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# Toward a Kripkean Concept of Number

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TOWARD A KRIPKEAN CONCEPT OF NUMBER

by

Oliver R. Marshall

A dissertation submitted to the Graduate Faculty in Philosophy in partial fulfillment of the requirements  
of the degree of Doctor of Philosophy, The City University of New York.  
2016.



This manuscript has been read and accepted for the Graduate Faculty in Philosophy to satisfy the dissertation requirement for the degree of Doctor of Philosophy.

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## Abstract

## TOWARD A KRIPKEAN CONCEPT OF NUMBER

By

Oliver R. Marshall

Saul Kripke once remarked to me that natural numbers cannot be posits inferred from their indispensability to science, since we've always had them. This left me wondering whether numbers are objects of Russellian acquaintance, or accessible by analysis, being implied by known general principles about how to reason correctly, or both. To answer this question, I discuss some recent (and not so recent) work on our concepts of number and of particular numbers, by leading psychologists and philosophers. Special attention is paid to Kripke's theory that numbers possess structural features of the numerical systems that stand for them, and to the relation between his proposal about numbers and his doctrine that there are contingent truths known *a priori*. My own proposal, to which Kripke is sympathetic, is that numbers are properties of sets. I argue for this by showing the extent to which it can avoid the problems that plague the various views under discussion, including the problems raised by Kripke against Frege. I also argue that while the terms 'the number of F's', 'natural number' and '0', '1', '2' etc. are *partially* understood by the folk, they can only be *fully* understood by reflection and analysis, including reflection on how to reason correctly. In this last respect my thesis is a retreat position from logicism. I also show how it dovetails with an account of how numbers are actually grasped in practice, via numerical systems, and in virtue of a certain structural affinity between a geometric pattern that we grasp intuitively, and our fully analyzed concepts of numbers. I argue that none of this involves acquaintance with numbers.

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## Introduction

Saul Kripke once remarked to me that natural numbers cannot be posits inferred from their indispensability to science, since we've always had them. This left me wondering whether numbers are objects of Russellian acquaintance, or grasped by conceptual analysis (being implied by known general principles such as those of logic or set theory), or both, or indeed neither, perhaps being grasped by an understanding of numerical concepts that is not achieved by analysis. The question is pressing, since a full account of human knowledge must include an account of mathematical knowledge, and here we should begin with numbers, since these are a prerequisite of other mathematics. Happily, my interest in this topic has coincided with a resurgence of work in the area, with leading psychologists and philosophers, including Kripke himself, staking out positions corresponding to the aforementioned options. Given this resurgence, a dissertation on the topic seems overdue.

I begin, in chapter 1, by discussing recent attempts to explain how we actually grasp numbers, using the resources of cognitive science. First I consider the influential view that we are acquainted with numbers via the innate sense of quantity known in the psychological literature as our "number sense." Having rejected this proposal, I turn to attempts by Tyler Burge and others to use it—as well as other proposals from cognitive science—as part of an account that purports to explain our grasp of numbers in terms of our acquisition of numerical concepts. In essence, my criticism is that such accounts of acquisition are either insufficient to explain our grasp of numbers, or presuppose too much about what they purport to explain, with the result that they are uninformative. I'll now say a little more about this, so that the reader can appreciate the dialectic that unfolds in the following chapters.

According to Burge, numbers are not implied by our knowledge of known general principles, since propositions about numbers are “underived from general principles” (2000: 40). Further, while Burge claims that such propositions are “irreducibly singular” (ibid), he also claims that this is *not* because numbers are grasped by acquaintance, but because they are grasped by an immediate, non-discursive and elementary kind of understanding that he calls “comprehension.” I’ll now say a bit about comprehension of propositions about the smallest natural numbers.

According to Burge, learning to count with numerals gives us the ability to deploy the corresponding numerical concepts, in the context of applied arithmetical propositions like there are 2 houses of Congress. Further, we can immediately perceptually apply these concepts to small concrete pluralities without counting (perhaps using our number sense). Furthermore, once we can do all this, it helps us to immediately assign these numerical concepts to numerals in the context of *other* applied statements of number, of the form ‘there are  $m$  F’s.’ This is comprehension. Moreover, given this comprehension *and* the ability to calculate, we can then assign the correct content to unapplied arithmetical statements like ‘ $2 + 2 = 4$ ’ immediately without calculation.

Crucial to all this is that we understand a numerical system, which according to Burge requires the ability to count, which he glosses as putting the objects counted in one-to-one correspondence with *numbers*. But it is also his view that that we cannot represent numbers prior to acquiring concepts of them. So if this account is to avoid circularity, it must assume that we can *already* represent numbers via propositions containing *non-numeral-like* concepts, and can use the latter in our counting experience, through which we come to understand propositions

containing numeral-like concepts. But in the present context this is a significant assumption about what is to be explained, one that needs spelling out.

One way to break this circularity would be to remove reference to numbers from the requirements for counting, by stating them as follows. Firstly, the words in the count list must be recited in a stable-order. Secondly, a one-to-one correspondence must be established between the words in the count list and the objects counted. Thirdly, one must be able to give the final word of the count in answer to the question ‘how many F’s?’ Then the problem is that it is possible to meet all of these requirements without grasping the cardinal significance of counting or grasping which cardinals are denoted by the members of the count list. To show this, I describe a stage during development when children can recite a short list of numerals in a stable order, put them in one-to-one correspondence with the F’s, and recite the last numeral in the count when asked ‘how many F’s?’ And yet, when instructed to give the experimenter  $m$  F’s—where  $m$  is the last numeral recited—they give the experimenter a random number of F’s.

This problem about concept acquisition does not arise for logicism, since according to it numbers are already available to us prior to counting, being deducible from general principles and concepts that are already understood. In a little more detail, the idea is that because we can understand count nouns, and can understand “one-to-one correspondence,” and can reason with higher-order logic, we understand of sentences of the form “the  $F$ ’s are in one-to-one correspondence with the  $G$ ’s.” From this, using the resources of higher-order logic, we can, in principle, deduce a version of the so-called “Frege-Russell numbers.” In my view, while it is through counting that we are first *taught* about finite cardinal numbers, an initial segment is already in principle accessible to us, by the deductive route just described.

Now we can say that while the infants can establish a one-to-one correspondence between numerals and objects, they do not understand the conceptual connection between counting, equinumerosity and cardinal numbers, and in particular do not understand that the last numeral of the transitive count —with which they answer “how many”— expresses a Frege-Russell number. This is why, when instructed, after counting, to ‘Give me  $m$  F’s’ —where  $m$  is the last numeral used in the transitive count— they give the experimenter a random number of F’s. Were they to understand that ‘ $m$ ’ expresses the relevant Frege-Russell number, and that the set they have counted thereby has that number, then they would be able to give the experimenter the requisite number of F’s.

Since I find the aforementioned feature of logicism very attractive, in chapter 2 I begin my survey of its various incarnations, with the aim of finding a relatively plausible retreat position from this discredited doctrine. However, since logicism is not traditionally concerned with how we actually grasp numbers, one of the main challenges faced by one advocating any of its incarnations, is to establish its relevance to arithmetic as this is actually practiced. To this end, I begin with Frege’s project of deriving the correspondents of the axioms of arithmetic from (allegedly) logical general principles, using logical definitions of the arithmetical primitives. I argue —against Patricia Blanchette— that the relevance of Frege’s project to actual arithmetic is supposed to be established by the synonymy of the axioms of arithmetic with Frege’s derived correspondents. Further, I show that synonymy is supposed to be achieved because Frege’s definitions of the arithmetical primitives are intended to express our actual arithmetical concepts; for example, his definition of “the number belonging to the concept  $F$ ” is intended to express the way that numbers are used in practice, by competent arithmeticians, who, despite their competence, may not have engaged in sufficient reflection to realize what exactly numbers are.

Although I find a lot to agree with in Frege's work, I conclude that his analysis of the arithmetical primitives cannot be correct. The most interesting reason is that the aforementioned definition of number is subject to a modal objection, which reflects Kripkean developments in semantics that are, to my knowledge, usually ignored in the philosophy of mathematics.

The dialectic of chapter 3 is too complex to admit of an informative summary. Suffice to say that Richard Heck has recently attempted to establish the relevance of the *neo-logicist* project—of deriving correspondents of the axioms of arithmetic from the Hume-Cantor Principle—to actual arithmetic, by attempting to show the relevance of this project to our actual concept of number. I argue that Heck's attempt fails, in part via an argument that leads me to the topic of set theory.

Chapter 4 is concerned with Kripke's proposal to establish the relevance of set-theoretic logicism to actual arithmetic, by representing our actual concept of number in set theory. A key ingredient of this proposal is Kripke's claim that one should in mathematics, whenever possible, use a notation that is "structurally revelatory" – one that has a structural affinity with the subject matter it represents. Partly under Kripke's influence, I conjecture that numbers are first grasped intuitively, by visualizing *something like* what psychologists call "the number line." In my view this is the accumulation of discrete units in a direction, and is a structurally revelatory representation of a progression of Frege-Russell numbers.

Kripke points out that any such Frege-Russell analysis of number will make our familiar decimal notation highly structurally *unrevelatory*, because decimal notation is not cumulative. Rather, decimal multi-digit numerals are finite sequences of one or more of the digits '0' – '9', ordered by length and then lexicographically. To ensure that decimal notation is structurally revelatory, Kripke amends Benacerraf's famous proposal, so that the numbers are *any*



*progression of finite sequences consisting of one or more of the ten objects referred to by '0' – '9', where these sequences are ordered by length and then lexicographically, and where sequences of two or more starting with 0 are excluded.*

Kripke argues that this proposal is a plausible analysis of our concept of number. This is because those of us who are trained in the decimal system learn to impose the aforementioned structure on the numbers. That is, we learn to parse, or identify and individuate numbers as finite sequences that make decimal notation structurally revelatory. Kripke then uses this claim to explain why decimal numerals acquaint us with numbers – in his parlance, why it is “immediately revelatory.” The explanation is that our identification of numbers as finite sequences provides a standard for knowing which number we are confronted with, while decimal numerals present numbers as such sequences.

Although I accept neither the above explanandum —that decimal numerals acquaint us with numbers— nor Kripke’s proposed explanation of it, my own proposal to explain our special facility with our preferred decimal notation is indebted to Kripke’s. This is one reason why I call my counter-proposal “Kripkean.” I start with the fact that decimal notation is structured to be read and visualized, which I claim helps us to overcome the limitations of our parsing ability. My proposal is then that a notation should be *visually revelatory*: it should reveal structural features of its subject matter visually, by helping one to see or visualize them. There may be some tension between the demands of having a visually revelatory notation and the demands of having a structurally revelatory one, with the result that a trade-off between the two is required. For example, decimal notation is visually revelatory, because we can visualize the decimal numerals in order, and this reveals, visually, the ordering of a progression of numbers. Thus decimal notation is somewhat structurally revelatory. But as a result of also being visually revelatory, and

so structured to be read and visualized, it is not as structurally revelatory as it might otherwise be. For example, it is not as structurally revelatory as stroke notation, which is not structured to be read and visualized. Thus there are grounds for insisting that decimal notation has structure that is not shared by the Frege-Russell numbers, despite Kripke's reason for saying otherwise. Finally, my proposal can also explain why decimal notation seems immediately revelatory. This is because in addition to understanding numerals, *we can also parse them with little conscious effort.*

Chapter 5 concerns the relation between Kripke's proposal about numbers and his controversial doctrine that there are contingent truths known *a priori*. This discussion contains a lengthy digression on the topic of context-sensitive semantics, the morals of which are applied again in the following chapter, to the topic of count nouns. This discussion is inspired by Kripke's criticisms of certain applications of contextualism. I also propose to explain our special facility with our preferred measurement system in a way analogous to the above proposal about decimal numerals.

In chapter 6 I offer a proposal about what the Frege-Russell numbers are, to which Kripke is sympathetic: that numbers are properties of sets. I show how this proposal can be developed into a system of definitions of the arithmetical primitives, against the logical background of the simple theory of types. I also argue for the proposal by showing the extent to which it can avoid the problems that plague the various other views under discussion, including the problems raised by Kripke against Frege. Finally, I argue that the general principles of the theory of types, from which the axioms of arithmetic are derived, should be accepted as the cost of deriving what are arguably synonyms of the axioms of arithmetic, because the definitions of the arithmetical primitives express our actual arithmetical concepts. The result is that the axioms

are justified not only by the theorems that follow from them, but also by *how* these theorems are derived.

In the course of all this, I also show how the above retreat position from logicism dovetails nicely with an account of how numbers are actually grasped in practice: that is, via numerical systems, geometric intuition, and a partial understanding of what numbers are according to my analysis. To summarize the answer to my original question, numbers are not objects of Russellian acquaintance. Rather, numbers are in principle accessible by analysis, and in practice grasped by a visually revelatory notation, as well as by visual intuition of a number line that is structurally revelatory of the Frege-Russell numbers.

## Chapter 1: Our intuitive grasp of number

### 1. Introduction

Some branches of mathematics concern *a given subject matter*: a subject matter of which most of us have an intuitive grasp, prior to receiving mathematical training. For example the axioms of geometry in Euclid's *Elements* concern space, or at least how space appears to us. This is a subject matter about which we have a stock of intuitions. The axioms of a ring, by contrast, do not concern a given subject matter, but instead concern the algebraic structures that they define. Unlike geometry, arithmetic was not originally developed from axioms. However, it too concerns a given subject matter, namely the natural numbers, so there is a question regarding how we are able to grasp these intuitively.

