New York: Georg Olms Verlag, 1998).

[207] Russell and Whitehead, *Principia*, p. v.

[208] The account that follows all comes from Quine's preface to the dissertation. There are however no page numbers, but it is only three pages in length. I have numbered them with small case Roman numerals, i-iii.

any n-adicy. In addition to this generalization, Quine observes that his system is more economical in primitive ideas though also more elaborate in its postulates. He also believes that he has achieved greater definitional elegance. All of these concerns of generality, economy, and simplicity are quite familiar now as primary concerns of Quine's later and more general philosophical outlook. We will return to some of this topic in concluding this chapter.

One might question the direct link I am claiming here between Russell and Quine. After all, these concerns are often found in the works of the more technically minded. For example, Gödel shares such aims as generality, economy, and simplicity and famously criticized Russell for his slovenliness but is not a Quinean naturalist of any sort.[209] I claim there is a unique connection here between Russell and Quine given that Quine learned his logic from *Principia Mathematica*.[210] Strikingly, Quine did not fall victim to the confusions found in Russell's logic, achieving a level of clarity that would have satisfied even Gödel. Quine, unlike Russell, steadfastly distinguishes between use and mention, between informal talk about the system and formally working within it, adheres to an extensional view of logic, and in general pursues a program of ontological economy, themes we will be returning to throughout this chapter.[211] For Quine, such seemingly technical concerns become part of the philosophical work. As he conceives of philosophy, clarifying our scientific theories is a properly philosophical activity furthering our understanding of the world. From such moves against Russell emerge an accounting of mathematics consistent with a naturalistic philosophy. There is no first philosophy to dictate a single correct theory of our world but rather, only such broadly pragmatic criteria as

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[209] Kurt Gödel, "Russell's Mathematical Logic," in Paul Arthur Schilpp, ed., *The Philosophy of Bertrand Russell*, (LaSalle: Open Court Press, 1971), pp. 123-53.
[210] Quine, *Time of My Life*, p. 59.
[211] Though there may be reason to think Quine and Gödel's charges of unclarity against Russell rest of a misunderstanding of his logic; see Hylton, "Logic in Russell's Logicism," in *Propositions, Functions, and Analysis*, pp. 49-82.

generality, economy, and simplicity. Clarity is a distinctly philosophical aim for Quine, and we see it here in how he reshapes Russell's logicism.

Before delving further into the details of Quine's early logical work, let us return to consider the nature of logic as I believe this issue will shape much of what will follow. As we have remarked on several times previously, both in this chapter and in others, the paradoxes of set theory greatly impacted how Quine conceived of the logicist project. Indeed, I claim that this accounts for why Quine felt no need to give an account of the nature of logic. As he explains at the outset of the published version of his dissertation, *A System of Logistic*, "The theory of types fulfills its purpose of avoiding contradictions by branding such and such combinations of symbols as meaningless."[212] Here, Quine makes his earliest explicit remark on the pragmatic aspect of set theory arising from the contradictions, though this was also the attitude implicit in the dissertation itself. Whereas Russell found himself in conflict with his own logical inclinations when it came to justifying the axiom of reducibility, and even types to an extent (as seen in chapter 2), inductively, Quine accepts from the paradoxes and their resolution that the foundations can no longer be thought of as an a priori science. Any account of logic open to Quine will have to be come from within mathematics, and so for Quine there is no tension between his logicism and his philosophical aims. This leaves him free to work out a logical system satisfying his particular philosophical concerns of simplicity, clarity, and economy. The first chapter of the dissertation brings out these aims in his adopting an extensional account of propositional functions, treating talk of them as interchangeable with talk of classes. Here, we come to see his earliest concerns with notions of meaning, or intensionality.

He begins by introducing his primitives, among them the notion of a function, or propositional function, i.e., a function whose values are propositions. He explains that his

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[212] Quine, *System of Logistic*., p. 19.

notation φ, ψ, χ, … is indifferent as to representing propositional functions or classes and relations. Whereas Russell opted for the threefold notation φ, ψ, χ, …; α, β, γ, …; and P, Q, R, … to represent propositional functions, classes, and relations respectively, Quine takes these all as propositional functions. In this way he represents x ε α, x is a member of α, and xRy, x bears the relation R to y, simply as φx and ψ(x, y), which are just special cases of his sequence notation χ(x, y, z, …).[213] And he adopts a version of Russell's type theory to block the paradoxes.[214] We can see in his notation his basic aims towards generality and simplicity, and as such unification, rejecting that there is anything essential to distinguishing propositional functions, classes, and relations. But more importantly, without any fanfare, Quine states that all such functions are to be taken extensionally. This signals a strong break with Russell and his insistence on taking intensional propositional functions and relations as the basis for the extensional theory of classes upon which he could then construct mathematics.

That this is an important philosophical move against Russell cannot be emphasized enough. Russell's adoption of propositional functions as the basis for his logic was not without thought or motivation, as Hylton explains.[215] First, these were the only sorts of objects Russell took to have the requisite generality to be properly logical. Previously, he had granted this status also to classes, but with the discovery of the class paradox, he recognized that he would have to subject classes to some sort of restriction. Second, since classes could be defined contextually in terms of propositional functions, unresolved questions about their nature—whether they were

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[213] Quine, "Logic of Sequences," p. 5.
[214] From the beginning Quine takes Russell's typical ambiguity quite seriously, also using it a device for simplifying the system. Rarely does he make type distinctions explicit but rather relies on context for their determination. It was thinking of typical ambiguity in this way that, in large part, led him to New Foundations; see his "On the Inception of N. F." in *Selected Logic Papers*, pp. 286-9. A related point of simplification from *System of Logistic* is that he counts all sequences as legitimate, but propositions are the special subclass of sequences $x_n \in y_{n+1}$, though in his sequence notation this would be y,x. Quine does not take epsilon as primitive but rather captures it in the comma flanked by variables of the requisite types (pp. 26-30).
[215] For more detail on Russell's reasons for taking propositional functions as fundamental and difficulties arising from doing so, see Hylton, *Russell, Idealism, and the Emergence of Analytic Philosophy*, pp. 288-327.

intensional or extensional and how a class of many objects could be treated as a single object—no longer required answers. And third, as we saw already in chapter 2, Russell saw the ontological distinctions between types as flowing from and hence justified by the distinctions between propositional functions and individuals, which emerged from his account of propositions. Hence, the resolution of the paradoxes and the view of the universe as structured into a hierarchy of types rested upon taking propositional functions as primary. Russell had no similar justification for the hierarchy if he instead took classes as fundamental.

Quine of course rejects this intensional basis for logicism and instead treats propositional functions extensionally identifying them with the classes of objects they are true of, their extensions.[216] The move comes without immediate justification, but it is implicit in the entire aim of the dissertation—showing that Quine's system is adequate for yielding the entire system of PM.[217] As such, there is no need to adopt intensional propositional functions. Their extensional counterparts are enough. These motivations become all the more apparent in the published version of the dissertation, *A System of Logistic*, as he explains,

> This assimilation of propositional functions to classes and of predication to membership represents no actual impoverishment of logistic, but only the elimination of useless lumber: for, as will subsequently be shown, all theorems of PM involving function variables or predication can be proved in the present system under the indicated manner of translation.[218]

But why should we prefer the extensional to the intensional? Quine says very little of the distinction in the dissertation, but again he elaborates his motivations in *System of Logistic* when discussing his extensional treatment of propositions. He warns that an intensional account may force us "to leave the terra firma of algorithmic logic and tread more metaphysical ground…."[219] In particular, he complains that propositions are rather difficult to pin down as identical. An

[216] Quine, "Logic of Sequences," pp. 3-5.
[217] Ibid., ch. XIII.
[218] Quine, *System of Logistic*, p. 31.
[219] Ibid., p. 33.

extensional account, however, provides a clear identity criterion. We can easily identify propositional functions as the same when they have the same extensions.[220]

Now, this all might seem rather ad hoc in comparison to what Russell thought a well-motivated development of the logic required for carrying out logicism. He himself thought just this, reflecting later in life on Quine's systems of logic being such that not "even the cleverist logician would have thought of [them] if he had not known of the contradictions."[221] But we have also seen Russell himself thinking in such a way with regard to his axiom system justifying it inductively on grounds that it "enables us to deduce ordinary mathematics."[222] It is this strand of Russell that Quine latches on to. In adopting an extensional view of logic Quine pushes aside difficult, if not hopeless worries, external to mathematics, and thus opens the way to a philosophy of mathematics that takes place entirely within mathematics itself. For deducing ordinary mathematics, only the extensions matter; all else is "useless lumber". In short, we can see him beginning to work out a philosophy of mathematics in which there is no first philosophy.

Let us return now to consider more directly the tension in Russell between the mathematical and the philosophical. In particular, I want to consider how this issue arises in the context of difficulties Russell has over the nature of propositions. According to Russell's metaphysics, propositions are composed of the entities which they are about. But analysis—the decomposition of propositions into their simple constituents—then raises a central difficulty for him. How were these component parts ever unified in a proposition in the first place? As Russell explains the problem in *Principles*:

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[220] For a later account and somewhat autobiographical account of Quine's preference for extensionality, see his "Confessions of a Confirmed Extensionalist," in Juliet Floyd and Sanford Shieh, eds., *Future Pasts: The Analytic Tradition in Twentieth-Century Philosophy*, (New York: Oxford University Press, 2001), pp. 215-21.
[221] Russell, *My Philosophical Development*, p. 61.
[222] Russell, *Principia Mathematica*, p. v. He remarks similarly in *Principles*, see p. xviii. And more specifically, as we saw earlier in this chapter and in chapter 2, he attempts to ground the axiom of reducibility in such a way.

> Consider, for example, the proposition "A differs from B." The constituents of this proposition, if we analyze it, appear to be only A, difference, B. Yet these constituents, thus placed side by side, do not reconstitute the proposition. The difference which occurs in the proposition actually relates A and B, whereas the difference after analysis is a notion which has no connection with A and B. It may be said that we ought, in the analysis, to mention the relations which difference has to A and B, relation which are expressed by *is* and *from* when we say "A is different from B." These relations consist in the fact that A is referent and B relatum with respect to difference. But "A, referent, difference, relatum, B" is still merely a list of terms, not a proposition. A proposition, in fact, is essentially a unity, and when analysis has destroyed the unity, no enumeration of constituents will restore the proposition. The verb, when used as a verb, embodies the unity of the proposition, and is thus distinguishable from the verb considered as a term, though I do not know how to give a clear account of the precise nature of the distinction.[223]

Analysis seems to yield mere lists of a proposition's constituents leaving it wholly mysterious what the unity of the proposition was originally to consist of. Such a view is inherent in the metaphysics Russell thought entirely necessary to a proper philosophy of arithmetic, namely that every component of a proposition is a term (or in Frege's terminology, an object). Constrained by his metaphysics, the problem is one Russell never fully resolves.[224]

Quine, however, does have a way of coping with such problems and in doing so we can see how he also resolves the tension Russell faced between the mathematical and the philosophical. Although, the two were deeply intertwined in Russell, Quine carries this view to it fullest conclusion, and in doing so, folds the philosophical into the mathematical. For Quine, philosophy takes place entirely within the best methods current science has to offer, in this case, the new mathematical logic.

To see how this plays out, let us examine the operation of predication, one of the "active primitive ideas" of the system of "Logic of Sequences".[225] Here, we can see how he brings his broadly pragmatic approach to logic—which I claim comes from at least one strand of Russell's thought—allowing him to resolve, or perhaps dissolve, worries about propositional unity. He describes predication as a binary operation upon a function and a sequence (an ordered set of

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[223] Russell, *Principles*, pp. 49-50.

[224] For more detail on the difficulties Russell faced here see Peter Hylton's essays, "The Nature of the Proposition and the Revolt Against Idealism," and "Beginning with Analysis," and on Frege and Russell, see his "Frege and Russell," all in his *Propositions, Functions, and Analysis*.

[225] The analogous operation in *System of Logistic* is ordination.

terms), which provides an argument sequence to a function. The product of predication he says is a proposition and is expressed by 'φX'. So we construct a proposition out of function and sequence by way of this primitive operation of predication, and this is all he takes there to be to a proposition. He is not, however, unaware of philosophical difficulties that may arise concerning propositions:

> But, it maybe be asked, what sort of thing is this product of predication? From the official standpoint of our system, it is to be answered only that it is whatever predication yields; and predication is primitive. Unofficially, we may say that by a proposition we mean exactly what one ordinarily means by the term; and, from this standpoint, we may describe predication as that operation upon function and sequence which renders the latter argumental to the former and produces a proposition.[226]

We see Quine, here, addressing such worries over the nature of the proposition by again applying the technical apparatus of mathematical logic, and in particular by carefully maintaining the distinction between working within the system and talking about it. A precise answer to the nature of a proposition can be given in terms of the operation of predication, and from an intuitive perspective this gives us what we ordinarily think of as a proposition. Quine has no absolute notion of a proposition. Rather he gives an account of it relative to the particular system he works within, suitable to whatever purposes propositions are to play in that system. In such a way, he uses the new logic of Russell to give a precise account of both the question and answer concerning worries of the nature of the proposition. This is all in great contrast with Russell, where issues raised by his metaphysics prevent any straightforward account of the nature of propositions.

Quine concludes his discussion emphasizing this sort of move from a slightly different perspective by remarking on how his account of propositions addresses worries about ontological categories:

> This treatment, however, is quite independent of metaphysical and epistemological considerations. It is altogether indifferent to the present system if function and argument be construed as abstractions which are,

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[226] Quine, "Logic of Sequences," pp. 38-9.

> in some philosophical sense, subsequent to the propositions from which they are abstracted, just as it is irrelevant that, from a psychological standpoint, propositions are pretty certainly prior chronologically to functions and sequences. Nor, indeed, are we even concerned with maintaining that propositions are, in any absolute sense, logically subsequent to functions and sequences—mainly, perhaps, because we have little conception of what possible meaning such a statement might have. The point is merely that it has proved convenient in the present system to form our primitives in such a way that, for us, the proposition emerges as complex.[227]

Again, his account reflects his guiding thought that what matters is an account of propositions suitable to their use in mathematics. There is no need for a philosophical account distinct from the mathematical work at hand. Here, Quine sets aside worries over whether the proposition or its components are ontologically prior—an issue which Russell and Frege debated over at some length.[228] Neither the philosophical view that the components are prior nor the perhaps more intuitive view (a fact about our psychology) that the proposition is prior has sway over Quine. The logical system itself resolves this issue. Whether propositions are in some ultimate philosophical sense prior is a question which we can make little sense of divorced from any context. But Quine does think the question worth answering, and we see him doing it from the perspective of his budding naturalism. He does the best he can for the traditional philosophical worry by making sense of it within the new mathematical logic. Then from within the framework of his logical system, both the question and answer have a straightforward and, perhaps more importantly, clear formulation. As a matter of systematic convenience, rather than as a matter of some extra-mathematical metaphysical view, the proposition emerges as complex. As we have been seeing, simplicity and elegance are, for Quine, perfectly respectable guides to our philosophical theorizing.

Aware that much of his account of propositions as sequences looks like mere technical innovation, he becomes more explicit about its philosophical payoffs in the published version of the dissertation, *A System of Logistic*, and addresses explicitly "the philosophical side of

[227] Quine, "Logic of Sequences," p. 39.
[228] For details see Hylton, *Russell, Idealism, and the Emergence of Analytic Philosophy*, pp. 260-3.

logic."[229] One of these issues we saw already in discussing his worries over an intensional account of propositions (above, pp. 22-3). Here, he also briefly contrasts his view with Wittgenstein's idea that propositions are to be identified with what symbolizes them. Quine suggests that the term "sentence" already serves this purpose. He clarifies his view against this explaining that he takes a sentence to denote a sequence, in particular that sort of sequence he identifies as a proposition. He also remarks that his "doctrine of propositions as sequences stands in striking agreement with Whitehead's point of view" that propositions, in contrast to judgments, are non-assertive; only a judgment evaluates a proposition as true or false.