The topic of this chapter is the view that I call “cognitive scientism.” This is the view that cognitive science can explain our intuitive grasp of numbers and capacity for arithmetical thought more generally. This view comes in various forms. In its more radical form, cognitive-scientism purports to tell *the whole story* about how we come to know arithmetical truths, and so supplant the work on these issues that is done in the philosophy of mathematics. In its less radical form, cognitive-scientism purports to be distinct from but related to the philosophy of mathematics in ways that will become clear, and in particular to be necessary for answering the question of how we actually acquire mathematical beliefs.<sup>1</sup>

I begin with radical cognitive-scientism. In sections (2) – (6) I consider, as a stand-alone proposal, the theory that we grasp numbers using the innate perceptual faculty known as our “number sense” (Dehaene, 1997). Then, in subsequent sections, I turn to various attempts to use this proposal, as well as others from cognitive science, as ingredients in more philosophical

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<sup>1</sup> Burge (2007, 2009, 2010), Giaquinto (2001a, b, 2007).

accounts of our grasp of numbers. Here I begin by discussing Russell's doctrine of acquaintance, before criticizing Marcus Giaquinto's attempt to use the number sense hypothesis to explain how we are acquainted with small numbers. Next I turn to Tyler Burge's criticisms of the doctrine of acquaintance, and his attempt to explain our intuitive grasp of numbers in terms of an epistemically immediate kind of understanding he calls "comprehension." Then I return to Giaquinto and his theory of our intuitive grasp of the number *structure*. Finally, I conclude by drawing a methodological moral about how to proceed in the coming chapters. A recurring theme is that the authors under discussion are vulnerable to objections rather like Frege's objections to psychologism, because they focus on the theory of mental representations rather than on the representational properties of language.

## 2. The number sense hypothesis

Most of us experience the phenomena of being able to estimate, visually, that there are between twenty and forty people in the room, and of being able to look at much smaller pluralities, such as three cows in a field, and see how many there are, apparently without counting. Further to these reflections, there are many disparate empirical studies in support of the hypothesis that humans, including pre-linguistic infants and people with a reduced numerical lexicon, have the ability to sense the cardinal size of pluralities. More specifically, it is hypothesized that even prior to learning numerical concepts we are able to:

- (a) Perceptually estimate the cardinal size of a given plurality, and perceptually discriminate different pluralities in terms of approximations of their cardinal size.

- (b) Perceive the exact cardinal size of pluralities of up to three or four members at a much faster rate than that required by discursive counting, an ability known as “subitizing.”

These abilities are claimed to be innate, since, as I will explain, they are also found in animals. That we possess these abilities has come to be widely accepted in the psychological literature, in no small part due to the work of Dehaene, who refers to them jointly as our “number sense” (ibid). According to Dehaene, this innate sense is what constitutes our ability to think about numbers intuitively. It is this rather than our ability to count that is supposed to explain how numbers first entered human culture.<sup>2</sup>

Before this claim can be assessed, it is necessary to distinguish between *counting* — which requires putting objects in one-to-one correspondence with discrete symbols or numbers— and *summation*, which requires only the accumulation of a continuous variable such as a physical magnitude.<sup>3</sup> For example, an egg timer does not count minutes discretely, but simply accumulates a quantity of sand. Likewise, the pedometer in an iPod does not literally count your steps, but accumulates a physical magnitude in response to hip movement. The reason that this distinction is important is that Dehaene hypothesizes that the number sense is an *analog* system that represents numbers —despite the fact that numbers are discrete— by summation, using what he calls “a continuous quantitative internal representation” (1997: 220). I will now describe some of the evidence for the claim that the number sense is analog. First I will describe some evidence that exists for the hypothesis that the estimative abilities of animals are analog. Then I will describe some evidence that our corresponding abilities are analog too. Doing this will require

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<sup>2</sup> Dehaene (1997: 102).

<sup>3</sup> Franks et al. (2006).

describing some experiments and experimental paradigms, so I ask for the reader's patience in this regard.

Desert ants are hypothesized to navigate their environment using path integration, keeping track of their changing position in space. Doing so requires them to reliably correlate information they possess with direction, and, more importantly for our purposes, with distance. It is hypothesized that the information they correlate with distance is accumulated using a pedometer. This is suggested by an experiment in which the stride length of ants is manipulated between their journeys from and to their nest. Ants that take longer strides, because their legs are lengthened with stilts before their return journey, overestimate the distance of their return journey in proportion to the change in their stride length. Further, ants that take shorter strides, because their legs are shortened before their return journey, underestimate the distance of their return journey in proportion to the change in their stride length. This can be explained by the fact that the ants accumulate and navigate with information that correlates with the number of steps taken on their outward journey, explaining why they take the same number of steps on their return journey, thus the pedometer hypothesis.<sup>4</sup> Unsurprisingly, the ants appear to do this by analog summation rather than counting,<sup>5</sup> most likely by using stress receptors in their joints to accumulate a continuous physical magnitude that varies in proportion to the number of steps they take.<sup>6</sup>

To give another example, rats can learn to press a lever repeatedly before pressing a second lever to get a reward. Having learned to do this, they soon learn to respond with roughly the required number of presses on the first lever, before pressing the second and searching for the

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<sup>4</sup> Wittlinger, Wehner and Wolf (2006).

<sup>5</sup> Franks et al. (ibid), Wittlinger, Wehner and Wolf (ibid).

<sup>6</sup> Thanks to Haim Gaifman for introducing me to this experiment.

reward. The accuracy of their estimative capacities can then be measured by the probability of search after the wrong number of presses (the confounding quantity of duration having been controlled for). For each number of presses required by the experimenter, the mean of the distribution of the rat's responses is slightly higher than is required. Further, the standard deviation around the mean increases as a constant ratio of the mean, from which it follows that greater magnitudes must differ more than smaller ones in order for the rat to discriminate them. This accords with Weber's law,<sup>7</sup> which is that the discriminability of any two magnitudes is a function of their ratio, i.e. that the ratio of the minimum change (required to discriminate two magnitudes) to the initial magnitude is constant. Weber's law applies to representations of continuous variables such as length, area, loudness, and so conformity to it is evidence of analog summation rather than discrete counting.

Further experiments show that rats can also accumulate information concerning magnitude while ignoring other confounding properties of the stimuli in question. For example, they learn to press one lever in response to two flashes and another lever in response to four, before learning to press the first lever in response to two sounds and the second in response to four. Surprisingly, when presented with a flash synchronized with a sound, they press the lever corresponding to 2, and when presented with two flashes synchronized with two sounds they press the lever corresponding to 4. This suggests that they learn to associate different levers with different magnitudes, rather than with different perceptual modalities. There is also evidence that in addition to ants and rats, birds, honeybees and cicadas can accumulate a variable that reliably correlates with the cardinal size of a given plurality rather than with its other properties.<sup>8</sup>

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<sup>7</sup> Platt & Johnson (1971).

<sup>8</sup> Butterworth (1999), Burge (2010), Carey (2009), Dehaene (ibid), Giaquinto (2001a, b, 2007).





first pair of magnitudes than it does the second:

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Dehaene reports that human performance on number comparison tasks is subject to the distance and magnitude effects. As regards the former, it takes longer for adult humans to distinguish pluralities of 15 from pluralities of 10 than it does for them to distinguish pluralities of 15 from pluralities of 3. As regards the magnitude effect, it takes longer to distinguish pluralities of 15 from pluralities of 10 than it does to distinguish pluralities of 10 from pluralities of 5.

Both of these effects are also manifest by adult humans when they are asked to compare pairs of digits rather than pluralities. Furthermore, the distance effect is manifest when subjects are asked to compare pairs of two-digit numerals. For example, it takes longer for adult humans to decide whether 71 is greater than 65 than it does for them to decide whether 79 is greater than 65.<sup>11</sup>

Dehaene also reports that digital mechanisms are not ordinarily subject to these effects. For example, modern computers that represent numbers in binary code are not subject to the distance effect, since it actually takes longer for them to distinguish the pair  $\{8_2, 10_2\}$  than it does for them to distinguish the closer pair  $\{7_2, 8_2\}$ . This is because distinguishing the former pair requires comparing the second to last digit of binary '1000' with that of '1010', while distinguishing the latter pair only requires comparing the first digit of '111' with that of '1000'. Neither are modern computers subject to the magnitude effect, since it takes longer for them to distinguish the pair  $\{6_2, 7_2\}$  than it does for them to distinguish the pair  $\{7_2, 8_2\}$ . This is because distinguishing the former pair requires comparing the last digit of '110' with that of '111', while,

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<sup>11</sup> Dehaene (ibid), Giaquinto (2007).

as we have seen, distinguishing the latter pair only requires comparing the first digit of ‘111’ with that of ‘1000’.

Dehaene argues from this that the brain does not perform these tasks like an ordinary digital computer. Rather, he claims, it should be modeled using some sort of analog accumulator, since such machines are themselves subject to these effects:

The peculiar way in which we compare numbers thus reveals the original principles used by the brain to represent parameters in the environment, such as number. Unlike the computer, it does not rely on digital code, but on a continuous quantitative internal representation. The brain is not a logical machine, but an analog device (1997: 220).

Paraphrasing Gallistel (1991) approvingly, Dehaene continues:

Instead of using number to represent magnitude, the rat [like the *Homo Sapiens!*] uses magnitude to represent number (ibid).

I will return to this claim in due course.

Further evidence that our number sense is still of use is found in studies of clinical patients who lack number sense, as shown by the fact that they can’t look at a plurality of e.g. nine things and say that there are nine of them, without counting. For example, one patient with a good education, and a high IQ but no number sense is unable to acquire normal arithmetical abilities.<sup>12</sup> Furthermore, patients with far more severe brain damage (“CBS” and “PCA”) who also lack number sense but are largely linguistically unimpaired, have great difficulty understanding count nouns and cardinal quantifiers (such as ‘at least 3 cows’) although they

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<sup>12</sup> Butterworth (ibid), Giaquinto (2007).

seem to understand logical quantifiers (such as ‘all’ and ‘some’).<sup>13</sup> Still other patients with very severe brain damage (“CBD”) who lack number sense have difficulty understanding logical quantifiers in addition to cardinal quantifiers.<sup>14</sup> This is consistent with the claim that understanding count nouns and quantifiers requires number sense.

Here I will register the first of many complaints. While these clinical studies are ingenious and suggestive, suitable patients are few and far between, and so the clinical evidence that the aforementioned abilities depend on our number sense is limited. Further, the evidence that can be gleaned from these studies is difficult to interpret, since, despite the best efforts of the authors of this work, some other confounding impairment cannot be ruled out as the explanation for the difficulties faced by these patients. The problem is that while the CBS and PCA sufferers can recite the numerals up to 20 in order and answer questions like ‘are there 3 dots?’ by reciting a numeral for each dot, this can be overlearned and is consistent with a failure to fully understand counting and its cardinal significance.<sup>15</sup> Therefore, the difficulties faced by CBS and PCA sufferers can instead be explained as follows. Understanding count nouns and quantifiers requires understanding one or more of the conditions for counting that these patients fail to understand.

Another proposed reason to think that an innate number sense is still of use to numerate human adults, is that its integration with our culturally acquired abilities to represent numbers precisely can explain why decimal users have a good idea of how many members a given plurality has on being given a decimal numeral, even though they have little or no idea of this on being given a binary numeral. To explain this phenomenon, Giaquinto suggests that there is “a

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<sup>13</sup> Morgan et al. (2011).

<sup>14</sup> McMillan et al. (2006).

<sup>15</sup> See section 13 of this chapter.

strong association of number size representation and decimal numerals” (2007: 92).<sup>16</sup> For example, if a decimal user counts that there are 327 students in the room, he thereby has a good idea of how many students there are because his approximate sense of 327 is associated with the corresponding decimal numeral.

Here I will register another complaint. While the above may be true, it cannot be the whole story, for the following reason. By hypothesis, we are only able to subitize (in the sense of p. 3 (b)) the exact size of pluralities of up to three or four members, and have to figure out the exact number of even slightly larger pluralities by more discursive means. So our approximate sense of 327 could only help facilitate knowledge of *approximately* how many students there are. It could not explain why, when we count that there are three hundred and twenty-seven guests, we thereby know *exactly* how many guests there are.

#### 4. The triple code model

Of course there is no need to claim that *all* arithmetical tasks are performed by the accumulation and mental manipulation of quantities. For example, according to Dehaene’s “triple code model,” numerate adults have two other kinds of mental representations at their disposal. Firstly, they have the mental correspondents of number words stored in a “verbal word frame.” Secondly they have the correspondents of positional numerals stored in “a *visual arabic number form*, in which numbers are represented as strings of digits on an internal visuo-spatial scratchpad” (Dehaene & Cohen, 1995: 85). According to the triple code model, while number comparison and subtraction are performed by manipulating quantities, some arithmetical facts such as multiplications are simply learned by rote and stored in the verbal word frame, and still others are computed using a mixture of such learned facts and the manipulation of quantities.

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<sup>16</sup> This claim is also endorsed by Carey (ibid: 337-338).

Finally, calculations with larger numbers are performed mentally using positional numerals. Nevertheless, according to Dehaene, it is our number sense —with its accumulated analog representation of number— that gives content to these other kinds of representation:

Under the assumptions of the triple-code model, neither the arabic number form nor the verbal word frame contain any semantic information. The meaning of numbers is represented only in the third pole of the model, the *analogical magnitude representation*” (ibid).

I will now say some more about how the analog representation of number is supposed to be embodied, and how it is supposed to be sharpened by these other kinds of representation.

The accumulator is a metaphor for a neural network, in which each object in a perceived plurality is allocated a quantity of neural activity, which is then normalized to an approximately constant quantity in case more activity is initially assigned to larger objects. The normalizations allocated to each object are then summed by what Dehaene calls “accumulation neurons,” and the resulting total is divided by the constant quantity, to yield an estimate of the size of the plurality as output, analogous to the final quantity accumulated. “Detector neurons” are disposed to fire when the estimates they receive are within fixed intervals. They reach a firing peak for the estimates they are “tuned” to, and show decreased firing activity on receipt of estimates that are larger or smaller than the ones they are tuned to, in a way that is normally distributed around the peak.<sup>17</sup>

According to Dehaene, the interval around the peak is the same for almost every detector neuron. The only exceptions are the neurons tuned to one, two and three, which show much smaller intervals, modeling the fact that we can perceive the cardinal size of very small

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<sup>17</sup> Dehaene (ibid: 20-23, 250-251; 1993: 394-395), Dehaene and Changeux (1993). Dehaene reports that there is evidence for the existence of detector neurons in the primate brain.

pluralities with great accuracy. For estimates of above three, the interval around the peak is fixed at about plus or minus 30%, so the range of estimates for which the various detector neurons fire increases with the estimate they are tuned to. For example, a detector neuron tuned to five will fire less frequently on receipt of an estimate of approximately four but not at all for an estimate of approximately one; on the other hand a detector neuron tuned to an estimate of fifty will still fire on receipt of an estimate of approximately forty. So, as the mean of the numbers presented in the comparison task increases, so does the inaccuracy of the corresponding detector neurons as they begin to fire more frequently for estimates they are not tuned to. Thus the inaccuracy of the model in performing comparison tasks conforms to the magnitude effect, as a result of the distribution around the mean increasing as a constant ratio of a growing mean.<sup>18</sup> As for the model's being subject to the distance effect, according to Dehaene this is also a result of the distribution of neuronal activity.<sup>19</sup>

Finally, to account for the fact that we can represent numbers precisely with numerals and number words, Dehaene proposes that our analog representation of number is integrated with language in a way that gives content to the latter while making the former more precise: “Symbols tune neurons much more sharply, thus allowing them to encode a precise quantity” (1997: 271). Thus a perceived plurality “evokes broad and fuzzy activation in the parietal neurons, while symbols induce firing in a smaller but highly selective subgroup” (ibid).<sup>20</sup>

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<sup>18</sup> Dehaene (ibid), Dehaene and Changeux (ibid).

<sup>19</sup> Dehaene (ibid: 251).

<sup>20</sup> The relation between our analog representation and language remains unclear to this day. See Dehaene and Brannon (eds.) (2011).

## 5. Why the triple code model cannot explain our intuitive grasp of numbers

Frege warned us to be careful “always to separate sharply the psychological from the logical, the subjective from the objective” (1884: x). In the light of this, we need to separate two questions. Firstly, there is the question of what sorts of mental representations must be posited by cognitive scientists in order to explain the relevant data. Secondly, there is the question of whether these representations represent numbers, or some other kind of quantity. This brings me to the first problem for the triple code model, which is that the accumulator does not represent the following constitutive properties of numbers: (I) discreteness, (II) potential infinity and (III) general applicability.<sup>21</sup> I will now discuss each of (I) – (III) in turn.

(I) The natural numbers are as a constitutive matter discrete. In contrast, the variable accumulated by an analog accumulator is continuous. For this reason, as Burge points out, while an analog “representation” can be *correlated* approximately with number, it cannot be accurate or inaccurate based upon whether or not it reflects the right discrete properties. For example, it cannot accurately represent 327 as opposed to 328. But if it does not have accuracy conditions concerning discrete properties, then it cannot *represent* these properties at all. But then it cannot represent natural numbers, since these are as a constitutive matter discrete.

One might try and meet this objection by appeal to the hypothesis that the accumulator accumulates a fixed unit of quantity, rather like an egg timer that is filled by pouring in cups of sand.<sup>22</sup> But this hypothesis is also subject to the previous objection, since the neural analogue of one cup of sand will still be *approximately* one cup, and so for example will not be able to represent 1 as distinct from 1.00001. Further, the claim that the accumulator is integrated with

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<sup>21</sup> Burge and Carey both discuss (I) and (II). See Burge (2010: ch. 10) and Carey (2001, 2008, 2009).

<sup>22</sup> See Dehaene (*ibid*) and Galistel and Gellman (*ibid*).



numerals and number words is of no help either, since the resulting smaller intervals around the “firing peak” are still fuzzy rather than discrete.

(II) The assumption that there are potentially infinitely many sentences of English is a constraint on linguistic theorizing among cognitive scientists; further, the corresponding assumption about numbers is an equally reasonable constraint on cognitive accounts of our arithmetical capacities. But the accumulator embodies a perceptual, pre-linguistic capacity, and as such lacks the recursive or iterative capacity for potential infinity. For example, it does not have the potential to repeat the step of accumulating a fixed unit of quantity indefinitely.

(III) Because the accumulator embodies a perceptual, pre-linguistic capacity, it can only detect the sizes of concrete pluralities. But as Frege pointed out, number is not simply a property of concrete pluralities, since almost anything that can be conceptualized in terms of a suitable kind-concept can also be numbered.<sup>23</sup>

To be clear, I do not deny that the variable accumulated by our accumulator represents something that can be correlated approximately with number. But I do take these three objections to show, conclusively, that the accumulator does not represent natural numbers. Furthermore, it does not represent rational numbers either. This is because while it is constitutive of the rational numbers that between any two of them there is another, the states of the accumulator do not possess this structural feature.

My next objection to the triple code model follows from the forgoing, together with the fact that Dehaene appears to *identify* numbers with neural outputs of his model (call this “reductionism”). For example, we are told:

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<sup>23</sup> Frege (ibid: §14).

Numbers, like other mathematical objects, are mental constructions whose roots are to be found in the adaption of the human brain to the regularities of the universe (ibid: 233).

My proposal is that the brain evolved a number system to capture a significant regularity of the outside world, the fact that at our scale, the world is largely composed of solid physical objects that move and can be grouped according to the laws of arithmetic (2001: 12).

Reductionism, taken together with the fact that the posited reducing mental constructions fail to reflect the properties described in (I) – (III), commits Dehaene to what I will call “Revisionism,” the view that numbers themselves do not have these properties. In which case he is guilty of just the sort of absurd revisionist psychologism that Frege ridiculed.

My final objection is that Revisionism in turn commits Dehaene to a fallacy. The problem is that if the mechanisms in Dehaene’s brain were as he describes them, then he would not be able to argue for the conclusions he does, since doing so requires reasoning with numbers that are discrete, while the mechanisms that Dehaene describes are not discrete. In particular, Dehaene argues for the existence of detector neurons by using psychophysical bridging laws to derive the magnitude and distance effects from distributions of simulated neuronal activity, distributions that are then hypothesized to model the neurons in question.<sup>24</sup> Clearly this relies on statistical argumentation, and in particular presupposes a distribution function, which assigns rational numbers to events.

I think that these objections show, conclusively, that Dehaene’s triple code model does not explain how we represent numbers. But perhaps one can still salvage something from it, by

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<sup>24</sup> Dehaene (ibid: 250; 2007).

abandoning the claim that our analog accumulator provides the numerical content of arabic numerals and number words. It is to this that I now turn.

## 6. Digital models

An analogue accumulator model is not the only way of explaining why our estimative abilities are subject to the distance and magnitude effects, since these effects have also been simulated in a digital “thermometer” model due to Zorzi and Butterworth, which is claimed to represent the number of a given plurality discretely.<sup>25</sup> In this model too, neural input is first normalized. Then a detector neuron is activated once this normalized neural input breaches a precise threshold. The detector neurons are activated incrementally and ordered by magnitude, so if the threshold of a given neuron is breached, it will activate along with all other neurons with smaller thresholds. For example, the neural representations of 4 and 6 can be pictured as follows:

4:

6:

Thus according to this model cardinal numbers are represented by the number of detector neurons or neural units activated (Zorzi et al. 2005: 74). As a result, the model can easily represent discreteness, while explaining the distance and magnitude effects during comparison tasks. For example, here is Giaquinto’s description of how to explain the distance effect:

[C]onsider for example the pairs {6, 8} and {2, 8}. There is a difference of two nodes in the representations of 6 and 8 and a difference of six nodes in the representations of 2 and 8. This means that there is a greater difference of input activity to the response nodes for the pair {2, 8} than to the response nodes for {6, 8}, and so the competition between the response nodes for {2, 8} is resolved more quickly.

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<sup>25</sup> Zorzi and Butterworth (1999). Zorzi et al. (2005).

Unfortunately, this model is still vulnerable to two of the objections leveled against the accumulator model, since it fails to reflect the properties described in (II) and (III): it lacks the recursive capacity to reflect potential infinity and can only detect the sizes of concrete pluralities.

Furthermore, when taken together with Dehaene's doctrine of Reductionism, the model contains an obvious circularity, since according to it numbers are represented by numbers of neural units. This is because the neural units activated by a plurality will represent the number of that plurality only if the units have the property of being equinumerous with the plurality, which is to say the right number. Further, the fact that the right number of units is activated also presupposes that the units are ordered by magnitude. So both ordering and cardinality are presupposed. This objection is reminiscent of another of Frege's arguments: that on pain of circularity numbers cannot be defined as numbers of units.<sup>26</sup> In the present context, the point is that on pain of circularity numbers cannot be reduced to numbers of neural units, since it is illegitimate for a proposed reduction to assume what is to be reduced in the reducing discourse. So whatever the other merits and demerits of the thermometer model, it cannot provide the basis for Dehaene's doctrine of Reductionism.

Another approach would be to attempt to explain how we represent number by claiming that the brain behaves like a digital information processor such as a Turing machine. This, it will be recalled, is a mathematical model of computation visualized as a machine, consisting of inputs, outputs, and instructions for deriving the latter from the former, which together make up its program. The machine is fed a potentially infinite tape that is divided into discrete cells, one of which is being scanned at any moment. Its inputs are ordered sequences constituting the present configuration of the machine. Its outputs consist of the configuration that is derived from

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<sup>26</sup> Frege (ibid: §38).

the inputs according to the instructions. Of course our brains do not contain infinite tapes. Nevertheless, one might claim, something similar simulated in the brain would still be able to count up to large numbers. This, it would seem, is the closest that a model of the brain can come to representing the properties described in (I) and (II).

It is instructive to compare this model with a blatantly unsatisfactory explanation of our ability to count, that the brain contains a homunculus who is able to count. Obviously the latter explanation is circular, unless one can discharge the assumption about the abilities of the homunculus. Similarly, the claim that the brain is an implementation of a Turing machine contains mathematical assumptions that need to be discharged, since a Turing machine is based on the assumptions of discrete infinity, an ordered sequence (the generalization of an ordered pair) and the derivation of one configuration from another. It is because of these assumptions that something similar simulated in the brain would appear to be able to count up to large numbers. For these assumptions to be discharged, one must actually build such a machine, say by implementing it in a neural network. But this requires programming the machine to follow the instructions for counting, which it can only do if it already has the native mathematical resources to scan a digit, write a digit, and move, which are arguably essential to intransitive counting.

In any case, the Turing machine model is at odds with the aforementioned evidence that the brain does not perform number comparison tasks like an ordinary digital computer. For these reasons alone, I conclude that the Turing machine model does not constitute a satisfactory explanation of our ability to count.

## 7. Philosophical applications of cognitive science

Burge, and Giaquinto are more cautious than Dehaene in their claims about what can be explained by cognitive science.<sup>27</sup> They are of the view that logico-philosophical considerations can tell us what numbers are and what is logically required to grasp them, while explanations from cognitive science can tell us how we actually come to do so. For example, Giaquinto endorses the view that “cardinals are properties of sets, but they might also be properties of concept extensions, collections, pluralities, nonmereological aggregates, or some other kind of collective, provided collectives of one or zero items are not excluded” (2001a: 7). Nevertheless, he also claims that answering the question of how we acquire arithmetical knowledge requires finding out how creatures with brains such as ours “actually acquire arithmetical beliefs and skills, a clearly empirical matter” (2001b: 57). He is also at pains to offer a possible account of our intuitive grasp of numbers that posits no abilities beyond those countenanced by cognitive science.

As such, Burge and Giaquinto are not subject to the charges of revisionist psychologism or reductionism. Nevertheless, I will argue that in their attempts to explain how we grasp numbers, these in authors presuppose too much about what they purport to explain, with the result that their proposed explanations are uninformative. I begin with Giaquinto’s attempt to deploy the number sense hypothesis as part of an explanation of how we are able to think about numbers intuitively, via a kind of acquaintance.