Here, Quine gives the example of a proposition predicating redness of a book, which in itself is non-committal as to whether the book is in fact red. He then remarks of his view of propositions as sequences that

> it presents a definite technical entity fulfilling just the demands which he [Whitehead] makes of a proposition. The proposition predicating redness of this book is for me the sequence
> Redness, this book
> or class of all red things, this book.
> Nothing could be more non-committal, less assertive, than the connexity constitutive of association in a sequence.[230]

The interesting point here is not so much Quine's agreement with Whitehead but rather his disagreement with Russell. In one sense, we have Quine adhering to Russell's view, we saw previously, that in carrying out the logicist reduction all we want is some entity that will satisfy the intended role for that object. Quine finds in sequences a clear and definite entity which successfully plays the role intended for propositions. To this extent, there is no further worry about whether propositions really are sequences. His example here is in striking contrast to Russell. The sequence *redness, this book,* is just the sort of example Russell uses to illustrate difficulties over a proposition's unity after analysis. Such a sequence does not capture whatever

[229] Quine, *System of Logistic*, p. 32.
[230] Ibid., p. 33; my emphasis.

the original unity consisted in, reflected by the sentence "This book is red." The sequence to him is just a list. Quine, however, finds propositions too unclear as a philosophical starting point. Rendering them as sequences, replaces the unclear with the clear, displaying the kind of connexity we want from propositions, at least in so far as they are to do what we want of them within Quine's logical system. This perhaps changes the subject matter for Russell. Propositions are not sequences for him; they are propositions and nothing more or less. But what more do we want from them; what else could they be so long as they maintain the desirable features of propositions while doing away with certain unwanted confusions? For Quine, this is the kind of analysis philosophy engages in. Upon finding some term in our scientific theory (broadly construed) that is insufficiently clear, we attempt to refine the notion so that it continues to satisfy the role for which it was intended while laying aside its troublesome and unnecessary features. Some roughly twenty-five years later in his philosophical masterpiece *Word and Object,* Quine wrote of the Wiener-Kuratowski set-theoretic construction of the ordered pair,

> This construction is paradigmatic of what we are most typically up to when in a philosophical spirit we offer an "analysis" or "explication" of some hitherto inadequately formulated "idea" or expression. We do not claim synonymy. We do not claim to make clear and explicit what the users of the of the unclear expression had unconsciously in mind all along. We do not expose hidden meanings, as the words 'analysis' and 'explication' would suggest; we supply lacks. We fix on the particular functions of the unclear expression that make it worth troubling about, and then devise a substitute, clear and couched in terms to our liking, that fills those functions. Beyond those conditions of partial agreement, dictated by our interests and purposes, any traits of the explicans come under the head of "don't-cares."[231]

It is exactly the sort of reasoning concerning propositions in his earliest logical work that led Quine to the ordered-pair a paradigm of philosophical analysis.

Having made this point about the aims of philosophy for Quine, let us now consider the significance of logicism for him. Recall for Russell, reducing mathematics to logic was part of a complex argument against the Idealists demonstrating that mathematics was a branch of human knowledge that was absolutely true. The success of such an argument depended crucially on the

[231] Quine, *Word and Object*, pp. 258-59.

reduction being to something that is in fact logical, and difficulties over the nature of logic, particularly Russell's inductive justification for his system, and the axiom of reducibility in particular, left this part of logicism hopelessly inadequate to the task. Quine, however, accepts Russell's broadly pragmatic approach to logic and sees no reason at provide any further account of the nature of logic. Logic has no privileged status distinct from other disciplines, such as mathematics or the sciences more broadly construed. What then is the significance of the logicist reduction in his hands?

Given Quine's emphasis on technical concerns, we might think he sees logicism as merely an interesting development within mathematics but one that ultimately has very limited philosophical payoffs. Such a reading does not seem completely unfair. Many mathematicians have contributed to the technical side of logicism with little interest in philosophical concerns. In fact, Russell himself may have lost track of his initial philosophical motivations as he became further and further engulfed in the technical project of *Principia Mathematica*.[232] I have been claiming, though, that Quine's use of the technical reflects the beginnings of his philosophy generally. This emerged particularly clearly in the above discussion of propositions, and I think this is indicative of how he views the significance of logicism generally. Logicism for Quine represents his earliest attempts at clarifying our conceptual scheme, in this case, focusing on its mathematical aspects.

Philosophical motivations appear scant in "The Logic of Sequences", but they are there, at least according to how Quine views philosophy in this period. Since his philosophy is a naturalistic philosophy, it takes place within science broadly construed. Philosophy's role here is largely to clarify and simplify our conceptual scheme, or theory of the world. But this is not a

[232] On this point see Hylton, *Russell, Idealism, and the Emergence of Analytic Philosophy*, p. 325; and his "Logic in Russell's Logicism," op. cit.

task distinct from that of the natural scientist or mathematician in general as he would later remark in *Word and Object*:

> The philosopher's task differs from the others', then, in detail; but in no such drastic way as those suppose who imagine for the philosopher a vantage point outside the conceptual scheme that he takes in charge. There is not such cosmic exile. He cannot study and revise the fundamental conceptual scheme of science and common sense without having some conceptual scheme, whether the same or another not less in need of philosophical scrutiny, in which to work. He can scrutinize and improve the system from within, appealing to coherence and simplicity; but this is the theoretician's method generally.[233]

And this is the sort of task I see emerging from Quine's earliest engagement with Russell's logicism. As we have remarked on several times previously, the paradoxes of set theory had great impact on how Quine conceived of the logicist project. We saw him explaining at the outset of *A System of Logistic* that types fulfilled its purpose in avoiding the contradictions.[234] Type theory reveals no hidden essence of the notion of set; it is adopted purely as a move to avoid the contradictions. With this guiding him, mathematics no longer had any pull as an a priori science, and like the natural sciences, would also rest on various theoretical virtues such as simplicity, coherence, and generality.[235] I should also add, since Quine does not view set theory is not a settled branch of mathematics, it is also like other such sciences exploratory and experimental. All of these aspects we see emerging from his engagement with Russell in both the dissertation and its published form.

We can now understand Quine as developing his general philosophical aims out of his engagement with Russell's logicism—the foundations of mathematics in its infancy as a modern science. Quine's purpose broadly speaking was then to help bring this science to maturity. As he often repeats, the aim in this earliest work is to generalize, and as such also simplify, *Principia Mathematica*, in particular by integrating the theory of relations into the theory of

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233 Quine, *Word and Object*, pp. 275-76.
234 Quine, *System of Logistic*., p. 19.
235 For a full account of Quine's views of theoretical virtues, see W. V. Quine and Joseph Ullian, *The Web of Belief*, 2nd ed., (New York, Random House, 1978), ch. 6; also W. V. Quine, "Posits and Reality," in *Ways of Paradox*, pp. 246-54.

monadic functions. As we saw, this led to greater economy in that theorems could then be proved in general for relations of any n-adicy. But this is only one aspect of this project. What is even more important than certain gains in economy and generality he says is that “notions will be found to exhibit various interesting connections, when generalized in terms of the present system, which they did not so clearly exhibit when confined to the more special scheme of *Principia Mathematica*.”[236] This is a point he will hold to throughout his philosophical career. Writing years later in the essay “Epistemology Naturalized” he remarks that the logicist reduction might have been thought to gain clarity in reducing the obscure to the less obscure, in particular, by reducing the truths of mathematics to the obvious, or potentially obvious truths, of logic. He denies this view, however, since the logic necessary to the reduction must be inclusive of set theory, and set theory itself is a powerful branch of mathematics, certainly more obscure than, say, the truths of arithmetic. He then continues, “Such reduction still enhances clarity, but only because of the interrelations that emerge and not because the end terms of the analysis are clearer than others.”[237] In particular, the reduction shows how the truths of diverse branches of mathematics interrelate by being unified into the single framework of logic and set theory. And it is in this sense that logicism further clarifies our mathematical knowledge.

We have of course seen another way in which Quine engages in logicism as a project of ontological clarification. Whereas Russell assumed an intensional basis for his logic, Quine instead turned to an extensional theory of propositional functions, that is, classes. On the one hand he found an intensional account rather mysterious and likely to force him into metaphysical flights. Perhaps even more importantly, though, he recognized mathematics as only requiring the extensional aspects of *Principia*. Given the unclarity he found in the intensional realm, this was

[236] Quine, “Logic of Sequences,” p. iii.
[237] Quine, “Epistemology Naturalized,” in *Ontological Relativity and Other Essays*, (New York: Columbia University Press, 1969), p. 70.

more than enough reason to simplify the system to an extensional basis alone. This too reflects another constant aspect of his philosophical concerns and the way in which philosophy partakes in the clarification and reorganization of our conceptual scheme from within our current best science. As he would later characterize his philosophical project particularly with reference to its logical underpinnings:

> Philosophy is in large part concerned with the theoretical, non-genetic underpinnings of scientific theory; with what science could get along with, could be reconstructed by means of, as distinct from what science has historically made use of. If certain problems of ontology … which arise in ordinary language, turn out not to arise in science as reconstituted with the help of formal logic, then those philosophical problems have in an important sense been solved: they have been shown not to be implicated in any necessary foundations of science. Such solutions are good to just the extent that (a) philosophy of science is philosophy enough and (b) the refashioned logical underpinnings of science do not engender new philosophical problems of their own.[238]

To the extent that an intensional account of propositions remains mysterious and also had no role to play in logicism conceived from within mathematics; and to the extent which an extensional account does not suffer these defects and does advances clarity, then there is no further philosophical worry to address. This is what mathematics can "get along with" and "be reconstructed by means of."

Finally there is a more strictly logical aspect to Quine's project as he sets out to deduce Russell's system from his own: "In the course of the formal development we shall prove theorems special cases of which answer to all the formal postulates of *Principia Mathematica* and to all the definitions in that work which differ from our own, thus establishing the fact that the formal system of *Principia Mathematica* follows in it entirety from the present system.[239]" This aim of the project is not unlike the one sketched in the previous paragraph, but here the interconnections range across proposed theories. We further our understanding of the various frameworks for foundational work by comparing their relative strengths, seeing which systems

[238] Quine, "Mr. Strawson on Logical Theory," in *The Ways of Paradox and Other Essays*, rev. ed., (Cambridge: Harvard University Press, 1976), p. 151.
[239] Quine, "Logic of Sequences," p. iii.

we can deduce from which. Axiomatization also helps for comparing existence assumptions, what sorts of sets the various systems are committed to and how strong these assumptions are. We also come to see more clearly were the theories are in fact incompatible as opposed to where they are only seemingly so. Again, this is a task Quine holds to throughout his philosophical career. It is, as we shall see in the chapters to come, the culmination of his work in set theory as laid out in *Set Theory and Its Logic*.[240] But it also characterizes his philosophical work generally. This sort of comparative and exploratory undertaking is suited to developing scientific theories generally, and logic is very much at the heart of making such comparison possible. Indeed, the simplifying and clarifying aspects of logic are not to be separated from the aims of science generally as he later explained:

> Each reduction that we make in the variety of constituent constructions needed in building the sentences of science is a simplification in the structure of the inclusive conceptual scheme of science. Each elimination of obscure constructions of notions that we manage to achieve, by paraphrase into more lucid elements, is a clarification of the conceptual scheme of science. The motives that impel scientists to seek ever simpler and clearer theories adequate to the subject matter of their special sciences are motives for simplification and clarifications of the broader framework shared by all the sciences. Here the objective is called philosophical, because of the breadth of the framework concerned; but the motivation is the same. The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished from a quest of ultimate categories, a limning of the most general traits of reality.[241]

Here, Quine is speaking specifically of the role of first-order logic as a canonical notation for the sciences generally. His talk of reduction and clarification characterizes equally well what he sees as the philosophical payoffs for logicism as he conceives it, a contribution to this limning of the most general features of reality. Indeed, as I have been arguing, it is in Russell's reduction of mathematics to logic that we find the roots of Quine's philosophy generally.

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[240] W. V. Quine, *Set Theory and Its Logic*, rev. ed., (Cambridge: Harvard University Press, 1969), see epecially part III.

[241] Quine, *Word and Object*, p. 161.

V. NEW FOUNDATIONS AND THE PHILOSOPHY OF SET THEORY

The previous four chapters have largely been concerned with historical and expositional issues related to set theory, and in particular with the set theory and philosophy of W. V. Quine. This chapter will continue with such themes, but here I intend to bring these previous considerations to bear on some more general issues in the philosophy of set theory. In particular, I will argue against the iterative conception of set, so often taken to be exemplified by Zermelo-Fraenkel set theory (ZF), as the single correct version of set theory. Given the variety of set theories discussed in previous chapters, it may seem surprising that there should be a single correct version of set. Since the late 1960's, however, set theorists have tended to treat the iterative conception of set, as expressed by ZF, with privileged status, as if it captures something of the essence of *set*, if you will. There are various reasons as to why ZF has gained this status, but perhaps foremost among them has been George Boolos's excellent exposition of the iterative conception in his 1971 essay "The Iterative Conception of Set."[242] In this paper Boolos not only makes this conception accessible to philosophers and mathematicians alike, but does it so compellingly that it is hard not to believe that there is something truly significant for the notion of set in the iterative conception.[243]

I, however, will deny that the iterative conception is the only viable notion of set. In fact, this recent favoritism for a single conception of set is, as perhaps already suspected, the anomaly in the history of set theory. As we have seen already, the development of set theory after the

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[242] George Boolos, "The Iterative Conception of Set," in *Logic, Logic, and Logic*, Richard Jeffrey, ed. with Introductions and Afterward by John P. Burgess, (Cambridge, Harvard University Press, 1998), pp. 13-29.

[243] It seems to me that there is something of the last vestige of the a priori in treating ZF in this way. It, in a sense, allows philosophers to hold on to the idea that mathematics and its truths are of a unique and special variety among all the sciences, not subject to the sort of pragmatic theory building constructions that philosophers seem more willing to admit for the traditionally empirical sciences. Indeed, Hao Wang talks of the intrinsic necessity of set theory depending on the iterative model; Hao Wang, "The Concept of Set," in Paul Benacerraf and Hilary Putnam, eds., *Philosophy of Mathematics: Selected Readings*, 2nd ed., (New York: Cambridge University Press, 1983), p. 553.

paradoxes, *as well as before*, has largely been a matter of competing intuitions about sets. Recall, Cantor, though operating only with an informal conception of set, perhaps escaped the paradoxes because his prior theological views barred him from allowing, what he deemed, absolutely infinite sets, among these the universal set, the set of ordinals, and the set of cardinals. Russell, on the other hand, struggled to find a consistent notion of set in part because his own intuitions led him so naturally to the idea of a universal set. Cantor's religious views certainly held no sway for Russell, but he could initially find no principled way to rule out the problematic sets. And finally, Zermelo accepted Cantor's restrictions but on largely pragmatic mathematical grounds. For him, as for many logicians working in this early period of set theory, he could see that this was a mathematically interesting theory, and, as he saw it, the aim of axiomatization was merely to capture enough of this theory to insure its continued interest for mathematical research but to rule out enough so that the theory would not give rise to contradiction. And as we have seen, Russell, too, would ultimately agree, at least to an extent, that this pragmatic criterion was what would guide the further development of set theory.