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<sup>27</sup> Burge (2007, 2009, 2010), Giaquinto (2001ab, 2012). Both are writing under the influence of Carey (2009). I am convinced that Carey’s account of how we come to understand numerical concepts is subject to similar objections to those leveled against Burge, but for reasons of space I must omit my discussion of Carey’s view.

## 8. Acquaintance

Russell introduces the term of art “acquaintance” for an *immediate* epistemic relation of *private, subjective awareness* that *completely reveals the nature* of the entity with which one is acquainted. I will refer to these doctrines as “Immediacy,” “Privacy” and “Complete Revelation.” I will now describe, briefly, each doctrine and its *prima face* relevance to the question of how we grasp numbers intuitively.

Beginning with Immediacy, this is the doctrine that acquaintance is not mediated by “inference or any knowledge of truths” (1912: 43). In this respect, acquaintance contrasts with the more indirect relation in which a knowing subject stands to an object, when she thinks about it via her understanding of a description that it uniquely satisfies. For example, Russell claims, while we are acquainted with ourselves immediately via introspection, we are not acquainted with the center of mass of the solar system, a point at the center of the sun that is inaccessible to acquaintance and so can only be thought about via a description. As regards our intuitive grasp of numbers, I have already noted that this can be contrasted with the descriptive way in which we think about more abstract structures, such as the family of structures satisfying the axioms of a ring. Because of this contrast, it is *prima face* worth considering whether our intuitive grasp of numbers is a kind of acquaintance.

Turning to Privacy, Russell claims that we have private awareness of among other things our own thoughts, our sense data, and our perceptually remembered experiences. Furthermore, he claims that based on our awareness of sense data, we become acquainted with their sensory properties by abstraction. For example, regarding sense data and their color properties, he says:

by seeing many white patches, we easily learn to abstract the whiteness that they all have in common, and in learning to do this we are learning to be acquainted with whiteness (1912: 101).

What I want to emphasize about this example is that we are not supposed to be acquainted with properties in virtue of grasping inter-subjectively accessible, shareable concepts of these things, but in virtue of abstracting them from sense data of which we are privately aware. This threatens to run afoul of Frege's warning about the need to separate the subjective from the objective. In the present context, the point is that we should be wary of the possibility that our private visual intuitions of numbers are different from our objective concepts of them, and that the former are as a result an imperfect guide to the latter. (More on this in the following section.)

Russell's doctrine of Complete Revelation is that when one is acquainted with an entity one grasps it entirely, rather than in a certain limited way. This is also exemplified in his characterization of acquaintance with sensory properties:

The particular shade of colour that I am seeing may have many things said about it... But such statements... do not make me know the colour itself any better than I did before: so far as concerns knowledge of the colour itself [by acquaintance], *as opposed to knowledge of truths about it*, I know the colour perfectly and completely when I see it, and no further knowledge of it itself is even theoretically possible (ibid: emphasis added).

This has some appeal as a doctrine about our intuitive grasp of numbers, since knowledge of arithmetical truths about numbers is arguably unnecessary for having an intuitive grasp of them.

With that said, I now turn to Giaquinto's theory of acquaintance and the extent to which his view accords with Russell's doctrines.



### 9. Giaquinto on acquaintance

Like many contemporary philosophers, Giaquinto rejects Russell's theory of perception, according to which we are directly aware of sense data but not the objects that cause them. Rather, he is of the view we are directly aware of perceptible public objects, which is tantamount to a rejection of Russell's doctrine of Privacy. Furthermore, Giaquinto rightly emphasizes Russell's tendency —on display in the above quotation— to characterize being acquainted with an entity as knowing an entity or possessing revelatory knowledge of it. Taken together with the rejection of Privacy, this raises a problem, because one can be directly aware of public objects without possessing revelatory knowledge of them. For example, one can be directly aware of Manhattan on seeing it for the first time, without being said to know it at all, let alone perfectly and completely.

To resolve this problem, Giaquinto proposes that in order to know an entity by acquaintance, one must come to know it via one's experiences of it, or, in the case of sensory properties, by one's experience of its instances. Turning to sensory properties in particular, Giaquinto claims that a Russellian account of our acquaintance with properties is to be found in the psychological theory of category acquisition, which he claims can explain how, for example, French infants learn the phoneme 'u' in 'tu' as distinct from 'ous' in 'vous':

Initial category acquisition results from the automatic and unconscious operation of cognitive mechanisms activated by repeated experience of instances. That fits Russell's formulation of learning to abstract whiteness from seeing many white patches, provided that we ignore the suggestion of intention and effort that the word 'learn' carries (2012: 505).

According to Giaquinto, one is acquainted with a sensory property  $F$  if one has (i) perceived instances of it and, as a result, (ii) abstracted a category  $C$  such that (iii) one can apply  $C$  exclusively to instances of  $F$  and so discriminate these from non-instances. This is not yet sufficient for completely revelatory knowledge of a sensory property, which is what Russell is after. However, Giaquinto claims that one must also be able to (iv) recognize instances of  $F$  as instances, (v) search for instances of  $F$  and (vi) imagine instances at will in sensory imagination.<sup>28</sup>

To what extent does this view accord with Russell's doctrines about acquaintance? As regards Privacy, the notion of acquiring and applying a category is no part of a public practice. Further, for simple properties such as colors and sounds, it is not unreasonable as an account of Complete Revelation. As for Immediacy, Giaquinto can also claim to have captured the grain of truth in this doctrine, since the process of acquiring and applying categories is supposed to be perceptual and sub-personal, and is not supposed to involve conscious inference or any knowledge of truths.

Turning to numbers, we have already seen that Giaquinto believes that cardinal numbers are properties of pluralities (or of something very similar). Further, he proposes that they are sensory properties that we can perceive with our number sense.<sup>29</sup> Furthermore, he claims that it is sufficient to be acquainted with a cardinal number  $m$  that one has (i') detected instances of  $m$  with one's number sense and (ii') acquired a numerical concept of  $m$ , such that (iii') one can

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<sup>28</sup> Giaquinto (2012: 506).

<sup>29</sup> This would address Benacerraf's epistemological problem, that we lack a plausible account of our cognitive and epistemic access to numbers on the assumption that they are abstract objects. See Benacerraf (1973). Maddy (1990) claimed we are able to see very small sets with something like a number sense, but subsequently retracted her indispensability argument for the claim that what we see are *sets* as opposed to collections, pluralities etc. Furthermore, her account of our number sense predates much empirical work on the topic.

apply this concept exclusively to its instances and so discriminate these from adjacent non-instances.<sup>30</sup> To meet (i') – (iii') it is claimed that we use our innate number sense to detect cardinal size. Then, once we have acquired cardinal concepts—which are not supposed to be innate— during development, we use our number sense to guide our application of these concepts to very small pluralities. So we are claimed to meet a sufficient condition for being acquainted with very small cardinals.

As for how numerical concepts are acquired, Giaquinto suggests that children may learn them in part by associating number sense representations with small numeral-like numerical concepts. But, as we have seen, while this may be of help, it cannot be the whole story, since the number sense does not itself represent discrete cardinal numbers. He also suggests—citing Cantor—that what may be required is to abstract from one's counting experience “a category representation of sets of a given size, one for each set-size from 1 to 3” (2001a: 13). (In which case, condition ii' above would be a special case of condition ii.) Since, on Cantor's view, such abstraction requires abstracting away from the nature of the elements of a plurality and the order in which they are given,<sup>31</sup> the result will be a multitude of units, for example | |. Then, Giaquinto continues, these might “serve as representations of those cardinal numbers and get mapped onto the initial numerosity [number sense] representations” (ibid).

Does this account of acquaintance with cardinal numbers accord with Russell's doctrines about acquaintance? In certain respects it accords with Immediacy, since the detection of numbers and application of concepts is supposed to be facilitated by our perceptual number sense rather than discursive counting. Further, the acquisition of small numerical concepts is supposed

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<sup>30</sup> Giaquinto (2001a). In this paper Giaquinto only proposes conditions (i)-(iii) as a sufficient condition for acquaintance with properties. I am not sure whether he would also claim that we meet his expanded set of conditions viz. cardinal properties.

<sup>31</sup> Cantor (1895)

to result from sub-personal category acquisition from early counting experience. However, the account fails with regards to Complete Revelation of numbers, since —to repeat— the number sense represents magnitude, not discrete numbers. To address this problem, Giaquinto might instead appeal to his account of how we abstract category representations consisting of small multitudes of units, since these are discrete and so more plausible candidates to represent numbers. But this raises several problems.

Firstly, the number sense then drops out of the picture as a source of acquaintance with numbers. Secondly, Giaquinto cites no evidence whatsoever for the claim that a sub-personal analogue of Cantorian abstraction is how we in fact come to represent small numbers; rather, this claim is a speculation based on philosophical reflection about what is required for doing so. Further, the speculation does not help as stated, since as Frege argues, numbers cannot be represented by multitudes of units, unless the units are differentiated in some way.<sup>32</sup> Later in this chapter I will discuss whether this last objection can be met. However, for the moment I will rest with my first objection, that we are not acquainted with numbers via our number sense.

Next I want to examine a related proposal, due to Tyler Burge. I begin with Burge's discussion of acquaintance.

#### 10. Burge on acquaintance

Burge charges that Russell only counts our grasp of sensory properties as acquaintance, because he conflates how we grasp properties with how we think about objects, namely by standing in a referential relation to them:

Russell counted grasp of universals an acquaintance relation. I believe that this position resulted from his characteristic conflation of understanding with referential

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<sup>32</sup> Frege (ibid: §39).

relations to objects. In predicating a concept of an object in the thought that man is a great pianist, we think the concept is a great pianist as part of thinking the thought. Thinking the concept is not a representational relation to the concept (2009: 66, emphasis in the original).

The first thing to note is that Russell is not *always* guilty of conflating understanding with reference. For example, he famously distinguishes two ways in which the entities that he called “relations” can occur in propositions. They can either occur as terms (or objects to be referred to), or they can occur in their relating role:

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 (1903: §54).

To give another example, at this point in his development, Russell has a doctrine about entities that he calls “denoting concepts.” These are propositional constituents—rather like Fregean senses—which are grasped by thinking and most certainly do *not* occur in propositions as the objects that those propositions are about:

A concept *denotes* when, if it occurs in a proposition, the proposition is not *about* the concept, but about a term connected in a certain peculiar way with the concept. If I say “I met a man,” the proposition is not about *a man*: this is a concept which does not walk the streets, but lives in the shadowy limbo of the logic-books. What I met was a thing, not a concept, an actual man with a tailor and a bank-account or a public-house and a drunken wife (ibid: §56).

However, Burge has more objections up his sleeve.

Burge's second objection concerns Russell's doctrine of Complete Revelation, that when one is acquainted with an entity, one grasps it completely, rather than in a certain limited way.

Burge's rejection of this last requirement is emphatic:

I think it would be absurd to think that finite beings can perceive or think about ordinary objects or properties *neat*. We cannot perceive or think about them without doing so in some representational, perspectival, cognitively limited way (ibid: 251).

Burge's conviction that the contents of perception and thought are both perspectival and cognitively limited can also help us understand why, in the case of sensory properties, we should be suspicious of Privacy. This is because if Burge's conviction is correct, then we must distinguish the way a property is presented in perceptual experience from the property itself. But this is what Privacy does not do. Rather, it conflates public, inter-subjectively accessible properties of objects with *private aspects of the way one perceives them*. As Burge puts it:

The qualitative elements in consciousness [such as how white patches look to me] are not objects of reference in perception. They are aspects of ways of referring; they are part of the perspectival framework of perceptual reference (2010: 121).

Further, Burge objects, Russell mistakes *our ability to think about* these qualitative elements of consciousness—as in when one deploys the concept *what white looks like*, while imagining a white patch—for our ability to perceive and refer to the properties that these elements are perspectives on:

Qualitative elements of consciousness are one thing. Singular representation of them (as referents or objects) in thought is another. Treating them as data for perceptual belief is a third. Russell runs these three things together in his notion of sense data.

Russell took universals both as properties of objects and as perspectives of the mind on objects. I believe that this is another fundamental conflation (2009: fn 3).

As a result of these mistakes together, Burge alleges, we get the view that we are privately acquainted with sensory properties. In the light of all this, I return to the question of whether Russell is guilty of conflating understanding and reference. Concerning the primitive notions of logic, he writes:

The discussion of indefinables... is the endeavour to see clearly, and to make others 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. Where, as in the present case, the indefinables are obtained primarily as the necessary residue in a process of analysis, it is often easier to know that there must be such entities than actually to perceive them; there is a process analogous to that which resulted in the discovery of Neptune, with the difference that the final stage—the search with a mental telescope for the entity which has been inferred—is often the most difficult part of the undertaking (1903: *Preface*).

Here Russell seems to say that we can become acquainted with primitive notions, as a result of having performed an analysis. Further, this acquaintance appears to be equated with understanding; this is why it is so difficult to obtain: the point is that indefinables like Russell's notion of a propositional function cannot be given a complete analysis in the form of a definition. Furthermore, this acquaintance is described as "seeing," "perceiving" and looking through "a mental telescope." The analogy with qualia also suggests that Russell is confusing public, inter-subjectively accessible logical notions with the way in which they are grasped while thinking. For it is one's way of grasping the logical primitives, not the primitives themselves, that are as

easily available and as much a part of one's experience of thinking as qualia are part of our subjective experience of perceiving. All this sounds like just the kind of conflation of which Burge accuses Russell.

Given his criticisms of Privacy and Complete Revelation, the only doctrine of Russell's that Burge accepts is Immediacy. Thus his account takes

as key to the *de re*—*de dicto* distinction Russell's idea that *de re* states and attitudes involve a capacity for referring to entities that is essentially nondescriptive, noninferential, and epistemically immediate (2009: 314).

Burge's proposal is that for a thought to be *de re*, it suffices for it to single out a *re* by a capacity other than the means to describe it, and for it to involve “ ‘a not completely conceptual’ relation to a *re*” (2007: 69). The point is that “not completely conceptual” encompasses both *de re* thoughts which are caused by the relevant *re*, and *de re* thoughts which are not so caused; that is, the latter

involve other sorts of not completely conceptual relations between attitude and object —sorts other than those involved in perceptual belief (ibid).

Next I will describe Burge's account of how we are related to numbers.

### 11. Burge on *de re* thoughts about numbers

As we have seen from Burge's criticism of Complete Revelation, his view is that most thought contents are composed of concepts, which type shareable perspectives on, or ways of thinking of, subject matters. Concepts can be semantically singular or semantically general, and individual concepts are semantically singular. This means that they refer to particular entities, in the way that the individual concept  $\underline{3}$  refers to a particular number. (I now follow Burge's



convention of using underlining to refer to individual concepts, including 2<sup>nd</sup> level individual concepts of concepts.)

Burge claims that Frege “was correct in thinking of numbers as having a certain second-order status” (2007: 71). He also follows Frege by claiming that numbers are grasped not by acquaintance but “only through understanding arithmetical propositions” (2009: 315).

Presumably Burge has in mind applied arithmetical propositions like there are 2 houses of Congress. Here I take the idea to be that we understand concepts such as 2 and 4 by understanding such propositions, and only then form unapplied arithmetical propositions such as  $2 + 2 = 4$ . Furthermore, Burge claims, such discursive understanding can be combined with three other capacities to place one in a not completely conceptual and so *de re* relation to numbers:

Being able to apply a canonical (numeral-like) concept for the number 3 in an immediate perceptual way, seems to me to constitute a ‘not completely conceptual’ relation to the number (2007: 72).

This requires more unpacking. Firstly, one must possess canonical individual concepts of numbers. These are the conceptual counterparts of numerals belonging to the decimal system, rather than descriptions of numbers in the conceptual counterpart of for example successor notation, such as successor(successor(successor(0))). Secondly, canonical concepts corresponding to the digits must form *the base* of mental computation, so that they determine small numbers in a way that is *computationally simple*, rather than determining them as the results of recursive computation. Regarding the difference between computationally simple and complex concepts, Burge has this to say:

Embedded in the content of a complex numeral individual concept (547) are simple individual concepts (5, 4, 7) that involve *de re* application...

Canonical concepts for larger numbers are built by simple recursive rules from the simplest ones...

Understanding what larger numbers are derives from this immediate hold on the applicability of the smaller ones (ibid).

Finally, one must be able to apply these simple individual concepts in a way that is guided by the aforementioned ability to subitize or perceive the exact size of very small pluralities at a much faster rate than that required by discursive counting. (In this respect, the account is reminiscent of Giaquinto's condition (iii').)

Burge claims that

Subitizing in adults may be an aspect of the same system of perceptual tracking that occurs in infants, primates, other mammals, and birds (2010: 485).

However, he endorses a different theory of subitizing than that already discussed:

The tracking of two bodies can be through a perception or perceptual memory that contains two representational contents as of different particular bodies separated in space (ibid: 486).

He continues that this might be

a representational content containing two object files for bodies, each of which has the semantics of a place-holder rather than a singular content with a definite referent.... As particular bodies are shown, the standing place-holder content could temporarily take on reference to particular bodies (ibid).<sup>33</sup>

Let me give an example. Suppose Ralph is shown two books on a table. Burge's proposal is that Ralph can then form a representation that abstracts from the kind *book*, and instead consists of indexed place-holder files for different bodies in space, such as: *this<sub>1</sub> body, that<sub>2</sub> body*. When

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<sup>33</sup> See also Carey (ibid), Trick and Pylyshyn (1993, 1994).

Ralph encounters two bodies of a different kind, such as cows in a field, he can then token and demonstratively apply this representation of bodies to the plurality of cows. Further, Burge claims, if Ralph *also* understands numerical concepts, then the aforementioned ability can guide his application of the concept of 2 to this plurality. Although Burge does not say this, I presume it is his view that by perceptually applying the concept of 2 to a given plurality, Ralph thereby applies the *number* 2 to that plurality, because on Burge's view concepts are ways of thinking of their referents. Thus the application of the number 2 to a plurality is also guided by perception rather than counting.

This account of subitizing as perceptual tracking posits production and deletion of placeholder files for bodies. This is similar in certain respects to counting, since the placeholder files are supposed to be discrete. So it raises the question of whether perceptual tracking enables us to represent discrete natural numbers (individually rather than as a progression), and arithmetical operations on them. Burge's answer to these questions is a resounding 'No'. For the account appeals to the ability to perceive *objects* and attribute one of their first-order properties, but does not appeal to the ability to perceive cardinal number: "Subitizing is not perception of abstract objects, the numbers" (2009: 313). Further: "The ground for not taking specific *numbers* to be represented is a straightforward inference to the best explanation" (2010: 488). That is, a theory of subitizing that appeals to the ability to perceive objects and attribute one of their first-order properties is simpler, and so, by inference to the best explanation more plausible than one that posits the ability to perceive numbers.

According to Burge, the temptation to think of perceptual tracking as representing numbers and arithmetical operations is an example of "the individual representationalist syndrome" of taking the subject of one's theory to represent what makes representation possible

(*ibid.*) I begin with the case of numbers. Obviously the number of files in the representation *this<sub>1</sub> body, that<sub>2</sub> body* must be of the same number as the bodies represented, and the subject must be sensitive to this at the sub-personal level, otherwise the representation would not be an accurate guide to number. As Burge puts it “The quick enumeration of up to four bodies is apparently carried out by an automatic sensitivity to the number of activated indexes” (*ibid.*: 485). What I take Burge to mean is that the subject’s “sensitivity” to or ability to keep track of number consists of the ability to successfully put files and bodies in one-to-one correspondence by the demonstrative application of files to bodies. But although the principle of one-to-one correspondence is thus used to explain perceptual tracking, it is not supposed to be represented by the files, and neither is number.

Further, according to Burge, a system that produces and deletes placeholder files does not thereby represent operations like addition, because there is “a low upper bound on the number of indexes available for perceptual tracking” (*ibid.*: 487). As we have already seen, no such representations are hypothesized to exist for pluralities of more than four members, at which point estimation is thought to take over. It follows that unlike the numbers, the production of placeholder files is not closed under addition or successor.

Having argued that perceptual tracking does not give us perceptual access to numbers, Burge conjectures that

Genuine arithmetical capacities seem to be decidedly propositional and conceptual.

They emerge, at least in performance, only after the advent of language (*ibid.*: 491).

Further, it is only

in individuals who have an understanding of a numerical system, [that] the primitive subitizing capacities join with conceptual abilities to support noninferential, noncomputational numerical assignments in thought to small groupings (2009: 313).

I now turn to Burge's theory of *de re* understanding.

## 12. Burge on *de re* understanding

I begin with the propositions through which we grasp numbers, such as there are 2 houses of Congress. Burge claims that these can be “comprehended” immediately as well as understood discursively, because of the three aforementioned capacities:

[C]omprehending the thoughts that canonically specify the smallest natural numbers through numerals is essentially linked to a noninferential representational ability—the conceptualized successor of subitizing. This is recognition of numbers and application of numbers without calculation or description. It is recognition through singular understanding (2009: 315-16)

It is not clear exactly what Burge intends by “comprehension.” However, I believe that he is trying to describe a more restrictive theoretical correspondent of what we would intuitively characterize as “knowing what content is expressed:” a kind of understanding that has the epistemic immediacy of acquaintance but not its privacy:

Comprehension in the third-person way is understanding that is epistemically immediate, unreasoned, and non-inferential and that carries no presumption that the comprehended material is one's own. It may be one's own. But it is comprehended without relying on taking it as one's own immediate or remembered product (1999: 350).

This requires some more unpacking. Before I say what Burge means by “third-person,” I begin with the question of what he means by “immediacy,” which he elaborates on as follows:

Comprehension is at least as direct and noninferential, psychologically and epistemically, as perceptual relations. Comprehending a representational content is exercising an ability that is constitutively associated with inference. But it is not itself inferential or descriptive (2009: 312).

The reason that comprehension is “constitutively associated with inference” is that comprehension entails what Burge calls “competence understanding,” and this in turn requires being able to engage in inference. For example, competence understanding of *that all men are created equal* requires competence understanding of the concept *all*, which in turn requires the ability to make certain logical inferences. But given that one already has this ability, one can understand *all* immediately—this is required for comprehension. In short, comprehension is a type of immediate understanding, despite being associated with inference, because it requires that one can already engage in the discursive reasoning that constitutes competence understanding.<sup>34</sup>

I now turn to the challenging interpretive question of what Burge means by “the third-person.” It is clear that comprehension is supposed to be distinct from the minimal kind of competence understanding that consists of being able to think or express first-order propositions about the world. For while comprehension entails competence understanding:

The ability to think thoughts—competence understanding—does not count as comprehension unless it is accompanied by third-person comprehension (2011: 366)

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<sup>34</sup> See also Burge (2009: 316)

Thinking thoughts does not itself entail comprehending them—in the sense of having a capacity for a third-person perspective on them (ibid: 366-7).

Here we should contrast third-person comprehension with introspective acquaintance.<sup>35</sup> To be introspectively acquainted with the content of one's own thought, one must rely on the fact that the act of thinking it takes place in one's own mind, for otherwise one could not access it introspectively. But as we have already seen from a previous quote (1999: 350), third-person comprehension of the contents of one's thoughts is not supposed to rely on an act of thinking being one's own. So what does it rely on? According to Burge it involves:

correctly assigning a thought content of one's own thinking to an expression or expressive event that causes that thinking (2011: 366).

This suggests that Burge intends to explicate 'third-person perspective' in terms of being correct rather than incorrect, and so in terms of the possibility of a mistake occurring as one assigns a thought content to an expression or expressive event, something that is impossible on paradigmatic accounts of first-person access. In conclusion, Burge's proposal appears to be that one comprehends a thought content just in case one can immediately, correctly and fallibly assign that content to an expression or expressive event.

Now I return to the thoughts that canonically specify the smallest natural numbers through numerals. How is it that we comprehend these? Here the proposal may be that learning to count gives us competence understanding of the concept 2 through propositions like there are 2 houses of Congress. Further, we can count to 2 at the drop of a hat, and immediately perceptually apply 2 to small pluralities without counting. Finally, these abilities allow us to

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<sup>35</sup> Kripke argues that one's introspective acquaintance with a given concept is one's canonical concept of that concept. See Kripke (2011).

immediately but fallibly assign 2 to '2' in the context of other statements of the form 'there are 2 F's.' This suffices for comprehension.

Burge also claims that being able to apply canonical concept of numbers in an immediate perceptual way, is required for comprehension of *unapplied* arithmetical propositions like 2 + 2 = 4. Here the idea appears to be that learning to engage in discursive calculation gives us competence understanding of 2 + 2 = 4. Further, given that one already has this ability and can perceptually apply the concepts 2 and 4, one can then assign the correct content to '2 + 2 = 4' immediately without calculation. This would serve to explain the intuition that such propositions seem in Burge's words to be "underived from general principles and irreducibly singular from an epistemic point of view" (2000: 40). For example, comprehending 2 + 2 = 4 does *not* seem to require understanding the corresponding proposition expressed in descriptive successor notation.

The considerations in the previous paragraph involve an application of Burge's transcendental argument for *de re* thought.<sup>36</sup> The argument is that the capacity for *de re* thought is required to think *any* thoughts with definite representational content, because to do so one must possess some concepts that are immediately related to their subject matter, which requires the capacity for *de re* thought. In particular:

[A] condition on having attitudes in pure mathematics is an ability to apply it, or at any rate to be able to apply other attitudes in perceptual or practical *de re* ways....

[P]ure mathematics almost surely requires supplemental singular abilities if it is to have genuine, autonomous, representational content...

*De re* relations to the numbers hinge on further *de re* relations to objects that one counts with the numbers. The perception-based counting is a necessary condition for

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<sup>36</sup> Burge (1977, 2007).



both the *de re* relations to the numbers and the comprehension of pure mathematics (2007: 78).

### 13. Problems for Burge's account

My first objection is that if we allow Burge the assumption that there are *de re* thoughts about numbers, then his theory is too restrictive, since according to it we can have such thoughts about only the smallest few numbers. This is because we are only able to subitize the exact size of pluralities of up to three or four members, and have to figure out the exact number of even slightly larger pluralities by more discursive means. Creatures with some mathematical training can do this by first factoring such pluralities into smaller ones they can subitize, then applying the relevant numerical concepts to these smaller pluralities, and then adding. And perhaps being able to do this places one in a relation to a number that is neither entirely immediate, nor quite as discursive as discovery by counting or calculation, but somewhere in between. But while it seems plausible to say that we can do this for 9 by factoring it into three groups of three and then adding, most of us are, presumably, unable to do this for pluralities of 547 or even 47. And in any case, Burge's requirement of computational simplicity still rules out that we can have suitably immediate thoughts about these slightly larger numbers.