In this chapter, and the next, I will argue that such pragmatic concerns continue to be the primary factor in developing set theory. Indeed, I will argue that such pragmatic motivations are much more in line with both the historical and contemporary development of set theory, and attempting to restrict set theory to ZFC potentially has the harmful outcome of inhibiting the growth of our mathematical and scientific knowledge. As such, set theories such as Quine's NF cannot and should not be ruled out on grounds that they somehow stray too far from what was originally intended in the notion of set. In fact, what I hope to have brought out already and will continue to bring out here is that the idea of a single intuitive notion of set, especially as the iterative notion, is largely a myth. I do not want to deny that there is some body of theory that we can pick out as set theory. Categorization is certainly a useful practice.[244] But to the extent that we can offer criteria for picking out what is to count as set theory, I will argue that NF, too, meets such criteria and furthers our understanding of sets in general.

I

To begin this chapter let us first turn to George Boolos as perhaps the most important proponent of the iterative conception of set, due, in no small degree, to the clarity of his exposition of this conception.[245] Boolos begins his paper examining Cantor's proposed definitions of a set. Recall, Cantor defined a set as "any collection into a whole … of definite and separate objects … of our intuition or thought," or as "every aggregate of determinate elements which can be united into a whole by some law."[246] While Boolos rightly observes that these gesturings at the definition of set are variously unclear, he also states,

> But it cannot be denied that Cantor's definitions could be used by a person to identify and gain some understanding of the sort of object of which Cantor wished to treat. Moreover, they do suggest—although it must be conceded, only very faintly—two important characteristics of sets: that a set is "determined" by its elements in the sense that sets with exactly the same elements are identical, and that, in a sense, the

[244] As Quine himself remarked in "Necessary Truth," "Boundaries between disciplines are useful for deans and librarians, but let us not overestimate them—the boundaries. When we abstract from them, we see all of science—physics, biology, economics, mathematics, logic, and the rest—as single sprawling system, loosely connected in some portions but disconnected nowhere," in *Ways of Paradox*, p. 76.

[245] Boolos gave voice to a view of sets that had been around in the literature but was not widely known among philosophers or given much prominence among set theorists; see Boolos, "Iterative Conception," p. 16, fn. 3. Joseph Shoenfield sketched the view prior to Boolos, though with less detail, in his *Mathematical Logic*, (Natick: A. K. Peters, 1967), pp. 238-9, and again later in his "Axioms of Set Theory," in Jon Barwise, ed., *Handbook of Mathematical Logic*, (New York: North-Holland, 1977), pp. 323-4. After Boolos, Dana Scott also gave a rather detailed account of the conception in his "Axiomatizing Set Theory, in Thomas Jech, ed., *Axiomatic Set Theory*, vol. II, (Providence: American Mathematical Society, 1974), pp. 207-14. Gödel gives some early expression to this idea already in his 1947 "What is Cantor's Continuum Problem?" in Benacerraf and Putnam, pp. 474-5. Here, he distinguishes the iterative conception from the conception of sets as extensions of predicates claiming that "the perfectly 'naïve' and uncritical working with this concept of set [the iterative] has so far proved entirely self-consistent," (pp. 474-5). This might be interestingly contrasted with his attitude expressed just seven years early where he claims that the axiom of foundation, which says there are no infinitely descending epsilon chains and hence no self-membered sets, is assumed only on pragmatic grounds of simplifying the work at hand. See his *The Consistency of the Axiom of Choice and of the Generalized Continuum Hypothesis with the Axioms of Set Theory*, in eds. Solomon Feferman et al., *Kurt Gödel: Collected Works*, vol. II, (New York: Oxford University Press, 1990), p. 38. As stated in my introduction, it was largely following Boolos's masterly exposition that this conception rose to the fore as *the* conception of set.

[246] Georg Cantor, *Contributions*, p. 85; *Grundlagen*, p. 916, n. 1.

> clarification of which is one of the principal objects of the theory whose rationale we shall give, the elements of a set are "prior to" it.[247]

To illustrate what Cantor might have meant by these hints about what sets are, Boolos first sketches out what has become known as "naïve set theory." We have seen this conception of a set many times previously. This is the idea that sets are determined by predicates, or are the extensions of predicates. Recall, given any monadic predicate, it is either true of an object or false of it. The collection of things of which the predicate is true we call its extension. It is in this sense that a set can be thought of as the extension of a predicate. The idea is very intuitive, and as Boolos notes, seems to make sense of Cantor's remark that sets are collections of definite elements united by some law. But, as we saw in previous chapters, despite its intuitiveness, the naïve conception is also inconsistent as shown by the predicate "is non-self membered," which leads directly to Russell's paradox. Thus, the naïve conception of set must be given up as making sense of Cantor's proposed definitions of set.[248]

"Faced with the inconsistency of naïve set theory," Boolos suggests then,

> one might come to believe that any decision to adopt a system of axioms about set would be *arbitrary* in that no explanation could be given why the particular system adopted had any greater claim to describe what we conceive sets and the membership relation to be like than some other system, perhaps incompatible with the one chosen. One might think that no answer could be given to the question: why adopt *this* particular system rather than that or this other one? One might suppose that any apparently consistent theory of sets would have to be unnatural in some way or fragmentary, and that, if consistent, its consistency would be due to certain provisions that were laid down for the express purpose of avoiding the paradoxes that show naïve set theory inconsistent, but that lack any independent motivation.[249]

I quote this passage at length because it illustrates especially well the emerging debate between proponents of the iterative conception and a more pluralistic and experimental understanding of set theory viewed developing in response to the paradoxes. The picture Boolos sketches here can clearly be identified with certain views of Quine, but I think also with Zermelo and Russell as well, even if to perhaps a lesser extent or less explicitly for the latter two. But despite certain

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[247] Boolos, "Iterative Conception," p. 13.
[248] Ibid., p. 14-6.
[249] Ibid., p. 16; Boolos's italics.

claims from all of them on the apparent ad hoc nature of axiomatizing set theory in light of the paradoxes, I think this is not entirely true. There is certainly a guiding intuition behind various approaches to set theory presented by Zermelo, Russell, and Quine respectively. Namely, they all aim to save as much as possible of the idea that a set is the extension of a concept. Indeed, Quine's approach can be seen as a generalization of what is common to both Zermelo and Russell's set theories, but let us first finish laying out Boolos's position before returning to these issues.

Boolos goes on to remark that this artificiality, however, does not have to be the case as there is another conception of set, the iterative conception, that strikes him as entirely natural.[250] On this conception, much like Russell's type theory, the sets are formed at stages, or levels. Unlike Russell's theory, the stages are cumulative. A set will be any collection formed at a stage. We begin with individuals, again as Russell did, and at stage zero we form all collections of individuals. If no individuals exist, we form only the empty set at this stage. This will give us all subsets of individuals including, of course, the set of all individuals itself. So we get 2^n sets at this stage, where n is the number of individuals and may be any natural number or even infinite. We now repeat this process at stage one, again for any individuals there may be and for all sets formed at stage zero. This process continues for all stages through that natural numbers. This brings us to the stage following stages 1, 2, 3, …, that is, stage omega, where we form all sets of sets of the previous levels including a set of all sets formed at these earlier stages. We then continue as before forming sets in this way through stages omega plus 1, omega plus 2, and so on to omega plus omega (or omega times 2), and then to omega plus omega plus omega (omega times 3) and so on to omega times omega, and so on. On this account, the sets are reformed at each stage later than the stage that it was originally formed at. To simplify matters, we say a set

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[250] Ibid., p. 16.

is formed only once, at the stage at which it was originally formed. He also remarks that ZF generally does not assume individuals, so the sets it quantifies over are the pure sets, the sets formed in the absence of any individuals so beginning with the empty set $\varnothing$, then forming $\{\varnothing\}$, then $\{\varnothing, \{\varnothing\}\}$, and so one through the stages as before.[251]

Boolos next axiomatizes this stage theory with the aim of ultimately showing how the axioms of ZF follow from it. He presents the following nine axioms with variables ‘x’, ‘y’, ‘z’, … ranging over sets; ‘r’, ‘s’, ‘t’ ranging over stages; the predicates of ‘=’ and ‘$\in$’ for equality and membership respectively, as well as ‘E’ for ‘is earlier than’ and ‘F’ for ‘is formed at’:

(I) $(\forall s)\neg sEs$, no stage is earlier than itself;

(II) $(\forall r)(\forall s)(\forall t)((rEs\ .\ sEt) \rightarrow rEt)$, the transitivity of ‘earlier than’;

(III) $(\forall s)(\forall t)(sEt \vee s = t \vee tEs)$, either for any two stages, either one comes before the other or they are equal;

(IV) $(\exists s)(\forall t)(t \neq s \rightarrow sEt)$, there is an earliest stage;

(V) $(\forall s)(\exists t)(sEt\ .\ (\forall r)(rEt \rightarrow (rEs \vee r = s)))$, each stage is followed immediately by a stage;

(VI) $(\exists s)((\exists t)tEs\ .\ (\forall t)(tEs \rightarrow (\exists r)(tEr\ .\ rEs)))$, There is a stage aside from the first stage which does not immediately follow any stage, e.g., the omega stage;

(VII) $(\forall x)(\exists s)(xFs\ .\ (\forall t)(xFt \rightarrow t = s))$, every set is formed at a unique stage;

(VIII) $(\forall x)(\forall y)(\forall s)(\forall t)((y \in x\ .\ xFs\ .\ yFt) \rightarrow tEs)$, members of sets are formed prior to the set itself; and

(IX) $(\forall x)(\forall x)(\forall t)(xFx\ .\ tEs \rightarrow (\exists y)(\exists r)(y \in x\ .\ yFr\ .\ (t = r \vee tEr)$, if a set is formed at a stage, then at or after any earlier stage, at least one of its members has also been formed.

The first five axioms here are meant to govern the stages, while the last four describe at which stages sets and their members are formed. In addition to these axioms, Boolos adds two

[251] Ibid., pp. 18-20.

additional axiom schemas, one for set specification and one for induction with regards to sets and stages. The specification axioms are of the form

> $(\forall s)(\exists y)(\forall x)(x \in y \leftrightarrow (\chi \,.\, (\exists t)(tEs \,.\, xFt)))$, where χ is a formula of our language with no occurrence of ‘y’ free.

These axioms state that for any stage there is a set formed whose members are just those sets formed at earlier stages of which the formula χ is true. The induction axioms are of the form

> $(\forall s)(\forall t)(tEs \rightarrow (\forall x)(xFt \rightarrow \theta)) \rightarrow (\forall x)(xFs \rightarrow \chi)) \rightarrow (\forall s)(\forall x)(xFs \rightarrow \chi)$, where χ is a formula of our language containing no occurrences of ‘t’ and θ is like χ except that it contains free occurrences of ‘t’ wherever θ contained occurrences of ‘s’.

Intuitively, these axioms say that if a stage is covered by a predicate provided that all earlier stages are covered by it, then all stages are covered by that predicate. Boolos defines ‘a stage being covered by a predicate’ as ‘the predicate holding of all sets formed at that stage’.[252] The point of all this axiomatizing is to show next that we can derive the usual axioms of Zermelo set theory (Z) from this account of the stage theory. In this sense, Boolos claims that the iterative conception is a natural account of sets on par with the naïve conception.

Recall that the axioms of Z are the empty set axiom, pairing, union, power set, infinity, separation, and extensionality. Boolos also includes regularity, or foundation, among these axioms. Extensionality, he says, “has a special status.”[253] We will return shortly to each of these axioms in turn. Also, to simplify matters, as is usual, he assumes there are no individuals, only sets. We have seen some version of the Zermelo axioms already in chapter two, but to reiterate, Boolos states them as follows:

(1) Empty Set: $(\exists y)(\forall x)\, \neg x \in y$, there is a set with no members;

(2) Pairing: $(\forall z)(\forall w)(\exists y)(\forall x)(x \in y \leftrightarrow (x = z \vee x = w))$, for any sets z and w, there is a set with z and w as its only members;

[252] Ibid., pp. 20-22.
[253] Ibid., p. 25.

(3) Union: $(\forall z)(\exists y)(\forall x)(x \in y \leftrightarrow (\exists w)(x \in w \,.\, w \in z))$, for any set z, there is a set with just the members of the members of z as its members;

(4) Power Set: $(\forall z)(\exists y)(\forall x)(x \in y \leftrightarrow (\forall x)(w \in x \rightarrow w \in z))$, for any set z, there is a set with just the subsets of z as members;

(5) Infinity: $(\exists y)((\exists x)(x \in y \,.\, (\forall z) \neg z \in x) \,.\, (\forall x)(x \in y \rightarrow (\exists z)(z \in y \,.\, (\forall w)(w \in z \leftrightarrow (w \in x \vee w = x)))))$, calling z a successor of x if the members of z are only the members of x and x itself, there is a set containing an empty set and the successor of any set it contains;

(6) Separation: $(\forall z)(\exists y)(\forall x)(x \in y \leftrightarrow (x \in z \,.\, \varphi))$, where φ is a formula in which 'y' does not occur free. For any set z, there is a set y of just those members of z of which φ holds;

(7) Regularity, or Foundation: $(\exists x)\, \varphi \rightarrow (\exists x)(\varphi \,.\, (\forall y)(y \in x \rightarrow \neg\psi))$, where 'y' does not occur free in φ and ψ is like φ except that it contains an occurrence of 'y' wherever φ has a free occurrence of 'x'. If there is a set x of which φ holds, then there is a set x of which φ holds but containing members y of which this formula does not hold.

Let us just present one such example from Boolos of how the axioms of Z follow from the stage theory. Take, for example, the empty set axiom '$(\exists y)(\forall x)(\neg x \in y)$', which states that there is a set with no members. If we let χ be '$x = x$', this yields '$(\forall s)(\exists y)(\forall x)(x \in y \leftrightarrow (x = x \,.\, (\exists t)(tEs \,.\, xFt)))$', which is a specification axiom stating that for any stage there is a set of all sets formed at some earlier stage. Since there is an earliest stage, there is a stage which has no sets formed before it. Hence, there is a set with no members, and so the empty set axiom follows from the stage theory. The other axioms of Z follow similarly, though replacement and choice do not.[254] Adding these two axioms give us the full set theory ZFC, that is, Zermelo-Fraenkel set theory with the axiom of choice.

II

There is no doubt something very appealing about the view of sets we have just sketched, but in this section, I want to take a more critical attitude towards the iterative conception by

[254] Ibid., pp. 22-6.

looking at the reasons Boolos gives in its favor as well by trying to bring out some of its less intuitive features. Following his remarks on the supposed ad hoc nature of adopting any set theory in light of the paradoxes (quoted above p. 119), Boolos remarks that the iterative conception "often strikes people as entirely natural, free from artificiality, not at all ad hoc, and one they might perhaps have formulated themselves."[255] This may be true, but so far this only makes it a competitor with other conceptions of set. As we saw in chapter two, to Cantor it may have seemed entirely natural, for example, to rule out a universal set given his background metaphysical views, but we saw there that Russell found the opposite view so appealing that he could not see his way to fully accepting his own early proposal of type theory. Even with types in its initial *Principles* formulation, he thought there must be a class of all terms, or more generally, objects, and this would regenerate the paradoxes.[256] Similarly, Zermelo, for whom the set theory Boolos describes is named, put forward his axioms with a largely pragmatic justification aiming to preserve as much as was mathematically interesting from Cantor's original theory.[257] My point here is just to signal that intuitions alone, even the intuitions of set theory's founders themselves, will not settle questions about which set theory we are to accept. And Boolos, I think, ultimately accepts this. Indeed he continues, observing of the iterative conception,

> It is, perhaps, no more natural a conception that the naïve conception, and certainly not quite so simple to describe. On the other hand, it is, as far as we know, consistent: not only are the sets whose existence would lead to contradiction not assumed to exist in the axioms of the theories that express the iterative conception, but the more than fifty years experience that practicing set theorists have had with this

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[255] Ibid., p. 16.