Burge is aware that this aspect of his theory makes it questionable, and is willing to grant that theories according to which multi-digit numerals facilitate *de re* thought about numbers are also "tenable" (2007: 74). However, he has a clear preference for the very strict theory of epistemic immediacy described above, arguing that it is preferable to the less restrictive theory due to Kripke, according to which decimal numerals including multi-digit ones are suitably epistemically immediate. Kripke's theory is motivated by consciously accessible, intuitive evidence, in particular the intuition that decimal numerals—including multi-digit ones—are

“immediately revelatory” (forthcoming).<sup>37</sup> By this Kripke means that if a decimal user counts or is told that the number of guests is 547, he thereby *knows how many* guests there are; likewise, if a decimal user calculates that the factorial of 5 is 120, he thereby *knows what number* the factorial of 5 is; no further inference or fact-finding is needed.

The issue here is whether we should characterize epistemic immediacy intuitively, as Kripke does, or in Burge’s more restrictive theoretical terms. Burge’s complaint is that Kripke’s intuitive notion ignores “evidence from psychology” that 547 is understood inferentially, by performing sub-personal, recursive computations on its psychologically basic, perceptually applicable elements, and that such evidence suggests we are immediately related to 4 but not to 547. But there are various reasons to resist these claims.

Firstly, there is the aforementioned worry that Burge’s criterion is too restrictive. Is it a consequence of Burge’s view that no one *really* knows how many F’s there are or what number they are thinking about, by grasping a multi-digit numeral? If so, then the view has an absurd consequence. Of course Burge will respond that from the point of view of cognitive science this consequence is not absurd. However, it is not clear why the point of view of cognitive science is of relevance here. This is because in ordinary arithmetical practice we reason from consciously accessible thoughts to others, and resolve computations with multi-digit numerals. So evidence from cognitive science about unconscious human effort—the time taken by sub-personal mental processes—is of questionable relevance to understanding arithmetical practice. It should also be noted that the point of view of our practice is the one from which the mathematical apparatus assumed in cognitive science was set up.

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<sup>37</sup> I discuss Kripke’s theory in chapter 4.

Further, Burge does not offer any actual *evidence* that 547 is understood inferentially by performing sub-personal computations on its perceptually applicable components. Rather, this claim appears to be based on the *theory* that the subject's understanding of multi-digit numerals requires recursion,<sup>38</sup> which, since the publication of Hauser, Chomsky and Fitch's influential paper, has been invoked widely in cognitive science as a requirement for our knowledge of both language and arithmetic.<sup>39</sup> Recursion is the capacity for iterating the step of taking the previous value of an applied function as an argument, as in:

$$a, f(a), f(f(a)), f(f(f(a))) \dots$$

A special case of this is the capacity for iterating a step, as exemplified by

$$|, ||, ||| \dots$$

What this makes clear, I hope, is that Hauser et al. intend to argue that recursion is a *logical* prerequisite of grasping both numbers and language. To this claim, they add the *speculation* that recursion is also a neurologically realized sub-personal computational capacity. These claims do not constitute *evidence* that we are immediately related to 4 but not to 547, even when taken together.

I now turn to problems with another aspect of Burge's criterion of immediacy: the perceptual applicability of understood, numeral-like numerical concepts. Recall that for Burge it is only

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<sup>38</sup> Burge (2007: 73-74).

<sup>39</sup> Hauser et al. (2002). Their view seems to be that natural selection did not select for the capacity of recursion, since this is simple enough *not* to be an adaptation, was largely unused and is not (to our knowledge) found in other animals. Instead they hypothesize that the capacity is an innate by-product of other human traits that were selected for.

in individuals who have an understanding of a numerical system, [that] the primitive subitizing capacities join with conceptual abilities to support noninferential, noncomputational numerical assignments in thought to small groupings (2009: 313).

This raises the question of what is required to understand canonical numerical concepts.

Burge's proposal is that understanding such concepts requires understanding applied arithmetical propositions, which in turn requires the ability to count:

Understanding '3' involves understanding 'there are 3 F's', which in turn requires being able to count the F's —put them in one-one relation to the numbers up to 3 (2007: 72).

Obviously this gloss of counting presupposes a grasp of numbers, and, as earlier quotes show, it is Burge's view that that we cannot represent numbers prior to acquiring concepts of them. So if this account is to avoid circularity, it must assume that we can *already* represent numbers via propositions containing *non-numeral-like* concepts, and can use the latter in our counting experience, through which we come to understand propositions containing concepts like 3. But in the present context this is a significant assumption about what is to be explained, one that needs spelling out. (In my view, Frege and Alonzo Church together provide the resources to break this circularity. More on this in the final chapter.)

One can remove reference to numbers from the requirements for counting, by stating them as follows. Firstly, the words in the count list must be recited in a stable-order. Secondly, a one-to-one correspondence must be established between the words in the count list and the objects counted. Thirdly, one must be able to give the final word of the count in answer to the question 'how many F's?' However, as Burge himself realizes,<sup>40</sup> it is possible to meet all of these

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<sup>40</sup> Burge (2010: 491).

requirements without grasping the cardinal significance of counting or grasping which cardinals are denoted by the members of the count list. To see this, consider that there is a stage during development when children can recite a short list of numerals in a stable order, put them in one-to-one correspondence with the F's, and recite the last numeral in the count when asked 'how many F's?' And yet, when instructed to give the experimenter  $m$  F's—where  $m$  is the last numeral recited—they give the experimenter a random number of F's. This result is due to Karen Wynn, who summarizes:

In all cases, children could successfully count larger sets of items than they could give when asked... Thus children's ability to correctly give a certain number of items lags well behind their ability to successfully count that same number of items (1992: 234).

This suggests that meeting the above requirements on counting does not suffice for understanding the cardinal significance of numerical concepts; in Burge's terminology, it does not suffice for full competence understanding. So what does suffice? Burge does not say.

To conclude this discussion, in purporting to explain *de re* thoughts about numbers in terms of the conceptualized successor of subitizing, Burge's account is too restrictive, unsupported by evidence, and based on a significant assumption about what it purports to explain, one that cannot be spelled out in terms of meeting the above conditions on counting. Further, since Burge's theory of pure arithmetical comprehension also appeals to the conceptualized successor of subitizing, it too faces all of these problems. This in turn undermines Burge's application of his transcendental argument for *de re* thought, that a condition on having attitudes in unapplied arithmetic is an ability to apply arithmetical concepts *de re*.

#### 14. Giaquinto on our intuitive grasp of the number structure

In section 9 I criticized Giaquinto's attempt to deploy the number sense hypothesis as part of an explanation of how we are acquainted with very small cardinal numbers, and also criticized his appeal to Cantorian abstraction of undifferentiated units. I now turn to his theory of how we grasp simple mathematical structures intuitively, and whether this can help explain how we can represent a progression of numbers.

According to Giaquinto, we are acquainted with simple mathematical structures via (a) our visual capacities together with (b) the aforementioned capacity to abstract category representations, which in the case of structures are abstracted from experienced configurations:

In the same way [as the visual system can acquire category representations], the visual system can acquire a representation for a category of visual configurations of marks that provide instances of a common structure... I have called these representations *category specifications* (2007: 220).

We can recognize a perceived configuration of marks as an instance of a certain structure, by activation of an appropriate visual category specification. Thus, I suggest, we can have a kind of visual grasp of structure that does not depend on the particular configuration we first used as a template for the structure. We may well have forgotten that configuration altogether. Once we have stored a visual category specification for a structure, we have no need to remember any particular configuration as a means of fixing the structure in mind. We can know it without thinking of it as 'the structure of this or that configuration' (ibid: 221).

But what exactly are we claimed to have stored? Giaquinto claims that in the case of numbers, the category specification abstracted is a visual representation of points marked on a line

extending in a direction. This is not implausible, since there is evidence that the direction of this line is associated with increasing magnitude representations (of our number sense), and with reciting numerals in culturally specific ways.<sup>41</sup> Indeed, the association of numerals with direction may explain why the latter is also associated with magnitude, since numerals are associated with magnitude. Further, direction is associated with progressions of names of months and letters of the alphabet, suggesting that it is associated with ordering independently of its association with magnitude.<sup>42</sup>

Above I described the category specification as a visual representation. However, according to Giaquinto, our category specification of the numbers is not a visual *image* of points marked on a line extending in a direction. Rather, it is a set of stored sub-personal representations of visual *features* of such a line (such as its direction), the activation of which allows us to visually imagine the line in a way that is dependent on parameter values corresponding to viewpoint, distance and orientation, which themselves act on the image, continuously changing the image in a way that is subjectively like perceptual scanning. A momentary image generated by activation of that category specification will represent only a finite portion of the line; but the specification that the line has no right end ensures that rightward imagistic scanning will never produce an image of a right-ended line. In this way the category specification is a visual representation for a line that extends infinitely in one direction (2008: 55).

In addition to generating a continuously changing visual image, our category specification of the numbers is also supposed to have abstract conceptual content, in that it is supposed to represent a

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<sup>41</sup> Shaki et al. (2012).

<sup>42</sup> Gevers et al. (2003, 2004).

kind of set. To my mind, the most valuable aspect of Giaquinto's proposal is his claim about how we discover this conceptual content:

[A]s a result of having the category specification, we have a number of dispositions which, taken together, give some indication of the kind of structured set it represents. These are dispositions to answer certain questions one way rather than another. For example:

Given any two marks, must one precede the other? *Yes.*

Do the intermark spaces vary in length? *No.*

Is the precedence of marks transitive? *Yes.*

Can any (non-initial) mark be reached from the initial mark by scanning to the right at a constant speed? *Yes.*

But some questions will have no answer:

Is the intermark length more than a centimetre?

These answers tell us something about the nature of the mental number line as determined by the features specified in the category specification. The answers entail that no mark has infinitely many predecessors; as the marks form a strict linear ordering, this entails that they form a well-ordering. So we can say that the structure of the mental number line is that of a well-ordered set with a single initial element and no terminal element (2007: 227-6).

To unpack the last two sentences, I will call the set that Giaquinto describes ' $X$ .' A relation  $<$  (strictly) linearly orders  $X$  if the following conditions are met:

Irreflexivity: *For every  $x$ ,  $\sim x < x$*

Trichotomy: *For every  $x$  and  $y$  either  $x < y$ , or  $x = y$ , or  $y < x$ .*



Transitivity: *For every  $x$  and  $y$  and  $z$ , if  $x < y$  and  $y < z$ , then  $x < z$ .*

Assuming that  $Y$  is a non-empty subset of  $X$ , an  $<$ -least element in  $Y$  is an  $x$  such that for any  $y$  in  $Y$ ,  $x < y$ . Further, an  $<$ -greatest element in  $Y$  is an  $x$  such that for any  $y$  in  $Y$ ,  $y < x$ . (As Giaquinto might say, for example, a least element is an  $x$  such that  $x$  is to the left of everything; further, a greatest element is an  $x$  such that everything is to the left of  $x$ .) Now I can say that  $<$  well-orders  $X$  because the following conditions are met:

- (i)  $<$  linearly orders  $X$
- (ii) Every non-empty subset of  $X$  has an  $<$ -least element.

Since Giaquinto says that  $X$  has no terminal element, I add:

- (iii)  $X$  has no  $<$ -greatest element.

(As Giaquinto might say, for example,  $X$  contains no element  $x$  such that everything is to the left of  $x$ .) The statement that  $X$  exists and satisfies (i) and (iii) is an axiom of infinity.

It seems to me that Giaquinto's account of mathematical intuition of structure captures something that can be overlooked in the philosophical literature, namely that mathematical intuition is an ability to ask and answer natural questions about a subject matter, which arises from familiarity with that subject matter. In the case of more abstract structures, this familiarity has to be acquired through training; but in the case of the numbers it is already available.

According to Giaquinto's account, this is because it is gained through abstraction of a category specification that disposes us to ask and answer questions:

We have to gather the nature of a number line from our inclinations to answer certain questions about it; although visual experience plays some role in this process, our answers are not simply reports of experience. In becoming aware in this indirect way of the content of a visual category specification for a mental number line, we acquire

a grasp of a type of structured set [a set of number marks on a line endless to the right taken in their left-to-right order of precedence], and we can then know the structure  $\mathbb{N}$  as the structure of structured sets of this type (2008: 57).

Obviously this account would also help explain why we accept the axioms of arithmetic.

Although Giaquinto does not say this, it is worth noting that if the direction of the line was also associated with multitudes of units —of the sort he claims are acquired by Cantorian abstraction— then this could help with the problem of undifferentiated units that I raised earlier (see section 9). For if discrete units were accumulated one-by-one in a direction, then each accumulated unit could be individuated by its relative position on the number line. If so, then Giaquinto’s proposal about how we represent simple mathematical structures can help with his proposal about how we represent individual small cardinals.

However, Giaquinto’s account is an unsatisfactory explanation of our grasp of the number structure, since his description of the relevant category specification presupposes too much about what it purports to explain. To see this, first recall the three constitutive properties of numbers identified in section 5: (I) discreteness, (II) potential infinity and (III) generality. Beginning with potential infinity, Giaquinto offers no explanation for “the specification that the line has no right end, which ensures that rightward imagistic scanning will never produce an image of a right-ended line” (ibid: 55). How is the pre-conceptual representation of this visual feature possible? We are not told. Further, no explanation is offered for why the points on the line are discrete. The only thing of relevance I can find in Giaquinto’s description of the category specification is the following:

One possibility is a set of evenly spaced vertical marks on a horizontal line, with a single leftmost mark, continuing endlessly to the right such that every mark, however

far to the right, is reachable by constant rate scanning from the leftmost mark (ibid: 53).

But this seems to me to assume the discrete infinity of the line, rather than explaining it.

Turning to generality, a *potential* problem is Frege's objection that if we did have to discriminate numbers by their positions in space, then, like a measurement system, they would only apply to things that existed in space. In which case, it would remain to explain how it is that numbers are generally applicable, in that they can be used to number abstract things that do not do not exist in space, as well as spatially located ones.<sup>43</sup>

There is a response that Giaquinto can avail himself of here, since he is of the view that numbers themselves are not inherently spatial, and that in addition to our visual category specification:

We pick up algorithms for generating the number-words/numerals, and we think of a number as what such an expression stands for. The number system thus has the structure of the number-word system and the numeral system. So we can grasp the structure of the set of natural numbers under their natural 'less-than' ordering as the structure of the set of number-words (or numerals) under their order of precedence (ibid: 56).

In which case, there is arguably no more need to discriminate numbers by their positions in space. But then the worry is that (I) – (III) are satisfied in virtue of the representational properties of language, and not in virtue of specific representational properties of the hypothesized category specification.

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<sup>43</sup> Frege (ibid: §40).

Furthermore, the details of the category specification do not appear to explain anything that cannot also be explained by our grasp of the numeral system, or simply by our ability iterate the step of accumulating discrete units in a direction. Consider for example Giaquinto's explanation of how we succeed in grasping a unique number structure – the so-called “standard model.” He appeals to the idea invoked above: that a unique number structure can be determined by grasping

the structure of the set of natural numbers under their natural ‘less-than’ ordering as the structure of the set of number-words (or numerals) under their order of precedence (ibid).

Obviously this faces the objection given at the end of the previous paragraph. So what role is there for the category specification of numbers? Giaquinto claims that because it disposes us to say that no mark has infinitely many predecessors, that precedence is transitive, and that one of any two marks on the line must precede the other,

the category specification determines that the number marks are well-ordered by their relation of precedence. This suffices to determine a unique structure. So we can grasp the structure of the natural number system as the structure of the set of number marks of the mental number line under their order of precedence (ibid: 57).

Notice, however, that Giaquinto assumes that the category specification disposes us to give *the right answers*—those that philosophical reflection tells us are sufficient to pick out the structure of the natural number system— without giving a cognitive explanation of why the category specification does this.<sup>44</sup> To give another example, he writes:

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<sup>44</sup> This is an application of a more general criticism due to Jeremy Avigad. See Avigad (2009).

Non-standard models are ruled out by the category specification, as it dictates that any non-initial mark can be reached from the initial mark by scanning to the right at a constant speed, and that inter-mark spaces do not vary. Of course certain second-order assumptions are built into the underlying conceptions of space, time and motion here; but these are our natural conceptions (2007: 237: fn. 13).

Again, no cognitive explanation is given of why the right assumptions are “built in,” so that the right answers are delivered. Rather, all the work is done by the assumption that this is so. But given this assumption, we can surely grasp the standard model based on our visual acquaintance with an instance of the following pattern: |, ||, |||, ||||..., so long as it is assumed that we are somehow disposed to say the right things about this pattern too. The point is that all the work is done by the assumption that the right answers are delivered, and so it is not clear what philosophical ground is gained by translating this idea into the theory of category specifications.

I conclude that some aspects of Giaquinto’s proposal constitute a promising philosophical description of our intuitive grasp of the natural numbers. However, his notion of a category specification presupposes too much about what it purports to explain, and is as a result uninformative. Further, the details of his theory of category specifications do not appear to explain anything that cannot also be explained by our grasp of the numeral system, or simply by our ability to ask and answer natural questions about a pattern.

### 15. A methodological moral

I hope to have convinced the reader that the authors I have discussed have not provided an informative explanation of our intuitive grasp of numbers. I think the source of the problem is that because Dehaene’s radical cognitive scientism allows him to ignore philosophical considerations relating to language and meaning, his explanation misses its target; this, together

with his other assumptions and the indispensability of numbers, commits him to a fallacy. On the other hand, because philosophers like Burge and Giaquinto take into account *so many* philosophical considerations about what numbers are and what is logically required to grasp them, there is little left for their psychological theories to explain. This suggests that we should continue to focus for the most part on philosophical considerations rather than those from cognitive science – especially considerations relating to the representational properties of language.

## Chapter 2: Frege's logicism

### 1. Introduction

I previously argued that a major stumbling block for cognitive scientism is to explain the infinity and general applicability of numbers. In this chapter I turn to Frege's attempt to explain these phenomena, by showing that the axioms of arithmetic can be derived in logic from generally applicable logical axioms. Based on Frege's remarks I lay out the necessary criteria for assessing the philosophical significance of this derivation – criteria regarding both axioms and definitions. I also argue that Frege accepts his logical axioms on the basis of their fruitfulness as well as their self-evidence, and that he could and should argue that one aspect of their fruitfulness is that they help him to discover the senses of the axioms of arithmetic. This brings me to Frege's views on sense, analysis and synonymy. Here I offer an interpretation of Frege on the basis of which I defend him against recent criticism from Patricia Blanchette. In my view, her criticism is based on overly anthropocentric conceptions of analysis and synonymy, and a failure to distinguish between different degrees and kinds of understanding. The present chapter thus continues the criticism of overly anthropocentric theories of mathematics, which began with the discussion of Burge et al. in chapter 1.

After that, I assess Frege's definitions according to the aforementioned criteria. I show that while there is more than a grain of truth in Frege's definitions, there are also conclusive objections to them, including two that are made by Kripke. This discussion sets the stage for the discussion in chapter 6, in which I amend Frege's definitions in a way that takes account of Kripke's objections.

## 2. Frege's philosophical goals and their precedents

The idea that mathematical thinking proceeds from basic assumptions is almost as old as mathematics itself, as is the partitioning of such assumptions into basic *propositions* and *primitives*, and the further partitioning of basic propositions into the *common* and the *particular*. For example, the basic propositions in Euclid's *Elements* are partitioned into "postulates," which must be assumed to think about a particular subject area such as geometry, and "common notions," which are not concerned with a particular subject area.<sup>45</sup> For instance, Euclid's fourth postulate *that all right angles are equal to one another* concerns geometric figures in particular. In contrast, the common notions *that the whole is greater than the part* and *that any two things equal to the same thing are equal to each other* can be taken to apply to everything, not just to figures in geometry. In addition to postulates and common notions, Euclid also provides a list of definitions expressing his analyses of the mathematical primitives of particular subject areas, such as his definitions of a line as a length without breadth, and of number as a multitude of units.<sup>46</sup>

Similar ideas to Euclid's are found in Aristotle's discussion of first principles in the *Posterior Analytics*:

I call an immediate basic truth of syllogism a "thesis" when, though it is not susceptible of proof by the teacher, yet ignorance of it does not constitute a total bar to progress on the part of the pupil: *one which the pupil must know if he is to learn anything whatever is an axiom* (AnPo: Book I, Part 2, emphasis mine).

Aristotle repeatedly characterizes certain axioms as common among the sciences, in a way that echo's Euclid's idea of a common notion. For example:

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<sup>45</sup> See Euclid (*Elements: Book I*)

<sup>46</sup> See Euclid (*Elements: Book I, VII*).



The axioms which are premises of demonstration may be identical in two or more sciences (AnPo: Book I, Part 7).

In virtue of the common elements of demonstration —I mean the common axioms which are used as premises of demonstration, not the subjects nor the attributes demonstrated as belonging to them— all the sciences have communion with one another (AnPo: Book I, Part 11).

However, since Euclid and Aristotle were writing prior to the development of algebra, neither appreciated that there is a general method of demonstration that can be applied to both arithmetic and geometry. Nor did Aristotle conceive of logic as such a method, but instead conceived of it as a particular rational science concerned with the activity of reasoning, rather like how he conceived of arithmetic as a particular science of numbers and of geometry as a particular science of figures.

In contrast, Frege of course appreciates that algebra is a general method of demonstration, and this in turn influences his view of logic. For he conceives of the latter as entirely general in its application and forming the common core on which all of science is based, since everything is in the range of its variables. For example, Euclid's fourth postulate can now be stated using general logic and the geometric notion expressed by 'Right Angle' as follows:

$$\forall x \forall y (Right\ Angle\ x \wedge Right\ Angle\ y \rightarrow x = y)$$

Continuing with the idea of generality, I have already noted that Frege thinks that arithmetic shares some of the generality of logic, since numbers are applicable to kind-concepts and almost anything can be bought under a suitable kind-concept:

[T]he only barrier to enumerability is to be found in the imperfection of concepts.

Bald people for example cannot be enumerated as long as the concept of baldness is

not defined so precisely that for any individual there can be no doubt whether he falls under it (1882: 164).

In addition to the requirement that a suitable kind-concept be precise rather than vague, Frege adds:

Only a concept which isolates what falls under it in a definite manner, and which does not permit any arbitrary division of it into parts, can be a unit relative to finite Number (1884: §54).

Thus a suitable kind-concept  $F$  must permit of division into  $F$ 's, but permit no arbitrary division of  $F$ 's into further  $F$ 's. For example, the concept *dog* does not permit division of the members of its extension into more dogs. Moreover, while the concept *sandwich* does permit division of the members of its extension into more sandwiches, not everything that results from dividing a sandwich is itself a sandwich, and so this concept permits no arbitrary division. In contrast, the concept *sand* does permit of arbitrary division into more sand, assuming that everything that results from dividing some sand is also sand.<sup>47</sup>

Because numbers are generally applicable, Frege is convinced that contrary to appearances the axioms (or “laws”) of arithmetic more closely resemble general or common axioms than ones that are particular to a subject matter:

As a matter of fact, we can count just about everything that can be an object of thought: the ideal as well as the real, concepts as well as objects, temporal as well as spatial entities, events as well as bodies, methods as well as theorems; even numbers can in their turn be counted. What is required is really no more than a certain sharpness of delimitation, a certain logical completeness. From this we may

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<sup>47</sup> This assumption is questionable. I return to the topic of count nouns in chapter 6.

undoubtedly gather at least this much, *that the basic propositions on which arithmetic is based cannot apply merely to a limited area whose peculiarities they express in the way in which the axioms of geometry express the peculiarities of what is spatial; rather, these basic propositions must extend to everything that can be thought.* And surely we are justified in ascribing such extremely general propositions to logic (1885: 1, emphasis added).

This is a slight elaboration on an argument from the *Grundlagen*:

The truths of arithmetic govern all that is numerable. This is the widest domain of all; for to it belongs not only the actual, not only the intuitable, but everything thinkable. Should not the laws of number, then, be very intimately connected with the laws of thought? (1884: §14).

As Frege says, immediately after making the above-quoted remarks about vagueness:

Thus the area of the enumerable is as wide as that of conceptual thought, and a source of knowledge more restricted in scope, like spatial intuition or sense perception, would not suffice to guarantee the general validity of arithmetical propositions (1882: 164).

Clearly then, Frege thinks not only that the laws of arithmetic are general, but also that their generality is explained by *logicism*, the view that they are analytic. Further, if the conclusion of this argument is to be established beyond all doubt, the laws of arithmetic must be derived from the laws of logic together with the purely logical notions expressed by Frege's definitions of '0', 'predecessor' and 'number', without appeal to intuition or any other non-logical source of knowledge:

The problem becomes, in fact, that of finding the proof of the proposition, and of following it up right back to the primitive truths. If, in carrying out this process, we come only on general logical laws and on definitions, then the proposition is an analytic one, bearing in mind that we must take account also of all propositions upon which the admissibility of any of the definitions depends. If, however, it is impossible to give the proof without making use of truths which are not of a general logical nature, but belong to the sphere of some special science, then the proposition is a synthetic one (1884: §3).

Unlike the material that was discussed in the previous chapter, Frege's derivation is not supposed to explain how arithmetical knowledge is actually acquired. Rather, all that is required to establish logicism is that the laws of arithmetic *can* be derived from logical laws:<sup>48</sup>

We are concerned here not with the way they [the laws of arithmetic] are discovered but with the kind of ground on which their proof rests; or in Leibniz's words, "the question here is not one of the history of our discoveries, which is different in different men, but of the connexion and natural order of truths, which is always the same" (ibid: §17).

Nevertheless, Frege's definitions are supposed to ensure that his derivation demonstrates the analyticity of *arithmetic* —the subject studied by mathematicians throughout history— as opposed to demonstrating the analyticity of another logically equivalent theory. For while the latter demonstration would be a significant achievement, it would be of questionable relevance to the doctrines of prior mathematicians and philosophers such as Kant, whose doctrines presumably concern arithmetic rather than its logical equivalents. Before I can explain how

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<sup>48</sup> This anticipates Reichenbach's distinction between the logic of discovery and that of justification. See Reichenbach (1938).

Frege intends to achieve this, I must first describe some of the essential features of this methodology in more detail.