[256] Russell, *Principles*, pp. 366-7. A similar situation arises also for propositions (*Principles*, pp. 527-8). For more detail, see Hylton, *Russell*, pp. 229-31.

[257] Wang calls such approaches to restoring consistency to set theory bankruptcy approaches ("Concept of Set," pp. 541-4). He contrasts this with misunderstanding approaches, which searches for some misunderstanding in our original understanding of a set. He claims Cantor as part of this tradition. I am not so sure. It seems Cantor may have never understood set theory to embody something paradoxical. It is unclear whether he introduced his distinction between consistent and inconsistent totalities in reaction to paradox or whether this distinction was always, at least implicitly, in place.

> conception have yielded a good understanding of what can and what cannot be proved in these theories, and at present there just is no suspicion at all that they are inconsistent.[258]

So in addition to its naturalness, the iterative conception appears consistent. But this seems to hold for any serious alternatives to the iterative conception. Certainly type theory also appears consistent[259] as does NF.[260] And while perhaps neither theory has received the scrutiny of the iterative conception as embodied in ZF, there does exist a vast amount of research on each, similar to the sort of research set theorists have carried out on ZF. So consistency alone does not uniquely pick out the iterative conception from among other set theories, particularly in the experimental form Boolos suggests here—that we think ZF consistent because in working with it, we have yet to find a contradiction. We should also flag another theme here that will continue to arise. Boolos remarks that “the sets whose existence would lead to contradiction [are] not assumed to exist in the axioms of the theories that express the iterative conception,” but this raises another question. Are other sets, perhaps of either mathematical or philosophical interest banned from these theories? The obvious possibility is that that none of them yield a universal set in contrast to NF. This is an issue we will return to shortly in more detail considering NF further more directly.

At this point Boolos himself turns directly to NF (and its extension ML). He remarks first that ZF is the standard first-order theory for expressing the iterative conception (also observing that the subsystem Z also embodies this idea as well as the extended systems Von Neumann-Bernays-Gödel (NBG) and Morse-Kelley (MK)). But he says of theories proposed incompatible with ZF, by which he means NF and ML:

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[258] Ibid., p. 16.

[259] See, for example, Gerhard Gentzen, “The Consistency of the Simple Theory of Types,” in M. E. Szabo, ed., *The Collected Papers of Gerhard Gentzen*, (Amsterdam: North-Holland, 1969), pp. 214-22. Gentzen does not include infinity as one of the axioms of the simple theory of types, though he does include choice.

[260] We saw earlier Rosser’s account that the usual sorts of contradictions do not arise in NF. Thomas Forster has conveyed to me in conversation that even a contradiction in NF would be exciting since it would have to arise in wholly unexpected ways.

> These theories appear to lack a motivation that is independent of the paradoxes in the following sense: they are not, as Russell has written, "such as even the cleverest logician would have thought of if he had not known of the contradictions." A final and satisfying resolution of the set-theoretical paradoxes cannot by embodied in a theory that blocks their derivation by artificial technical restrictions on the set of axioms that are imposed *only because* paradox would otherwise ensue; these other theories survive only through such artificial devices. *ZF alone (together with its extensions and subsystems) is not only a consistent (apparently) but also an independently motivated theory of sets: there is so to speak, a "thought behind it" about the nature of sets which might have been put forth even if, impossibly, naïve set theory had been consistent.* The thought, moreover, can be described in a rough, but informative way without first stating the theory the thought is behind.[261]

At least in part Quine would accept Boolos's point, and so would Zermelo, and even Russell in certain moods. This was the point of Quine's remark that in light of the contradictions "commonsense is bankrupt". Any resolution of the paradoxes will have some degree of unnaturalness about it. But this is not to say that set theories such as types or NF are wholly unmotivated. The development of both types and NF is guided by capturing as much of comprehension in its original form as possible as the basis for set existence. ZF's separation is perhaps just that much further from the original insight of set existence as a set being the extension of a predicate. Furthermore, types has very much the same sort of hierarchical structure that the iterative notion does, and NF takes its guiding thought from types and Russell's use of typical ambiguity in expositing his system. Indeed, we could even understand NF as taking this idea of a hierarchy, found in both type theory and ZF, and abstracting away from any actual layering of the universe. We might view the insight of both types and ZF being not that the universe actually comes in layers but rather that the subscripting of variables can provide a syntactic test for set existence without the extra, perhaps metaphysical, claim that the universe comes in a prearranged hierarchy.[262] Both the idea of sets as the extensions of predicates and the

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[261] Boolos, "Iterative Conception," p. 17; first italics Boolos's, second mine. The Russell quote is from *My Philosophical Development*, op. cit., p. 61.

[262] In 1905 Russell also proposed the zigzag theory as a possible solution to the paradoxes stating, "In the zigzag theory, we start from the suggestion that propositional functions determine classes when they are fairly simple, and only fail to do so when they are complicated and recondite." He could not however find criteria for determining when a propositional function became too complicated. See his "On Some Difficulties in the Theory of Transfinite Numbers and Order Types," in Douglas Lackey, ed., *Essays in Analysis*, (London: George Allen and Unwin, 1973),

ideas of sets being layered in a hierarchy point to ZF not being the only independently motivated set theory with an easily describable thought behind it.

But Boolos thinks the iterative conception brings out a very particular motivating thought that NF does not have. He next explains,

> Whatever tenuous hold on the conceptions of *set* and *member* were given one by Cantor's definitions of "set" and one's ordinary understanding of "element," "set," "collection," etc. is altogether lost if one is to suppose that some sets are members of themselves. The idea is paradoxical not in the sense that it is contradictory to suppose that some set is a member of itself, for, after all, "$(\exists x)(Sx \,.\, x \in x)$" is obviously consistent, but that if one understands "$\in$" as meaning "is a member of," it is very, very peculiar to suppose it true. For when one is told that a set is a collection into a whole of definite elements of our thought, one thinks: Here are some things. Now we bind them up into a whole. We don't suppose that what we come up with after combining some elements into a whole could have been one of the very things we combined (not, at least, if we are combining two or more elements).[263]

NF certainly does allow for self-membered sets, for example, in its universal set given by the instance of the comprehension schema "$(\exists y)(\forall x)(x \in y \leftrightarrow x = x)$." Then V is certainly a member of V because it is self-identical. Now, this may very well sound counterintuitive in thinking about sets along the lines sketched by Boolos. As he presents it here, set theory sounds like a theory of collections of very ordinary physical objects. Here are some things and we collect them together.[264] This sounds fine when we think about, say, rocks or paperclips, but this brings us back again to the issue we have faced before. How intuitive is set theory anyway? On its most intuitive version it was of course inconsistent. But on the gloss Boolos gives it here, it would hardly make sense of abstract collections of numbers or say of sets themselves: "Here are some numbers. Now we bind them up into a whole." But this is only a metaphor, perhaps a

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pp. 145-51. A. A. Fraenkel, Y. Bar-Hillel, and A. Levy remark that Quine's NF perhaps makes sense of Russell's zigzag theory; see their *Foundations of Set Theory*, 2nd rev. ed., (Amsterdam: North-Holland, 1973), **pp.???**. Michael Potter also observes this in his *Set Theory and Its Philosophy*, (New York, Oxford University Press, 2004), p. 54.

[263] Boolos, "Iterative Conception," pp.17-8; Boolos's emphasis.

[264] Wang also talks this way in talking about the collections that make up set theory. He uses the example of two tables in a room and by looking at them, pointing at them, or thinking about them in the right sort of way, we might view them separately or as a unity; see "Conception of Set," p. 531. Parsons, in an attempt to get away from this idealistic view of how sets come to be, instead puts forth an account in terms of potentiality and actuality in his "What is the Iterative Conception of Set?" in Benacerraf and Putnam, pp. 503-29. While perhaps more in line with the Platonist tendencies of most set theorists, it depends upon us making rigorous sense of the controversial modal notions.

useful one, but a metaphor does not make rigorous sense of the sense in which set theory is a mathematical theory of collections. I am at a loss, as I think we should be, when confronted by this situation along the same lines of our rock or paperclip collections. The abstract theory as embodied in the idea of a set as the extension of predicate, however, makes pretty clear in what sense sets are collections of objects. And this idea also seems to make sense of Cantor's remark about combining into a whole by a law. I can collect rocks, paperclips, numbers, and even sets according to whether or not a predicate is true of them. In its most general form, this idea led to contradiction. So intuitions are to be used with great caution. But it does guide us as to how set theory might use the notions of *set* and *membership* and in what sense a set can be a member of itself. What it means for an object to be a member of a set is just for a certain predicate to be true of that object. This may not be our most ordinary understanding of set and membership, but it *is* one of the guiding thoughts that set theory began with.[265]

Boolos's metaphor seems to fail us in other ways as well. What made set theory so important was its ability to make sense of the infinite. This is one of the minimal requirements of what any consistent set theory should do. How would our collecting together an infinite number objects be made sense of according to our ability to bind objects together. Certainly we cannot do this by running around grabbing up an infinite number of objects. Perhaps the metaphor helps if we think about a finite number of objects and then we can always just keeping adding one more (I am not sure that it does, especially as the idea that follows is that of the potential infinite, not the actual infinite as Cantor intended). But even if this does seem to get us further along in understanding infinite collections, it seems to fail yet again when we start talking about uncountable infinities and infinities of every increasing size. None of this is meant to be

[265] I am not suggesting that the extension of a predicate view is the only way we might make sense of set theory. I only urge it as a contender, and one that perhaps fares better when it comes to making sense of collections of objects, abstract or otherwise, in the set-theoretic sense or of self-membership.

definitive against Boolos, but it is to urge strong caution in our appeal to metaphors when discussing set theory. The extent to which we have any reliable intuitions about set theory, to me, seems very limited and can hardly be a deciding factor in determining which set theories are worthy of our investigations as areas of research. Indeed, much of what we think interesting about set theory runs quite contrary to our commonsense view of the world until we begin doing set theory.

Both of these issues raise a further question about the relationship of time to the iterative conception of set.[266] According to the iterative conception, as Boolos describes it, sets are formed at stages. So we have not only the question discussed above of how we collect objects into sets, but also the question of when. It seems counterintuitive to think of set being created at particular points in time, either past or future. Set theorists tend to work on the assumption that all the sets are already available to them; that they do not need to wait for some perhaps very large sets to be created at a future time (and since we can always "keep going" in the iterative hierarchy, it would seem that there are always sets not yet formed). Nor do they wonder at which point in the past a particular set came into being. Both questions sound absurd (though, the question of when a particular set was discovered or that more sets might be discovered in the future are not similarly absurd topics). Wang tries to put such questions aside by claiming that set formation as the iterative conception describes it, is an idealization of human capacities, both in terms of our collecting abilities and time.[267] But we have to ask again, how ideal is this idealization? Again, it seems to rest on intuitions that set theory was supposed to make sense of in the first place. Indeed, as Parsons observes, the kinds of intuitions Wang would need to make

[266] Parsons, among others, have focused on this difficulty for the iterative conception. Trying to cope with this issue led him to his account of set existence in terms of potentiality and actuality. See his "What is the Iterative Conception?"

[267] Wang, "Conception of Set," pp. 531-2.

sense of the iterative conception's idea of set formation, would not only go beyond an intuition of the potential infinite—our ability to always perform a further step—but beyond limitations imposed by space-time structure itself.[268]

But perhaps Boolos admits all this; that a battle among intuitions will not be resolved. As he concludes,

> There does not seem to be any argument that is guaranteed to persuade someone who really does not see the peculiarity of a set's belonging to itself, or to one of its members, etc., that these states of affairs are peculiar. But it is in part the sense of their oddity that has led set-theorists to favor conceptions of set such as the iterative conception, according to which what they find odd does not occur.[269]

Again, we might say that "oddity" is a relative term, and much of what *any* set theorist would say to the uninitiated would sound quite odd indeed!

III

I want to now consider more carefully some of the axioms of Z that both follow and do not follow from the stage theory. I will begin with replacement and extensionality as neither follow from the iterative conception, and this seems quite uncontroversial. As such, however, both raise interesting questions about whether the iterative conception deserves the privileged status it has gained. I will then turn to regularity, or foundation, which Boolos say does follow from the stage theory. This will bring us back to one of the issues considered in the previous section—that it is too strange to say that a collection can be a member of itself. The axiom of choice will be the last of the axioms I wish to examine. It, too, does not follow from the iterative conception, but as this leads to much broader issues concerning NF and set theory more generally, I will postpone discussion of it until chapter six.

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[268] Parsons, "What is the Iterative Conception?" p. 509. NF seems to fare better on such issues of set formation raised in this paragraph and on previous pages since it allows us to say that the sets are already there. NF comprehension principle then allows us to identify which sets exist, but there is no talk of the sets being created by us or anything else.

[269] Ibid., p. 18.

Let us begin with replacement since its absence from the iterative conception will be least controversial. ZF, as Boolos describes it, are the previous axioms of Z plus the replacement axiom schema:

> F is a function $\rightarrow (\forall z)(\exists y)(\forall x)(x \in y \leftrightarrow (\exists w)(w \in z \,.\, F(w) = x))$, if F is a function, then if its domain is a set, so is its image.

Boolos thinks it possible that we could have allowed an extension of the stage theory from which replacement would follow such as all instances of

> If each set is correlated in some way with at least one stage, then for any set z there is a stage s such that for each member w of z, s is later than some stage with which w is correlated.

But while Boolos thinks that this "is an attractive further thought about the interrelation of sets and stages… it does seem to us to be a *further* thought, and not one that can be said to have been meant in the rough description of the iterative conception."[270] For example, it could be that there are exactly ω_1 stages since nothing in the rough account of the stage theory rules this out. In such a case replacement would not hold generally. On the other hand, replacement has some well-known desirable consequences for the theory of sets. For example, it allows us to define a sequence of sets $\{R_\alpha\}$ with which each stage can be identified if we let $R_0 = \varnothing$; $R_{\alpha+1} = R_\alpha \cup$ $P(R_\alpha)$ (the power set of R_α); and $R\lambda = \cup_{\beta<\lambda} R_\beta$, where λ is a limit ordinal. Replacement assures us that R is well-defined, s is a stage if $(\exists\alpha)(s = R_{\alpha)}$, x is formed at s if $x \subseteq s$ and $x \notin s$, and that s is earlier than t if $(\exists\alpha)(\exists\beta)(s = R_\alpha$ and $t = R_\beta$, and $\alpha < \beta)$. Replacement allows us to go beyond translating the axioms of the stage theory into the language of set theory to stronger axioms asserting the existence of stages further out than those suggested by the rough account of the stage theory.[271]

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[270] Ibid., pp. 26-7 (Boolos's italics).
[271] Ibid., p. 27.

Echoing Russell's inductive justification of reducibility (see above, p. 96), Boolos then states,

> Although they are not derived from the iterative conception, the reason for adopting the axioms of replacement is quite simple: they have many desirable consequences and (apparently) no undesirable ones. In addition to theorems about the iterative conception, the consequences of replacement include a satisfactory if not ideal [fn. An ideal theory would decide the continuum hypothesis, at least.] theory of infinite numbers, and a highly desirable result that justifies inductive definitions on well-founded relations.[272]

While this remark is perfectly acceptable to a view of set theory carried out in a pragmatic and experimental spirit, it seems to run somewhat contrary to Boolos's declared preference for the iterative conception over all other versions of set theory.[273] While perhaps intuitive and easy to describe, we see here that certain important mathematical features do not follow from it until we add the replacement axiom.[274] Again, adding the axiom seems perfectly acceptable if our guiding thought in developing set theory is to maintain as much of what is mathematically interesting about it in light of the paradoxes. But if set theory is supposed to be grounded in the iterative conception to the exclusion of all other conceptions of set theory, we seem to be caught in an uncomfortable halfway point. On the one hand we privilege ZF over all other set theories because it follows from the iterative conception and so gives expression to this intuitive notion of set formation. But on the other hand, we see that the iterative conception is not strong enough to capture important mathematical features of set theory. Thus we are left adding axioms in a somewhat ad hoc way so as to restore the power lost in restricting the conception of set to avoid contradiction. I do not mean to suggest here that we should now instead privilege some other

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[272] Ibid., p. 27. We will return shortly this idea of an ideal theory in considering the axiom of choice.

[273] There have of course been much stronger claims made for favoring certain axioms of set theory. Gödel and Wang both talk of axioms "forcing themselves upon us," though it does not seem entirely clear what this forcing consists of. See Kurt Gödel, "What is Cantor's Continuum Problem?" and Wang, "The Conception of Set," both in Benacerraf and Putnam, pp. 484 and 552 respectively.

[274] In particular, the iterative conception, as Boolos describes it so far, without replacement leaves us with a set theory in which some sets do not have a cardinal number of members. For details, see W. D. Hart, *The Evolution of Logic*, (New York: Cambridge University Press, 2010), pp. 75-6. Such a set theory, to use Hart's words, "would be crippled."

version of set theory, say NF, over ZF. I only want to point out that the intuitive picture that was supposed to lead us naturally to prefer ZF only gets us so far mathematically speaking.

Let us next consider the axiom of regularity, or foundation, which Boolos says is among the axioms of Z following from the iterative conception. It most certainly does follow as demonstrated by Boolos's proof sketch. Let φ hold of a set x'. x', then, is formed at some stage which is not covered by $\neg\varphi$ since $\neg\varphi$ is false of x'. By an induction axiom, then, there is an earliest stage s not covered by $\neg\varphi$. Hence, there is an x formed at stage s of which $\neg\varphi$ does not hold and so, of which φ does hold. If y is in x, then y is formed before stage s, and so, the stage at which y is formed is covered by $\neg\varphi$. Hence, $\neg\varphi$ holds of y, and this is what $\neg\psi$ says.[275] What is tendentious about this is his claim that the regularity axiom is in fact an axiom of Z.

Recall from chapter two that Zermelo does not assume this axiom as one of the axioms of his system. Indeed, it was not explicitly proposed as an axiom until von Neumann's work of the mid-twenties, and he assumes it on purely pragmatic grounds of simplifying his set theory in light of the particular proof he is carrying out.[276] Zermelo himself was the first to explicitly adopt foundation as a general axiom of set theory but did not do so until 1930.[277] At least as late as 1973, Fraenkel et al. state it merely as optional among axioms for Z.[278] So while the idea might look intuitive now in light of the iterative conception and in light of the paradoxes, it was oddly enough not an intuition about sets that occurred to anyone at the founding of the theory. Indeed, while it might not be a further thought about the iterative conception, it does seem a further thought about set theory in general.

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[275] Boolos, "Iterative Conception," pp. 24-5.
[276] John von Neumann, "Über eine Widerspruchfreiheitsfrage in der axiomatischen Mengenlehre," in Abraham H. Taub, ed., *John von Neuman: Collected Works*, vol. I, (New York: Pergamon Press, 1961), pp. 494-508.
[277] Ernst Zermelo, "On Boundary Numbers and Domains of Sets: New Investigations in the Foundations of Set Theory," in Ewald, ed., *From Kant to Hilbert*, pp. 1219-33.
[278] Fraenkel et al., *Foundations of Set Theory*, pp. 52-3. A little later in the work, they remark that since foundation "is not essential for mathematics, it cannot be regarded as fundamental by the traditional axiomatic attitude" (p. 89).

So what recommends regularity then? In short, the axiom says there are no self-membered sets or sets with circular membership conditions, i.e., no sets such that $x \in y$ and $y \in x$. Recall that Boolos argued that it is too strange to say that a collection can be a member of itself. But I countered that the metaphor driving this intuition does not hold up. Boolos offers no justification for it other than that it follows from the stage theory he sketches. I do not think this is necessarily unfair in all cases as a justification, but here, it prejudices our choice of set theory. While other set theories also express much of what the ZF axioms do, a set theory that allows for non-well-founded sets would be automatically ruled out by regularity. If what it is to be a set is to be well-founded, then a set theory that includes non-well-founded sets is ruled out from the start. Boolos's claim, then, about ZF alone being an apparently consistent and independently motivated account of sets seems to stand automatically unchallenged. Starting with the iterative conception, as Boolos describes it, drives us to accept ZF (including regularity) and its subsystems and extensions as *the* set theory.[279]

But there are other motivations for accepting regularity as an axiom of set theory. While it seems Mirimanoff first entertained the idea of focusing on well-founded sets in 1917, followed by von Neumann in his 1925 "An Axiomatization of Set Theory," it was Zermelo who fully accepted regularity as an axiom in his 1930 "On Boundary Numbers and Domains of Sets."[280] Neither von Neumann nor Zermelo claimed that the universe of sets was actually restricted to

[279] For another sort of response to Boolos, see Thomas Forster, "The Iterative Conception of Set," *Review of Symbolic Logic*, 1:1 (2008), pp. 97-110. He argues here that the iterative conception is broader than cumulative hierarchy and includes some non-well-founded sets.

[280] Dimitri Mirimanoff, "Les Antinomies de Russell et de Burali-Forti et le Problème Fondamental de la Théorie des Ensembles," and his "Remarques sur la Théorie des Ensembles," both in *L'Enseignement Mathémtique*, 19 (1917), pp. 37-52 and 208-17; John von Neumann, "An Axiomatization of Set Theory," in van Heijenoort, pp. 393-413; Ernst Zermelo, "On Boundary Numbers and Domains of Sets: New Investigations in the Foundations of Set Theory," in Ewald, pp. 1219-33. Details of this history can be found in various locations; see for example Akihiro Kanamori, "Mathematical Development of Set Theory," pp. 26-9; Michael Potter, *Set Theory and Its Philosophy*, (New York: Oxford University Press, 2004), pp. 51-3; or Adam Rieger, "Paradox, ZF, and the Axiom of Foundation," in Peter Clark, Michael Hallett, and D. DeVidi, eds., Vintage Enthusiasms: Essays in Honour of J. L. Bell, (Glasgow ePrints Service: http://eprints.gla.ac.uk/3810, 2008), p. 10.

well-founded sets. Rather they both introduced the axiom with the practical ground of obtaining categoricity results for their axiom systems. Regularity simplified the universe by eliminating non-well-founded sets, but none of them saw well-foundedness as essential to the concept of set. As Zermelo made the point with regard to accepting regularity: "This … axiom, which excludes all 'circular' sets, and all 'sets that contain themselves', and in general all 'groundless' sets, has always been satisfied in all practical applications of set-theory. Thus, for the time being, it presents no essential restriction to the theory."[281]

More recent justifications for regularity rest on similar grounds in that it simplifies the set-theoretic universe particularly with regard to inductive definitions and investigating models of set theory. This attitude is standard in set theory texts.[282] As is well known, all of the other axioms of ZF hold regardless of regularity. Furthermore, all ordinary mathematics takes place within a universe of well-founded sets. So with regard to ease of use as well as perhaps Occam's Razor, regularity may seem quite worthwhile to assume, regardless of whether we think this is inherent to the notion of set. As Jech makes the point in his standard introduction to set theory:

> It should be stressed that, whether or not one accepts the Axiom of Foundation, makes no difference as far as the development of ordinary mathematics in set theory is concerned. Natural numbers, integers, real numbers and functions on them, and even cardinal and ordinal numbers have been defined, and their properties proved in this book, without any use of the Axiom of Foundation. As far as they are concerned, it does not make any difference whether or not there exist any non-well-founded sets. However, the Axiom of Foundation is very useful in investigations of models of set theory….[283]

Kanamori goes somewhat further in his history of set theory, remarking that regularity is in fact what makes set theory its own special branch of mathematics:

> It is nowadays almost banal that Foundation is the one axiom unnecessary for the recasting of mathematics in set-theoretic terms, but the axiom is also the salient feature that distinguishes investigations specific to set theory as an autonomous field of mathematics. Indeed, it can be fairly said that current set theory is at

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[281] Zermelo, "On Boundary Numbers," p. 1220.
[282] See for example Jech, cited below; and Kenneth Kunen, *Set Theory: An Introduction to Independence Proofs*, (New York: North-Holland, 1980), p. 100-1.
[283] Karel Hrbacek and Thomas Jech, *Introduction to Set Theory*, 3rd revised and expanded ed., (New York: Marcel Dekker, Inc., 1999), p. 259.

> base the study of well-foundedness, the Cantorian well-ordering doctrines adapted to the Zermelian generative conception of sets.

Although he is also careful to note that it is "a notable inversion [that] this iterative conception became a heuristic for motivating the axioms of set theory generally." [284]

So there do seem to be some distinctly mathematical reasons for preferring a set theory of well-founded sets as given by regularity. But is this attitude towards the well-founded sets an accurate picture of set-theoretic current set-theoretic research? It seems not. Particularly notable in this context is Aczel's work on non-well-founded sets.[285] Since its publication, interest in non-well-founded set theory has become widespread with important applications in computer science. Philosophers, too, have recognized such set theories to have important application, particularly in modeling ordinary language and in coming to grips with the semantic paradoxes.[286] And many of the current texts on set theory now include sections dedicated to non-well-founded sets.[287] Furthermore, Kanamori himself has noted that set theory itself has now become an autonomous subject of mathematical investigation[288] rather than merely serving as a foundation for all other mathematics. Viewing set theory in this way, it seems we should be interested in all sets, not just the well-founded ones. Should we not also want to broaden our investigations to understanding what the non-well-founded ones are like? So it seems that non-

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[284] Kanamori, "Mathematical Development," pp. 27-8.

[285] Peter Aczel, *Non-Well-Founded Sets*, (Stanford: CSLI Publications, 1988).

[286] The most notable work here is John Barwise and John Etchemendy, *The Liar: An Essay on Truth and Circularity*, (New York: Oxford University Press, 1987). W. D. Hart has suggested that we might use set theory with a universal set to give a better account of the universal quantifier, as this would give a sense in which 'all' really does mean all. See his *The Evolution of Logic*, (New York: Oxford University Press, 2010), pp. 113-4. In a recent collection of essays edited by Agustin Rayo and Gabriel Uzquiano, *Absolute Generality*, (New York: Cambridge University Press, 2006), some mention is made of this, but the idea is quickly dismissed on grounds similar to what Boolos says at the beginning of his paper on the iterative conception. See in particular Øystein Linnebo, "Sets, Properties, and Unrestricted Quantification," pp. 156-7; and Alan Weir, "Is it too Much to Ask, to Ask for Everything?" p. 340.

[287] See for example Jech and Hrbacek, *Introduction to Set Theory*; Yiannis Moschovakis, *Notes on Set Theory*, 2nd ed., (New York: Springer, 2006); and Keith Devlin, *The Joy of Sets: Fundamentals of Contemporary Set Theory*, 2nd ed., (New York: Springer, 1993). Rieger observes that the picture on the cover of Devlin's book is in fact of a non-well-founded set. See his "An Argument for Finsler-Aczel Set Theory," *Mind* 109:434, (2000), p. 241, fn. 1.

[288] Kanamori, "Mathematical Development of Set Theory," p. 1.

well-founded sets do have a place in the mathematics and philosophy surrounding set theory. Indeed, it is perhaps fair to say that out of hand dismissals of such set theories have impeded the acquisition of mathematical knowledge.

I now want to turn to the extensionality axiom and the special epistemological status Boolos says it has, which, like replacement, does not follow from the iterative conception. He observes that if someone were to deny any other axiom of ZF, we would be more inclined to believe the axiom false than if someone were to deny extensionality. The claim "there are distinct sets with the same members" seems so far from any ordinary conception of set that it would justify our believing that the asserter of such a statement must have some non-standard usage of the word 'set', that they are not merely claiming extensionality to be false. "Because of this difference," Boolos concludes, "one might be tempted to call the axiom of extensionality 'analytic,' true by virtue of the meanings of the words contained in it, but not to consider the other axioms analytic."[289] Analyticity is, of course, a controversial notion, and Boolos recognizes this saying that until we have an account of how a sentence can be true by virtue of meaning, we should refrain from classifying extensionality as analytic. Still,

> [i]t seems probable, nevertheless, that whatever justification for accepting the axiom of extensionality there may be, it is more likely to resemble the justification for accepting most of the classical examples of analytic sentences, such as "all bachelors are unmarried" or "siblings have siblings" than is the justification for accepting the other axioms of set theory. That the concepts of *set* and *being a member of* obey the axiom of extensionality is a far more central feature of our use of them than is the fact that they obey any other axiom. A theory that denied, or even failed to affirm, some of the other axioms of ZF might still be called a set theory, albeit a deviant or fragmentary one. But a theory that did not affirm that the objects with which it dealt were identical if they had the same members would only by charity be called a theory of *sets* alone.[290]

So the important point here regardless of analyticity is that one feature we look for in identifying something as a set theory is that its objects should be identical when they have the same members. This seems correct to me. Much of the value gained by sets is that they have a very

[289] Boolos, "Iterative Conception," pp. 27-8.
[290] Ibid., p. 28 (Boolos's italics).

clear identity criterion in extensionality, and without this it seems we are dealing with some other kind of entity. But non-well-founded set theories do share this feature with the iterative conception. So if we pick out extensionality as characterizing sets, then why should we favor the iterative conception, especially as this key axiom does not follow from it? Indeed, we could, according to Boolos, deny any of the other axioms and still end up with a fragmentary or deviant set theory.[291]

This does not seem entirely correct. Throughout its history another axiom has also been cited as characterizing set theory, the comprehension axiom. Now this also does not follow generally from the iterative conception, but this is surely a virtue. In its most general formulation, it should not be part of any set theory since unrestricted comprehension yields the set-theoretic paradoxes. The iterative conception does however yield separation, ZF's restricted version of comprehension, and any set theory will need some such principle to specify which sets exist. Without it we have no sets. This boils set theory down to two identifying characteristics—extensionality and comprehension—and nothing about these two principles suggests the iterative conception. Indeed, they seem more likely to tell against the iterative conception in the following way.

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[291] There is a potential difficulty for non-well-founded sets and the extensionality axiom. Set theory with the regularity axiom assures us that there is a recursive algorithm for deciding identity between sets. While non-well-founded sets do not violate extensionality, this axiom must be somehow strengthened to account for identity between such sets. There are various ways this might be done, but it is not entirely clear that anyone of them is to be preferred. Aczel raises this issue in his *Non-Well-Founded Sets* in considering the various anti-foundation axioms. For a concise exposition of the issue to W. D. Hart, "On Non-well-Founded Sets," *Crítica*, XXIV:72, (1992), pp. 3-20. For a recent account of non-well-founded set identity in terms of games that is fairly analogous to the situation with well-founded sets, see Thomas Forster, "The Iterative Conception of Set," *Review of Symbolic Logic*, 1:1, (2008), pp. 97-110. Quine himself recognized this issue at least as early as 1975 in his "On the Individuation of Attributes," in *Theories and Things*, (Cambridge: Harvard University Press, 1981), pp. 102-3. Here, he observes, "The system of my 'New Foundations' does have ungrounded classes, and so the system of my Mathematical Logic; and it could be argued that for such classes there is no satisfactory individuation. They are identical if their members are identical, and these are identical if their members are identical, and there is no stopping. This, then, is a point in favor of the systems that bar ungrounded classes" (Quine's italics).

The naïve conception of set gave us too much in that it yielded contradictory sets, such as Russell's set of all non-self-membered sets. One fairly commonplace way of judging a successful axiomatization is that it rules out these contradictory sets while doing as little damage as possible to the original theory. In this sense, the iterative conception as expressed by ZF rules out too much.[292] While we do not want contradictory sets, we also had other sets such as the universal set in the original theory specified by the instance of comprehension "$(\exists y)(\forall x)(x \in y \leftrightarrow x = x)$." ZF does away with this as does type theory (at least in its most general version. Recall that type theory does yield a series of quasi-universal sets). NF, however, does not while still apparently ruling out contradictory sets. Indeed, its only two axioms are versions of extensionality and comprehension. This is the same for type theory, but it has perhaps the undesirable complication of dividing up the universe into levels, again perhaps moving beyond the most basic characteristics of sets. ZF's axioms seem perhaps even more ad hoc. Given the weakening of comprehension in the form of separation, many of the other axioms just serve as fixes for restoring the power lost from the original naïve theory. Starting with the iterative conception and moving to ZF does seem to provide some motivation for adopting ZF, but what I am trying to bring out here is that starting from our naïve conception of set and moving back to ZF makes ZF seems quite arbitrary and unmotivated as a set theory, at least with regard to capturing the original notion of set we began with. I do not take any of these arguments to be definitive, but I do think that they show that any set theory can perhaps be made to look quite arbitrary depending on , what we take as our starting point for thinking about sets. We have seen

[292] Rieger argues similarly in his "Paradox, ZF, and the Axiom of Foundation," p. 18. In a sense, perhaps something like this can be found already in Zermelo's "Investigations", in his comment that in light of the paradoxes "there is at this point nothing left for us to do but to proceed in the opposite direction and, starting from set theory as it is historically given, to seek out the principles required for establishing the foundations of this mathematical discipline. In solving the problem we must, on the one hand, restrict these principles sufficiently to exclude all contradictions and, on the other, take them sufficiently wide to retain all that is valuable in this theory," (p. 200).

this play out in a variety of ways, particularly in investigating the historical development of set theory.

This still leaves us to discuss the axiom of choice, but I will postpone this until the next chapter as it will lead us to much more general considerations of NF. For now I will just sum up our considerations so far of Boolos's account of the iterative conception of set. The general point I have tried to make throughout this chapter is that while other set theories may seem arbitrary, there is something quite arbitrary in ZF as well. And we have seen this both in the assumption of replacement as an extra postulate as well as in ZF ruling out certain seemingly legitimate, and perhaps quite interesting, sets, namely the non-well-founded sets. Let us turn now to the axiom of choice, which will lead us to consider more generally NF and Quine's views on set theory as a whole.

VI. MORE ON NEW FOUNDATIONS AND THE PHILOSOPHY OF SET THEORY

In this final chapter, we consider Quine's NF more generally in comparison to other set theories and conclude with his views on the philosophy of set theory as a whole. Recall from the previous chapter that Boolos claimed that ZF is the only apparently consistent and independently motivated set theory. I have been arguing against this claim trying to bring out the ways in which it is far less intuitive than Boolos presents it and the ways in which NF and other set theories are in fact also independently motivated. Indeed, throughout this dissertation I have been urging that the development of set theory has been largely a battle of competing intuitions over what sets are like. This may be worrisome in that perhaps it leaves us with no objective way of determining what should count as a viable set theory. What we call set theory could just be a matter of personal preference. Should we allow just any theory of collections count as a set theory? This situation is not so bleak however. There do seem to be some criteria readily available for assessing whether we count some body of theory as set theory.

We have already seen some of this in our historical recounting of the development of set theory in that the concept of a set began with some idea of sets being the extensions of predicates (though initially Cantor says little about what sets are). While I take all set theories to have this origin, and the history seems to play out this way, this in itself is perhaps a controversial claim. Many philosophers and logicians have stated that there are two separate notions of a set, the logical and the mathematical, where the former takes sets to be the extensions of predicates and the latter takes them to be formed according to the iterative conception.[293] Having touched upon this debate elsewhere I will not say any more here other than to reiterate that I do not think the distinction holds up very well, and that it is mostly a distinction that encourages a privileging of

[293] Gödel, for example, draws this distinction in his "What is Cantor's Continuum Problem?" pp. 474-5.

the iterative conception. The usual argument is that the logical conception of set has its home in philosophy and that is where the paradoxes arose. Within mathematics, the iterative conception was always present and so mathematicians were never troubled by the paradoxes or the notion of a set more generally. The history, however, shows the iterative conception and this distinction emerging only after the paradoxes (see Chapter II especially). I will put this issue aside now.

W. D. Hart has suggested another set of criteria that serves to distinguish set theory.[294] First, set theory must deal with the paradoxes in some way. Second, it should contain a mathematically interesting account of the infinite as this was one of the aspects that drove the development of set theory in the first place. As such it should preserve at least some version of Cantor's theorem. And third, set theory should be capable of serving as a framework for classical mathematics. This seems a reasonable, minimal set of criteria for identifying a theory of collections as set theory in the way the tradition intended it, though we might still revise them in light of further development in set theory. We have already seen that NF succeeds on the first two criteria. The primary focus of this chapter will be to show that it succeeds also on the third. To begin this discussion I pick up with Boolos's considerations of the axiom of choice in relation to the iterative conception.

I

Boolos introduces choice in the form often called "the multiplicative axiom,"[295] which states

> For any x, if x is a set of nonempty disjoint sets, then there is a set, that is, a choice set for x, that contains exactly one member of each of the members of x.

[294] In conversation, but Quine himself suggests something like this in his "Foundations of Mathematics," in *Ways of Paradox*, pp. 31-2.
[295] W. D. Hart has pointed out to me that the name comes from Russell as he used this version of the axiom to show infinite products non-empty. For details see Russell, *Introduction to Mathematical Philosophy*, p. 122.

The axiom is crucial for much of ordinary mathematics, but as Boolos rightly notes, neither choice nor its negation follow from the iterative conception. Much like replacement, he suggests we might extend the stage theory so that it does decide choice (no doubt preferably that it be true). “But,” he remarks,

> it seems that no additional axiom, which would decide choice, can be inferred from the rough description without the assumption of the axiom of choice itself, or of some equally uncertain principle, in the inference. The difficulty with the axiom of choice is that the decision whether to regard the rough description as implying a principle about sets and stages from which the axiom could be derived is a difficult decision, because essentially the same decision, as the decision whether to accept the axiom.[296]

For example, we might try to extend the stage theory by adding the following principle:

> Let x be a set of non-empty disjoint sets, so x is formed at some stage s. Then the members of x must be formed at some stage earlier than s. Therefore, at stage s or earlier, there exists a set that contains exactly one member of each member of x.

But such a principle, Boolos explains, begs the question since we have no reason to think that such a set is in fact formed. It is the axiom of choice itself that allows us to chose exactly one member from each member of x, that is, to form a choice set for x.[297] He concludes then, “To say this is not to say that they axiom of choice is not both obvious and indispensable. It is only to say that the justification for its acceptance is not to be found in the iterative conception of set.”[298]

So, again, much like the axiom of replacement, a crucial mathematical principal does not follow from the iterative conception of set. This is again not to say that we should reject ZF as a framework for mathematics, but rather to say that we can only get so far motivating it by way of the iterative conception. The situation is the same for type theory; choice must be assumed as an additional axiom if the system is to be sufficient for ordinary mathematics. If we approach set theory in a pragmatic spirit, such additional postulates should be of minimal concern. Our guide

[296] Boolos, “Iterative Conception,” p. 28.
[297] Ibid., pp. 28-9.
[298] Ibid., p. 29.

to developing set theory is to incorporate enough power to make the system mathematically interesting. But the option of adding choice is not available to NF. In fact, this will be our first serious challenge to adopting NF as a set theory for NF does decide choice, and it says this principle is false. So can NF serve a plausible framework for mathematics? This is the question we now turn to.

There is no doubt that the axiom of choice is an important principle for mathematics, used in a wide variety of results from all areas of the subject.[299] But the axiom itself has had a rather controversial history. Zermelo, in his 1904 well-ordering paper,[300] was the first to put forward the principle as an explicit axiom, which he used to show that every set can be well-ordered.[301] Attacks on the proof immediately followed describing its use as an illegitimate method for mathematics. Zermelo responded with his 1908 proof and his fully developed axiom system for set theory.[302] And while choice finally won out as an accepted method of proof, its status remains somewhat different from other axioms of set theory.

Justification for the axiom generally points to its usefulness and importance in mathematics. Indeed, much of Zermelo's strategy in defending it was to show that a significant amount of already accepted mathematics relies upon it, but as an implicit assumption. Such arguments continue to be common in set theory texts. For example, Jech points to such important results relying on choice as the Hahn-Banach Theorem, Tichonov's Theorem, and the Maximal Ideal Theorem as accounting for the axiom's "universal acceptance".[303] Despite this widespread acceptance, mathematicians and logicians continue to be careful about the use of

[299] For some examples of the mathematics relying on choice see Jech, *Introduction*, pp. 144-53.
[300] Ernst Zermelo, "Proof that Every Set can be Well-Ordered," in van Heijenoort, pp. 139-41.
[301] For a complete history of the axiom and the controversies that followed it, see Gregory H. Moore, *Zermelo's Axiom of Choice: Its Origins, Development, and Influence*, (New York: Springer, 1982).
[302] Indeed, Moore argues that the controversy over choice was the primary motivation for Zermelo's axiomatization. As I stated in Chapter 2, I do not fully agree with this account.
[303] Jech, *Introduction*, p. 153.

choice. In particular, they often distinguish proofs in ZF from those done in ZFC (ZF plus choice) and frequently try to reprove results relying on choice with proofs that do not. Indeed, much of Jech's most notable work focuses on such results.[304]

Aside from such justifications by way of its usefulness, there may also be something that seems rather obvious about the axiom. From a collection of disjoint sets, it allows us to pick a unique representative for each one yielding a choice set for the collection. Jech observes that the axiom is often described by using the analogy of an election where each set in the collection are the candidates for a particular office and the election process gives us a choice function from each of these sets. But he is careful to point out that such an analogy talks only of a finite case and that this gives us no justification for assuming the axiom to hold in infinite cases.[305] Reflecting on the arithmetic of infinite numbers, for example, suggests in a pretty straightforward way that what holds for the finite should give us no reason to think it will hold analogously in the infinite. Indeed, as I have pointed out several times already, we seem to have very few initial correct intuitions about the infinite, and it was only Cantor's discovery of set theory that allowed us to gain a rigorously makes sense of it understanding of it.

It turns out that for the finite case, the axiom is unnecessary since it is provable when dealing only with finite collections of finite sets. The situation is, of course, very different for infinite sets. Indeed, even when an infinite collection contains only finite sets, choice is not generally provable.[306] So choice must be added as an axiom to deal with infinite sets in general. But now we get some a fairly unintuitive result. Banach and Tarski, generalizing a result of Hausdorff, proved, using choice, that a sphere can be decomposed into parts and rearranged

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[304] Thomas J. Jech, *The Axiom of Choice*, (Mineola, NY: Dover, 1973).
[305] Thomas J. Jech, "About the Axiom of Choice," in John Barwise, ed., *Handbook of Mathematical Logic*, (Amsterdam: North Holland, 1977), p. 350.
[306] Ibid., pp. 350-51.

using rigid motions, so as to obtain two spheres each the same size as the original sphere.[307] W. D. Hart makes the point even more dramatically glossing the result as saying that it allows us to decompose a sphere the size of a pea and reassemble it into a sphere the size of the sun.[308] This has become known as "the Banach-Tarski Paradox." The point here is not to claim that choice must then be false in light of such a result, but merely to bring out its lack of grounding in commonsense. As Potter observes, the unintuitive nature of the result depends upon our trying to understand it in terms of our ordinary intuitions about elementary geometry. The argument for this paradoxical result, however, does not rely on any such intuitions but rather depends on a mathematical understanding of transcendental functions.[309]

I have been continually coming back to attempts, such as this, at grounding set theory in ordinary everyday conceptions of, say collections or the infinite. Our understanding of such issues may develop in conjunction with certain initial intuitions about sets, but this is a constant back and forth between the technical development of the subject and our initial intuitions about it. If intuitions alone were the final arbiter for the success of a mathematical theory, I suggest we would throw out much of mathematics as we now know it. Certainly set theory would have ended with Russell's paradox. But I also want to make the point that there is some unfairness when examining set theories other than ZF (or ZFC). Because they violate our ordinary understanding, say, of what it means to be a member of a collection (see chapter IV), they are often treated with disdain and unworthy as research projects. I am urging that such non-standard

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[307] Ibid., pp. 351-2. The original paper is Stefan Banach and Alfred Tarski, "Sur la Décomposition des Ensembles de Points en Parties Respectivement Congruents [1924]," in Alfred Tarski, *Collected Papers*, vol. I, 1921-1934, eds. Steven Givant and Ralph N. McKenzie, (Boston: Birkhäuser, 1986), pp. 121-54. Many authors mention this result to illustrate that choice is not as intuitive as it may seem. There are other such results, such as the existence of unmeasurable sets of reals, but such examples are not nearly as vivid for the non-specialist.
[308] For a modern technical account see Stan Wagon, *The Banach-Tarski Paradox*, (Cambridge: Cambridge University Press, 2005).
[309] Potter, *Set Theory and Its Philosophy*, pp. 275-6.

set theories are only thought non-standard, that is contrary to intuition, because we have become so familiar with the iterative conception as exemplified by ZF.

But to return to our immediate topic—what of the axiom of choice in NF? The situation does not initially look good since in 1953 Ernst Specker proved that choice is refutable in NF.[310] We want to know exactly what Specker proved, though. Will it prohibit much of accepted mathematics? If yes, this seems a serious point against NF as a framework for which mathematics can take place in. If much of accepted mathematics can be developed in NF, however, this can not be used against NF. In fact, we might then have reason to prefer NF over ZF. Choice is independent of the axioms of ZF; we can neither prove it nor refute it in ZF. On this count then, NF seems to be a more ideal theory, as Boolos characterizes an ideal theory, since it decides the axiom of choice whereas ZF does not. We would get both the development of accepted mathematics and an answer to the truth or falsity of choice if we were to accept NF as a working set theory. Let us see then what exactly the situation is.

Specker's refutation of choice in its original form is rather abstract and hard to grasp apart from what appear to be some rather arcane details of NF. To make the proof more accessible, I follow, instead, the account Rosser gives in his review of Specker's result.[311] We will assume the axiom of choice and then proceed by reductio. He begins by observing that the proof depends significantly on many properties of cardinals being invariant when their types are raised uniformly. That is, given a cardinal m, we choose a class α of cardinality m and let T(m) be the cardinality of the singleton subclasses of α, USC(α). T(m) thus gives us the cardinal m, but it is of one type higher. (We have already seen the importance of such type raising

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[310] Specker, Ernst P., "The Axiom of Choice in Quine's New Foundations for Mathematical Logic," *Proceedings of the National Academy of Sciences*, vol. 39, no. 9, (September 1953), pp. 972-975.

[311] J. Barkley Rosser, "Review of Specker's 'The Axiom of Choice in Quine's New Foundations for Mathematical Logic," *Journal of Symbolic Logic*, vol. 19, no. 2 (June 1954), pp. 127-28.

operations in NF in our chapter 3 discussion of Cantor's theorem.) To illustrate such invariance, Rosser gives a few examples:

$m \leq n \equiv T(m) \leq T(n)$;

$T(m + n) = T(m) + T(n)$, when $m + n$, m, and n are all cardinals; and

$T(0) = 0$, $T(1) = 1$, $T(2) = 2$, and so on.

And as a consequence, we see that if $m = q + q + q + k$, where $k = 1, 2$, or 3, then $T(m) = T(q) + T(q) + T(q) + k$. So residues (mod 3) are invariant, the cardinality, in general, is not. In fact, we cannot even prove for all finite m that $T(m) = m$.

Next, to define 2^m so that it has the same type as m, we find an α, if such an α exists, such that the cardinality of $USC(\alpha) = m$. We then let 2^m be the cardinality of all subclasses of α. Now if 2^m exists, then $m < 2^m$ and $T(2^m) = 2^{T(m)}$, another case of invariance. However, 2^m does not exist for all m. Indeed, if $|V|$ is the cardinality of the universe, then 2^m exists if and only if $m \leq T(|V|)$. We also note that $2^{T(|V|)} = |V|$, $2^{T(T(|V|))} = T(|V|)$, and so on. We also define, for any cardinal m, $\Phi(m)$ as the set (it is stratified) of m and as many of 2^m, $2^{2\wedge m}$ … as exist.[312] So if they all exist so that $\Phi(m)$ is infinite, then $\Phi(T(m))$ consists of $T(m)$, $T(2^m)$, $T(2^{2\wedge m})$, … and is also infinite. The interesting case is when $\Phi(m)$ is finite. For example, $2^{|V|}$ does not exist, so $\Phi(|V|)$ contains only $|V|$.

Now, by choice we can show that if $\Phi(m)$ is finite, then $\Phi(T(m))$ is also finite, and also that the cardinalities of $\Phi(m)$ and $\Phi(T(m))$ have different residues (mod 3). Let $\Phi(m)$ have finite cardinality. Then there is a greatest member of $\Phi(m)$, say n. So 2^n cannot exist; otherwise it would be the greatest member of $\Phi(m)$. Thus $T(|V|) < n$, since cardinal comparability follows

[312] I have used the carrot ('^') to indicate a super-superscript.

from choice. Then $T(T(|V|)) < T(n)$, and so $T(|V|) = 2^{T(T(|V|))} \leq 2^{T(n)}$. This gives us two cases to consider.

<u>Case 1</u>: $T(|V|) = 2^{T(n)}$. Then the members of $\Phi(T(m))$ are $T(m), T(2m), \ldots, T(n), 2^{T(n)}, 2^{2\wedge T(n)}$, so intuitively $\Phi(T(m))$ has two more members than $\Phi(m)$. So $\Phi(T(m))$ is finite and its residue (mod 3) differs from that of $\Phi(m)$.

<u>Case 2</u>: $T(|V|) < 2^{T(n)}$. Then $2^{T(n)}$ is the greatest member of $\Phi(T(m))$. So now $\Phi(T(m))$ has intuitively one more member than $\Phi(m)$, and similar to the first case $\Phi(T(m))$ is finite and its residue (mod 3) differs from that of $\Phi(m)$.

Now by choice again, there is a least m for which $\Phi(m)$ is finite, and since $\Phi(T(m))$ is also finite, $m \leq T(m)$. By the definition of $T(m)$, there is a p such that $m = T(p)$ and $p \leq m$. Hence, $p = m$, and so $T(p) = T(m)$, and then finally $m = T(m)$. Therefore, the cardinals of $\Phi(m)$ and $\Phi(T(m))$ have the same residue (mod 3). Contradiction. Thus, the axiom of choice is false in NF. Also, since the generalized continuum hypothesis implies choice, it too must be false.[313]

So where does this leave us then with regard to NF as a framework for ordinary mathematics? It seems in a situation very similar to where we are with ZF. Rosser observes almost immediately in his note that he "has become increasingly convinced that" all ordinary mathematics can be developed within the Cantorian sets, that is, those sets α for they and the set of their singleton subsets have the same cardinality (we have already discussed such sets in chapter 3 with reference to Cantor's theorem). And for such sets, it seems quite reasonable to assume that choice holds. Indeed, Specker's proof only shows it failing for non-Cantorian sets such as the universe.

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[313] Adolf Lindenbaum and Alfred Tarski, "Communication sur les Recherces de la Théorie des Ensembles [1926]," in *Alfred Tarski, Collected Papers*, vol. I, Steven R. Givant and Ralph N. McKenzie, eds., (Boston: Birkhäuser, 1986), pp. 171-204. They sketch their result on pp. 187-8. Waclaw Sierpinski proved it in "L'hypothèse généalisée du continu et l'axiom du choix," *Fundamenta Mathematica* 34 (1947), pp. 1-5.

But as I have already remarked mathematicians are generally careful about their use of choice, not necessarily doubting its truth but keeping track of where it is necessary to a result. And Rosser is no exception to this. Even prior to Specker's proof, when it might have seemed completely reasonable to assume the full axiom of choice, that is, choice for the universe, Rosser limited himself to the denumerable axiom of choice, the existence of a choice function for countable families of non-empty sets.[314] Indeed, denumerable choice suffices for the development of most mathematics including that of the real numbers.[315] As Rosser describes the situation in a post-Specker appendix to *Logic for Mathematicians*, there are an infinite number of choice axioms each of which says we can make λ many choices, where λ is any cardinal. It is only where λ is the cardinality of the universe that Specker's proof holds.[316] Hence, this result does not threaten Rosser's reduction of mathematics to NF.

The failure of choice for NF generally may still seem an oddity, and perhaps a very uncomfortable one. While Zermelo's well-ordering theorem claimed to show that all sets can be well-ordered, the theorem cannot hold in NF since this is equivalent to the axiom of choice. But we should not forget, that Zermelo's set theory rules out certain sets from the start, most noticeably in comparison to NF, the universal set. And the universe is such a set that is non-well-orderable.[317] If our starting point does not allow for such sets, then there are of course no problems for Zermelo's well-ordering principle. But the very question I have been trying to raise throughout this dissertation is what grounds are there for ruling out such sets as the universal set from the start. It seems to have a perfectly clear specifying condition, and it also seems perfectly

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[314] Rosser, *Logic for Mathematicians*, p. 512.
[315] For some discussion and some examples see Potter, *Philosophy of Set Theory*, pp. 161-65.
[316] J. Barkley Rosser, *Logic for Mathematicians*, 2nd ed., (Mineola, NY: Dover Publications, 1978), pp. 540-41.
[317] The universe, however, can be well-ordered in NFU so long as there are enough atoms (urelements) in the universe. For details see Randall Holmes "The Set-Theoretical Program of Quine Succeeded, but Nobody Noticed," *Modern Logic* 4, (1994), pp. 1-47.

reasonable that in the course of set-theoretic research (because our interest is in sets as such) we might want to know what the universe and other big sets are like. Indeed, as we have seen, this was one of Quine's driving motivations in proposing NF in the first place.

As a final point on the axiom of choice, recall Boolos's comment about an ideal theory saying that it should decide the continuum hypothesis, which is independent of the axioms of ZF (and ZFC). Choice is also independent of ZF and has had a controversial history with regard to its truth. Most logicians and mathematicians now accept its but largely on its pragmatic value for mathematics overall—it yields many desirable results and no (or perhaps, not many) undesirable ones. But NF, too, can have choice, just not in the unrestricted form that ZFC has it. This is because ZFC is inherently restricted in banning such sets as the set of cardinals, the set of ordinals, and the universal set itself. Restricted versions of choice, such as the denumerable axiom of choice or choice restricted to Cantorian sets, will work perfectly well in NF and provide for much of ordinary mathematics just as ZFC does. Yet, NF is perhaps more ideal (in Boolos's sense) than ZF with regard to choice. Choice is independent of ZF and so must be added as an additional axiom if ZF is to serve as a framework for mathematics. Indeed, in a sense then, ZF gives us no reason to think that choice is true. It yields many desirable consequences, so it makes sense that we would want it to be true. But wanting and actually being so are two different things. NF actually tells us that choice in its most general form is false and so actually decides a set-theoretic statement that ZF does not. This does not inhibit the development of mathematics in NF since choice holds for Cantorian sets. Indeed, we might think of this failure as telling us something further about *all* the sets there are. In ZF style set theory, we build in the assumption that all sets are Cantorian. With NF we explore the set-theoretic universe as a whole without prejudice towards particular kinds of sets. In the failure of choice, we then gain some insight into the general properties of non-Cantorian sets and what distinguishes them from the Cantorian ones.[318] Furthermore, as a consequence of disproving choice, we also gain information about the generalized continuum hypothesis (GHC). A result of Tarski and Lindenbaum shows that GHC proves choice, so GHC is also false in NF. So in Boolos's sense, it seems NF may be a more ideal theory than ZF (or ZFC) in deciding questions such as these.

That the failure of choice in NF does not inhibit the development of ordinary mathematics in this theory is reassuring, but this may not be the only worry to have. As is well known, developing mathematics set-theoretically generally proceeds by first showing that set theory captures arithmetic. The other number systems of integers, rationals, and reals are then built up from this basis. So we also want to be certain that NF can provide for arithmetic, and here, further difficulties lie. Since NF's comprehension schema is restricted by stratification, certain inductive definitions used in arithmetic will be problematic.

For NF, sets do not exist when their specifying conditions are unstratified, and so this will come into play with induction as well—we will only have induction for stratified conditions. So for example, we might expect that we should be able to prove for each natural number n, that it counts the set of its predecessors. That is, we might expect to prove by induction $(\forall n)(n \in \mathbf{N}$

[318] Forster in his "The Status of the Axiom of Choice in Set Theory with a Universal Set," *Journal of Symbolic Logic* 50:3 (1985), pp. 701-7 tries to sketch out some general characteristics of where and why AC fails for set theories with a universal set. There are of course theories related to ZF for which AC fails, e.g. ZF + Determinacy. It seems to me that this may be one particularly good reason for taking research on NF seriously—perhaps we will see trends across set theories that will help us to see connections rather than differences, in a sense to come to a better understanding of a unified body of knowledge called "set theory" rather than ZF style set theory (which is usually taken to be set theory simpliciter) and its deviant relatives such as NF. This seems to be perfectly in line with a Quinean approach to set theory as a comparative endeavor investigating whatever sets there might be, rather than just our favorite ones. In *Set Theory and Its Logic*, Quine himself describes the failure of choice not as a mark against NF but rather as "just another case, and a beauty, of unconventional behavior on the part of non-Cantorian classes…. One could look upon NF as merely more general, in this respect, than set theories where everything in Cantorian," (p. 296).

$\supset \{m: m \in \mathbf{N} \,.\, 0 < m \leq n\} \in n$, but we see here that this condition is not stratified since n occurs on both sides of the membership relation.

To resolve such issues, Rosser suggested the axiom of counting which states for any natural number n, n counts the number of its predecessors, $(\forall n \in \mathbf{N})\{m: 0 < m \leq n\} \in n$. Equivalent to the axiom is the type raising operation T for ordinals (analogous to T for cardinals) is the identity map when applied to natural numbers, that is, for all natural numbers n, $T\{n\} = n$, or the order-type of the relational unit subsets of n is equal to n. This further yields that all finite sets are Cantorian, that all finite sets are strongly Cantorian (i.e., the singleton function is a set), and that there is an infinite strongly Cantorian set. Indeed, these claims are equivalent under the axiom of counting.[319] The counting axiom also helps to preserve a variety of more ordinary mathematical results such as those theorems of number theory concerning the number of integers having a particular property. And it simplifies the natural numbers generally in assuring us that for any set $\alpha \in n$, α has n members.[320]

But let us not forget that counting is an additional axiom not decided by the two original axioms of NF, extensionality and comprehension. We might try to justify it on its rather intuitive sounding nature, though I have urged that we be wary of such justifications in the past, for example, with regard to choice. And indeed, much like choice, counting is a surprisingly powerful axiom. In fact, NF plus counting (NFC) proves the consistency of NF, so on pain of inconsistency (by way of Gödel), it had better not be the case that counting follows from the two initial axioms. Still, we might say that counting fairs somewhat better than choice in it not yielding any especially counterintuitive results, aside from those already present in NF without counting that is (and these results tend to come from the presence of non-Cantorian or big sets in

[319] For details see Forster, *Set Theory with a Universal Set*, pp. 30-1.
[320] For these and some other examples of counting in ordinary mathematics, see Rosser, *Logic for Mathematicians*, 2nd ed., pp. 485-89.

NF and so are perhaps only counterintuitive because other set theories do not allow us to investigate such sets). Unless we demand some further self-evident grounding for our mathematics, we seem to be no worse off in our choice of axioms than we are for ZF (or ZFC). Indeed, in its economy of axioms, NF recommends itself considerably over ZF (and ZFC). And this is enough for the point I have been arguing—that, so far at least, the view that ZF somehow gives us the only viable account of sethood relies more on prejudice than fact.

II

In this concluding section, I want to now try to give some account of what exactly a Quinean philosophy of set theory looks like.[321] Given what we have seen of its exploratory and experimental nature, it cannot be to bring forth something like the essence of sethood as a sort of a priori science. Quine is open-minded about competing set theories, as I take it he is, in some sense, about about all competing scientific theories. This is not to say that almost anything might count as set theory for him. Indeed, the point of the previous section was to show, according to some generally uncontroversial criteria, that Quine's NF could be reasonably reckoned within the discipline of set theory. So given his approach to set theory, I want to turn to the question of what a Quinean philosophy of set theory is if not to bring forth the essence of sethood, for example, in terms of an iterative hierarchy.

The culmination of Quine's work in set theory came in his 1963 *Set Theory and Its Logic*, a work, I claim, that stands along side *Word and Object* as one of his greatest philosophical achievements. The book is dedicated to Russell as initiating Quine's interest in the subject and

[321] Joseph Ullian, in his overview of Quine's work in logic and its relevance to his philosophy has influenced my understanding of Quine's logical work greatly not only in what follows immediately but throughout this dissertation. See his "Quine and the Field of Mathematical Logic," in Edwin Hahn and Paul Schilpp, eds., *The Philosophy of W. V. Quine*, expanded ed., (Chicago: Open Court Publishing, 1998), pp. 569-89.

has the epigraph from the Pirates of Penzance, "How quaint the ways of paradox," stressing yet again the central importance Quine sees in the paradoxes as giving rise to the development of set theory. His major theme is a comparative study of the main approaches to set theory. In the first two parts of the book, he develops a set theory, which he intends as largely neutral between the usual systems of Russell, Zermelo, and himself. He describes his policy as one of minimizing set existence assumptions making them only where the development of the theory demands them.[322] To this end he employs the apparatus of virtual classes, simulating talk of classes by way of contextual definitions rather than assuming their existence outright from the start. This gives him a useful contrast later when he introduces actual classes, better demonstrating "what power real classes confer that the counterfeits do not."[323] He adopts axioms strong enough only to imply the existence of finite sets. Where more substantial claims enter in, such as the existence of infinite sets or the axiom of choice, he adopts these only as hypothetical claims at the outset of proving the theorem requiring these added assumptions.[324] It is only after introducing his neutral and largely minimal set theory that he thinks we are ready in the third part of the book to consider the relative advantages and disadvantages of the various more familiar and substantial set theories. His aim is to prepare us to consider the relative merits of the usual set theories from a perspective which is prejudiced towards none of them.[325]

He also returns again to the theme that "intuition is bankrupt" when it comes to developing a viable system of sets, for intuition brought set theory in its earliest days to contradiction. This is not to say that intuitions never come into play or are never helpful in set

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[322] Quine, *Set Theory and Its Logic*, p. vii.
[323] Ibid., p. ix.
[324] For example, see Quine's discussion of this approach in *Set Theory and Its Logic*, p. xi.
[325] As he remarks, "Because the axiomatic systems of set theory in the literature are largely incompatible with one another and no one of them clearly deserves to be singled out as standard, it seems prudent to teach a panorama of alternatives" (*Set Theory and Its Logic*, p. x).

theory, but we need to be cautious as Quine explains (I put forth a similar argument in the previous chapter but here we have Quine himself making it):

> The notion of class is so fundamental to thought that we cannot hope to define it in more fundamental terms. We can say that a class is any aggregate, any collection, any combination of objects of any sort; if this helps, well and good. But even this will be less help than hindrance unless we keep clearly in mind that the aggregating or collecting or combining here is to connote no actual displacement of the objects, and further that the aggregation or collection or combination of say seven pairs of shoes is not to be identified with the aggregation or collection or combination of those fourteen shoes, nor with that of the twenty-eight soles and uppers. In short, a class may be thought of as an aggregate or collection or combination of objects just so long as 'aggregate' or 'collection' or 'combination' is understood strictly in the sense of 'class'.[326]

This is not to say we have no way of knowing what a class is. Quine does think there is some use for coming to understand classes by way of an analogy to the ordinary collections of physical objects we come in contact with every day. But to understand collections *as classes*, he turns to predicates. If we begin with a sentence about a thing, we can then think of removing reference to that thing from the sentence leaving us only with the predicate, or an open sentence, true of some things and false of others. The class, then, as we are now familiar with, is exactly those things of which the predicate is true, the extension of the predicate. He also notes that we want classes to be identical when they have the same members, so we also adopt extensionality as our principle of class identity. But he continues to be careful in appealing to intuitions; his gesturing at what a class is as distinct from ordinary collections is not meant as definitive:

> I was describing the function of the notion of class, not defining class. The description is incomplete in that a class is not meant to require, for its existence, that there be an open sentence to determine it. Of course, if we can specify the class at all, we can write an open sentence that determines it…. But the catch is that there is in the notion of class no presumption that each class is specifiable. In fact there is an implicit presumption to the contrary, if we accept the classical body of theory that comes down from Cantor.[327]

Maintaining Cantor's theory of the infinite is not the only reason Quine cites for not adhering too closely to the idea of classes as the extensions of predicates. Because this "natural attitude", as he describes it, led to contradiction, we must instead make deliberate and careful choices about our class existence axioms since "intuition is not in general to be trusted here." But since we

[326] Quine, *Set Theory and Its Logic*, p. 1.
[327] Ibid., p. 2.

have a variety of interesting alternative axiomatizations available, he thinks it hasty to focus on just one of them "to the point of retraining our intuition to it."[328] In the contemporary philosophy of set theory, this is of course exactly what has happened. Being a viable set theory has become nearly inseparable from the intuitions guiding the iterative conception of set.

On Quine's view, then, seeming artificiality is not a reason to reject a particular set theory. Yet, he also does not rule out a particular set theory as eventually being adopted as best for he thinks his own approach—a careful weighing of the benefits and drawbacks of the various options—"can encourage research that may some day issue in a set theory that is clearly best."[329] As we might expect, his attitude here is much like the attitude one could have with regard to the progress of science generally. Various theories are developed until finally one emerges as best, however temporary this privileged position might happen to be.

This careful comparative development of set theory with regard to its existence assumptions and axiomatic strength is, for Quine, the philosophy of set theory. This should be no surprise to those familiar with his approach to the philosophy of the sciences generally. Neither in the natural sciences nor in mathematics do we look for a first philosophy. Our starting point is within science itself, so in the case of set theory, its philosophy begins within set theory rather than in some extra-scientific metaphysical conception that it must be fitted to.

Having now cleared his readers' minds of set-theoretic prejudice in the first two parts of *Set Theory and its Logic*, he turns in part three to consider the familiar set theories of Russell, Zermelo, and himself. Rather than draw distinctions and build boundaries between them, Quine takes the very radical approach of bringing these various and often incompatible set theories into

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[328] Ibid., p. 5.
[329] Ibid., pp. x-xi.

discussion with each other. So let us turn to the details of Quine's picture and how it might further the growth and knowledge of set theory.

He begins his study with Russell's theory of types, progressing next to Zermelo's theory, and finally to his own NF. This account is not historical nor is it intended to be. Rather we can view his account as a sort of logical progression through the various set theories he considers showing how each one can be seen, in a sense, as emerging from the other. We have seen both Boolos and Russell, among others,[330] argue against Quine's NF by remarking on its seemingly artificial development as only a response to the paradoxes, that there is no motivating thought behind it other than this. Quine of course largely accepts this, but sees it as the situation generally in set theory after the paradoxes. To him it is not a criticism of NF any more than it is a criticism of the other set theories he considers. The paradoxes forced such artificiality upon all of us. As we have heard over and over again from him, our most intuitive thought about sets, that is, as extensions of predicates, led us into contradiction. And we have seen this play out in the early history of set theory. Talk of sets existing in some sort of hierarchy entered in not as an intuitive first thought about sets but rather emerged from considerations of paradoxes, first in Russell and sometime later in Zermelo (much later than his first axiomatization). Artificial development, then, is unavoidable.

Perhaps critics of NF would grant Quine's point that all set theory is in part artificial, but for them, NF is just a little too artificial as opposed to types or Zermelo set theory. Quine's comparative account of set theory can be read as a response to just this sort of criticism. Aside from comparing the relative merits of the various set theories and how their respective disadvantages may lead one to a different axiomatization, he—perhaps even more importantly—

[330] I should include here Donald A. Martin's "Review of *Set Theory and Its Logic*," *Journal of Philosophy* 67:4 (1970), pp. 111-4. See also Quine's reply in *Journal of Philosophy* 67:8 (1970), pp. 247-8.

brings out what unifies them. To this end he begins with types and shows how its hierarchical structure leads to Zermelo and then finally to his own NF. What Quine does is take this talk of hierarchy and abstract away from this metaphor as much as possible until he is left with its barest logical structure, present across the boundaries of the various competing and incompatible axiomatizations of set theory. This comparison accounts for certain intuitions about set theory while also moving against our becoming too attached to certain intuitions we might have about set theory. The final section of *Set Theory and Its Logic*, then, is a crucial move in an argument against those who would focus on just one formulation of set theory "to the point of retraining our intuition to it."[331]

Let us see now how this argument goes. We discussed already in Chapter 2 how Russell saw his theory of types as emerging from Poincaré's diagnosis that the contradictions all have there origins in quantifying over illegitimate totalities, what Russell took to be summed up in his vicious circle principle. Recall that in its simplified version, the theory of types divides the universe into levels—individuals at the lowest level, then classes of individuals, then classes of classes of individuals and so on. For something to be a member of a class, it must be of level n while the class of which it is a member is of level n + 1. This dividing of the universe into levels, motivated by Russell's vicious circle principle, then apparently restored consistency to the theory of classes. But Quine observes that this move is drastic in that its type restrictions tamper with the original logic. Furthermore, (as we saw also in chapters 3 and 4) type theory has the undesirable feature of reduplicating objects in other levels of the hierarchy, among them the empty class, a series of quasi-universal classes, and the various objects of mathematics such as the different number systems.

[331] Quine, *Set Theory and Its Logic*, p. 5.

We might take these drawbacks alone to move us beyond types to some other approach to set theory, but here, Quine observes that there is also something valuable in the constructive metaphor of type theory—"it is a part of set theory that carries extra conviction, because of the construction metaphor…."[332] Quine then moves to Zermelo set theory with its unrestricted (with regard to type) variables not merely as a reaction to the undesirable features of types but rather as a natural generalization of types' hierarchical structure. Whereas other philosophers of set theory have ruled out types as set theory, often by fiat, Quine engages with both theories to demonstrate how we might see the interconnections between them.[333]

To show how Zermelo's theory emerges from Russell's, Quine first observes that typical ambiguity in types does not change a many-sorted theory into one with general variables. The point of typical ambiguity is that although we often do not explicitly specify the types involved for each formula (for ease of writing and reading), we must still be certain that they can be made explicit in the appropriate way if called upon to do so. So for example the typically ambiguous formula "$(\exists y)(\forall x)(x \in y)$" can be shown legitimate by restoring type indexes according to the scheme "$(\exists y^{n+1})(\forall x^{n})(x^{n} \in y^{n+1})$". Quine observes, however, that we may also have reasons to consider the translation of the many-sorted types into a theory of general variables. For instance, the type indices are themselves cumbersome, but typical ambiguity can make things worse in some cases. Consider the two formulas "$x \in y$" and "$y \in x$". Both are meaningful under types, and yet, "$x \in y \,.\, y \in x$" is not. In this way we see that types blocks the *set-theoretic* contradictions with the drastic move of revising general logic. Quine, favoring the maxim of

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[332] Ibid., p. 264.
[333] The standard move is to declare types part of logic, that is, higher-order logic, and so not of set theory. Quine remarks on this in *Set Theory and Its Logic*, pp. 257-58, as well as in many other places in his writings. Boolos makes this sort of declaration in his 1975 "On Second-Order Logic," in *Logic, Logic, and Logic*, p. 42, fn. 9 in discussing Quine's presentation of a class theory in the third edition of his *Methods of Logic*, (Chicago: Holt Rinehart, and Winston, 1972).

minimum mutilation, thinks this is to be avoided where possible. Furthermore, given his comparative interest in set theory (and scientific theories generally), a single underlying logic makes such studies far more feasible.

Quine then argues, however, that the many-sorted logic is not essential to type theory. Instead, we can allow the variables to be completely general and add a predicate T_n to impose the type restrictions. Formulas can then be restricted in the familiar way as follows: "$(\forall x)(T_n x \supset Fx)$" or "$(\exists x)(T_n x \,.\, Fx)$". This move, he says, makes sense of Russell's grammatical restrictions.[334] Whereas Russell declared all formulas of the form "$x^m \in y^n$" with $n \neq m + 1$ meaningless, we can now take such formulas to be simply false. So the grammatical restrictions generated by types appear unnecessary in the first place. The two axiom schemas of types go over into the new theory as follows: comprehension and extensionality become respectively "$(\exists y)(T_{n+1}y \,.\, (\forall x)(T_n x \supset x \in y \equiv Fx)$" and "$(T_{n+1}x \,.\, T_{n+1}y \,.\, (\forall w)(T_n w \supset (w \in x \equiv w \in y))) \supset x = y$" (officially identity is defined in terms of membership and first-order logic and so is eliminable).

Having shown the eliminability of typed variables in favor of typing predicates, Quine goes on to show how smoothly types can lead us into Zermelo's set theory by converting the restricted variables of types into cumulative types with general variables.[335] We begin by equating the empty sets of all the various types, so as to get a single empty set. Quine then

[334] This was one of Quine's earliest motivations for his attempts at reworking Russell's logic. See his remarks on his 1936 "Set-theoretic Foundations for Logic," in his "On the Inception of N. F." both in *Selected Logic Papers*, pp. 83-99 and 286-7 respectively.

[335] The details can be found in *Set Theory and Its Logic*, pp. 272-4. Here, I hope to give only the basic idea behind the conversion to cumulative types and Zermelo set theory. This was an idea that Quine was already onto much earlier, which he laid out in his "Unification of Universes in Set Theory," *Journal of Symbolic Logic*, vol. 21:3, (1956), pp. 267-79. It is perhaps worth noting that Quine here sees types and Zermelo as sharing more with each other than with NF and though he states a preference for his own theory, he also observes that "in the present paper I am pursuing other lines in order to dramatize the process of unifying the universes of theories containing multiple styles of variables" (p. 275). Here we see the distinct approach Quine takes to set theory. While so much of the philosophy of set theory concerns singling out a privileged theory, Quine instead looks for where they might be of a piece with one another.

introduces the trick of identifying individuals, the memberless objects of type 0,[336] with their unit classes and treating membership between individuals as identity. So the objects of type 0, the T_0x's—the individuals—can now be defined by the formula "$(\forall y)(y \in x \equiv y = x)$", and the objects of higher type, the $T_{n+1}x$'s—the classes—by the formula "$(\forall y)(y \in x \supset T_n y)$". So any typed variable can be re-written according to these definitions so that we use only completely general variables, and the types become cumulative, that is, $T_m x \supset T_n x$ for all $m < n$.

Here, we see just the sort of philosophy of set theory Quine engages in. Rather than looking at differences and attempting to privilege one set theory over another, he tries to see their similarities. For Quine, coming to understand the realm of sets means investigating sets from the various perspectives different set theories allow for. His endeavor in set theory, like in much of science, is cooperative rather than exclusionary and aims to broaden our knowledge through an open-mindedness about set theory. This moving from non-cumulative to cumulative types allows us perhaps to better understand the apparent hierarchical structure of sets. It was always present in both types and Zermelo set theory, and Quine has now shown us explicitly how the idea connects between the two approaches to set theory. Quine himself found this connection immensely striking. He later observed of Zermelo's theory that "in its multiplicity of axioms it seemed inelegant, artificial, and ad hoc. I had not yet appreciated how naturally his system emerges from the theory of types when we render the types cumulative and describe them by means of general variables." So while many have remarked on the artificiality of Quine's systems, Quine initially thought the same of Zermelo's. The sort of elegance, simplicity, and unity Quine (as well as many other philosophers of science) has so often put forth as desirable

[336] Individuals are necessary to types in accounting for the infinite. The axiom of infinity for types says that there are an infinite number of individuals.

for scientific theories generally[337] comes now also to Zermelo's theory by way of its emergence from the perhaps more intuitive theory of types. There are, of course, a variety of ways we might consider types to be intuitive. Here, I have in mind (as I suspect Quine does too) that types, at least initially seemed to do the least damage to the original conception of sets as extensional and specified by a comprehension principle of a sort. In linking these theories, this sort of intuitiveness of types passes on to Zermelo. Indeed, Quine concludes that had he appreciated this link earlier, "I might not have pressed on to 'New Foundations.'"[338]

Bringing out such connections now also gives us another way to see NF as sharing an important aspect with types and Zermelo set theory. The move from types to Zermelo perhaps gives us further reason to think the idea of a hierarchy somehow essential to what a set is. But as we saw in the previous chapter, it is notoriously difficult to make philosophical sense of this idea. We have on the one hand talk of sets coming into being at particular levels of the hierarchy, but then on the other, talk of sets existing eternally from the past and to the future. With NF, Quine perhaps gives a way of making sense of this idea of hierarchy while also freeing it from a particular metaphysical account of sets in the following way. We can think of NF as abstracting away completely from the idea of a hierarchy until we are left with a much weaker purely syntactic account of set existence. As Dreben has put it—and Ullian following him—Quine's approach is one of syntactic exploration.[339] The hierarchy gives us only the numbering scheme used as NF's syntactic test for set existence, an idea which Quine came upon by taking typical ambiguity very seriously, which appears to have no need for an actual hierarchy of sets.[340] We need not then make the further leap to, say, the iterative hierarchy to give us an account of the

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[337] See for example Quine's "Posits and Reality," in *Ways of Paradox*, pp. 247-8.
[338] Quine, "Inception of 'N. F.'," p. 287.
[339] Ullian, "Quine and the Field of Mathematical Logic," pp. 584 and 588, n. 56
[340] Quine, *Set Theory and Its Logic*, pp. 287-89; and see also his remarks in "The Inception of 'N. F.'," p. 244 where he discusses how he arrived at NF by considering Russell's types in conjunction with Zermelo's theory.

essence of sethood. This would be an extra, more robust philosophical assumption. Quine's syntactic account, instead, keeps our feet on the ground.[341] In doing the philosophy of set theory, Quine tries to keep to his naturalistic strictures, and for him we must always remember that "philosophy of science is philosophy enough."[342]

[341] He makes this remark in his *Methods of Logic*, 4th edition, (Cambridge: Harvard University Press, 1982), p. 212 when considering the substitutional versus set-theoretic account of logical truth. The point seems apt here as well—we try to get by with fewer assumptions where possible.

[342] W. V. Quine, "Mr. Strawson on Logical Theory," in *Ways of Paradox*, p. 151.

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