### 3. Frege's methodology

Frege takes for granted that language is the bearer of what, in his early writings, he calls “meaning” or “content” (1879, 1884), which is grasped by understanding language. He emphasizes that achieving full understanding may require reflection and analysis, because we often use language competently but unreflectively:

Often it is only through enormous intellectual work, which can last for hundreds of years, that knowledge of a concept in its purity is achieved, by peeling off the alien clothing that conceals it from the mind's eye (1884: vii).

However, in Frege's view, this is not the only route to understanding. For he is especially interested in the aspect of content that is relevant to logical entailment, and so discoverable by what can be inferred from it; thus, in his early writings at least, he is not interested in any difference between the contents of ‘The house is above the river’ and ‘The river is below the house’. Frege calls the aspect of content that is relevant to logical entailment “conceptual content.”<sup>49</sup>

I remark that the contents of two judgments may differ in two ways: either the consequences derivable from the first, when it is combined with certain other judgments, always follow also from the second, when it is combined with these same judgments, [and conversely,] or this is not the case. The two propositions “The Greeks defeated the Persians at Plataea” and “The Persians were defeated by the Greeks at Plataea” differ in the first way. Even if one can detect a slight difference in

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<sup>49</sup> Compare the notion of “logical content” in Salmon (1992).

meaning, the agreement outweighs it. Now I call that part of the content that is the *same* in both the *conceptual content*. Since *it alone* is of significance for our ideography, we need not introduce any distinction between propositions having the same conceptual content (1879: §3).

Assuming conceptual contents are individuated by their entailments, a necessary condition for understanding a sentence fully is to discover what it entails. Thus one of Frege's goals is to show which true conceptual contents—henceforth “truths”—entail which:

The aim of proof is, in fact, not merely to place the truth of a proposition beyond all doubt, but also to afford us insight into *the dependence of truths upon one another*. After we have convinced ourselves that a boulder is immovable, by trying unsuccessfully to move it, there remains the further question, what supports it so securely (1884: §2, emphasis mine).

But the aforementioned condition is not sufficient for full understanding, assuming that one accepts Frege's later distinction between the two aspects of content that he calls “sense” and “reference” (1892a). For as we will see, the sense expressed by a sentence—which Frege calls a “thought”—is individuated more finely than by its entailments.

In order to show which truths can be inferred from which, Frege represents truths with formulae in an unambiguous formal language that he calls “*Begriffsschrift*.” This requires all inferentially relevant aspects of content to be given syntactic representatives, which in turn must be given explicit definitions. Then, once the axioms, definitions and inference rules of the language are laid out, the network of logical relations among truths can be demonstrated, by showing how formulae of *Begriffsschrift* can be derived from others. In particular, Frege's aim is to demonstrate that the laws of arithmetic are inferable from his definitions together with the

laws of logic, by deriving formulae of *Begriffsschrift* representing the former from those representing the latter.

Although Frege's ambition is first and foremost to demonstrate the relationship of arithmetic to *logical* laws and notions, it seems to me that the methodological approach to understanding arithmetic that I have just described can be applied even in the absence of this particular logicist ambition. For one might argue that a correct analysis of arithmetic can be derived from laws, which although not purely logical, are nevertheless sufficiently general to explain arithmetic's general applicability. For example, one might make this argument about the axioms of set theory, or about Euclid's common notions, or towards some version of the Hume-Cantor Principle (more of which later). Since the topic of general laws or axioms will be a major issue in this chapter, I will now look at Frege's doctrines regarding these in more detail.

#### 4. Frege's doctrine of the primitive truths

Regarding the laws of logic, Frege's stated aim is to

arrive at a small number of laws in which, if we add those contained in the rules, the content of all the laws is included, albeit in an undeveloped state (1879: §13).

These are claimed to be "general laws, which themselves neither need nor admit of proof" (1884: §3). Frege characterizes these as "primitive truths:"

Science demands that we prove whatever is susceptible to proof and that we do not rest until we come up against something unprovable. It must endeavor to make the circle of unprovable *primitive truths* as small as possible, for the whole of mathematics is contained in these primitive truths as a kernel (1914: 221).

Frege also emphasizes that there is more than one way of logically systematizing *Begriffsschrift*, and as a result more than one set of primitive truths that can be taken as axioms:

Now it must be admitted, certainly, that the way followed here is not the only one in which the reduction can be done. That is why not all relations between the laws of thought are elucidated by means of the present mode of presentation. There is perhaps another set of judgments from which, when those contained in the rules are added, all laws of thought could likewise be deduced (1879: §13).

And again:

Whether a truth is an axiom depends therefore on the system, and it is possible for a truth to be an axiom in one system but not in another (1914: 222).

Clearly then, the requirement that the primitive truths are unprovable has to be distinguished from their being underivable, since one or another of them may have to be derived in a given systematization of *Begriffsschrift*. So what does Frege mean by “unprovable?” Burge suggests a plausible answer:

[B]asic truths are unprovable in the sense that they cannot be grounded or given a justification by being derived from other truths. They can be derived, according to logical rules, from other truths within certain systems. But the derivations would not be justifications, groundings, or proofs in this epistemically fundamental sense...

... although some basic truths might be expressed as theorems in a formal system, they are not, from the point of view of the natural order of justification or proof, essentially derivative. They are essentially basic. But in the relevant system, they would not be axioms. Thus not all basic truths that are candidates for being axioms are, relative to a given system, in fact axioms (2005: 314).

Assuming this is correct, then how are we to *discern* that a truth is unprovable, if not by the fact that it is underivable?



Clearly Frege does *not* think that the primitive truths must be *obviously* self-evident. For even assuming that he believes that primitive truths are somehow self-evident or self-justifying, he is clearly skeptical of our ability to recognize this unreflectively:

Proof is now demanded of many things that formerly passed as self-evident (1884: §1).

Another route to the discovery of the primitive truths that Frege appears to harbor doubts about, is conceptual analysis of the notions they concern, as is shown by his prophetic discussion of Basic Law V governing extensions of concepts:

$$\text{extension}(\Phi) = \text{extension}(\Sigma) \leftrightarrow \forall x (\Phi x \leftrightarrow \Sigma x)$$

Concerning extensions (which in his later work are value-ranges of functions) Frege says:

A dispute can break out here, so far as I can see, only with regard to my fundamental law concerning value-ranges (V), which has not yet perhaps been expressly formulated by logicians, although one has it in mind, for example, when speaking of extensions of concepts (1893: vii).

Frege also says in retrospect that he adopted Basic Law V on the basis of considerations of fruitfulness (or productivity), not self-evidence:

I have never disguised from myself its lack of self-evidence that belongs to the other axioms and that must properly be demanded of a logical law... I should gladly have dispensed with this foundation if I had known of any substitute for it. And even now I do not see how arithmetic can be scientifically established and brought under review; unless we are permitted – at least conditionally – to pass from a concept to its extension (ibid: appendix).

What this example shows is that if one wants to argue for a primitive truth based on its fruitfulness, one must be careful to establish that this is not just wishful thinking. In order to do this one must, in Frege's words,

obtain a clear insight into the network of inferences that support our conviction. Only in this way can we discover what the primitive truths are, and only in this way can a system be constructed (1914: 221).

That is, one must explore Frege's system and different variants of it, in order to realize what they entail, and to realize that all such variants must be founded on subsets of the same set of consistent truths. This set should therefore be accepted as the set of primitive truths.

I believe that Frege could and should supplement this with the following argument. One should accept his truths as primitive because doing so allows one to make what promises to be a highly non-trivial discovery, namely what we would call "the right modeling" of arithmetic. That is, one should accept truths as primitive because Frege's derivation allows us, for the first time, to fully grasp the senses of the axioms of arithmetic, by fully analyzing terms like 'number' and 'predecessor', as well as demonstrating the relation of these axioms to the axioms of logic.

This argument from fruitfulness raises a very important question. Do Frege's definitions ensure that the formulae he derives *really* express the senses of the axioms of arithmetic? Or do these formulae express truths that are logically equivalent to the axioms while differing in sense? This question is not only of relevance to assessing whether we should accept Frege's axioms. For as we saw at the end of section 2, it is also of broader relevance to assessing whether Frege has any reasonable claim to have achieved his goal of demonstrating the analyticity of *arithmetic* — the subject studied by mathematicians throughout history— as opposed to demonstrating the analyticity of another logically equivalent theory. A similar question is also relevant when

assessing any attempt —logician or otherwise— to derive arithmetic from purportedly primitive truths.

Next I will describe the definitions that Frege uses in his derivation, before turning to his criterion for preserving the senses of the axioms of arithmetic.

### 5. Frege's definitions

Frege distinguishes between two kinds of definitions. The first kind requires the introduction of a *new* simple expression of *Begriffsschrift* and the stipulation of its definiens. The second kind requires the introduction of a simple expression of *Begriffsschrift* corresponding to another simple expression, such as 'predecessor', that is already in use. In this kind of case, the definiens is not simply stipulated but discovered by conceptual analysis, before being assigned to the expression of *Begriffsschrift* by stipulation. This is especially important, since the inferentially relevant content of 'predecessor' may not be evident from its surface simplicity. Of this kind of definition Frege remarks that one can only assert its correctness after analysis "when this is self-evident," and so "what we should here like to call a definition should really be regarded as an axiom" (1914: 227). Both stipulated and axiomatic definitions assign sense and reference to their definienda.

In what follows I use '=' and Frege's symbol '≡' subscripted to indicate identity of sense, even though Frege may not have had sense, as distinct from conceptual content, explicitly in mind in his earlier writing. I begin by giving Frege's definitions of 'equinumerous', 'the number which belongs to the concept *F*', '0', 'predecessor' and 'natural number'.<sup>50</sup> Frege's definition of 'equinumerous' is:

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<sup>50</sup> Of course Frege also needs logical definitions of the relevant arithmetical operations.

there exists a relation  $[R]$  which correlates one-to-one the objects falling under the concept  $F$  with those falling under the concept  $G$  (1884: §72).

We can represent this in modern notation as follows:

$$\text{Equinumerous}(F, G) \equiv_{\text{df}} \exists R[\forall x\forall y\forall z\forall u (Rxy \wedge Rzu \rightarrow x = z \leftrightarrow y = u) \wedge \forall x (Fx \rightarrow \exists z (Gz \wedge Rxz)) \wedge \forall z (Gz \rightarrow \exists x (Fx \wedge Rxz))]$$

Next I turn to ‘the number which belongs to the concept  $F$ ’. For this I introduce a variable-binding operator ‘#’ that attaches to an open formula ‘ $Fx$ ’ to form a term ‘ $\#x: Fx$ ’, which is then defined so as to refer to a certain kind of logical object, namely the extension of a second-level concept, where the concept in question is that of being equinumerous with a given first-level concept (ibid, §68):

$$\#x: Fx \equiv_{\text{df}} \text{the extension of the concept } \textit{equinumerous with the concept } F$$

Frege does not define the notion of an extension of a concept, since he believes it to be a well-understood part of logic that like the notions of a function and an argument, cannot be defined in more basic terms, but only “elucidated.”

Having defined ‘ $\#x: Fx$ ’ I follow Frege in defining ‘0’ as follows (ibid: §74):

$$0 \equiv_{\text{df}} \#x: x \neq x$$

Frege’s definition of ‘ $m$  immediately precedes  $n$  in the number sequence’ is:

there exists a concept  $F$ , and an object falling under it  $x$ , such that the Number which belongs to the concept  $F$  is  $n$  and the Number which belongs to the concept ‘falling under  $F$  but not identical with  $x$ ’ is  $m$  (1884: §76).

For this I introduce ‘ $P(m, n)$ ’ and define it as follows:

$$P(m, n) \equiv_{\text{df}} \exists F \exists x (Fx \wedge [\#y: Fy] = n \wedge [\#y: Fy \wedge y \neq x] = m)$$

To define the general term ‘natural number’, Frege begins with the notion of a sequence (or ordering), the members of which are related by an arbitrary binary relation  $R$ , such that  $y$  follows  $x$  in the  $R$ -sequence just in case  $y$  can be reached from  $x$  by finitely many iterations of  $R$  (1879: §24). Given that Frege’s aim is to establish logicism, *it is absolutely crucial* to introduce a symbol corresponding to ‘ $y$  is the same as  $x$ , or follows  $x$  in the  $R$ -sequence,’ and define it without using ‘reached from  $x$  by finitely many iterations’, or any other intuitive notion. This symbol will be ‘ $R^{*\bar{=}}(x, y)$ ’. But first Frege defines the notion of a concept  $G$  being hereditary in a sequence with respect to  $R$  (1879: §24):

$$\text{Her}(G, R) \equiv_{\text{df}} \forall x(Gx \rightarrow \forall y(Rxy \rightarrow Gy))$$

Since  $R$  is an arbitrarily chosen relation and the first-order variables  $x$  and  $y$  range over all objects, Frege will be able to define the natural numbers in terms of something more general (something a little bit like the modern set-theoretic notion of an inductive set). The ancestral relation  $R^*$  of  $R$  is then defined as relating  $x$  to  $y$  just in case  $y$  falls under every hereditary concept that  $x$  does (ibid: §26):

$$R^*(x, y) \equiv_{\text{df}} \forall G(\text{Her}(G, R) \rightarrow (\forall z(Rxz \rightarrow Gz) \rightarrow Gy))$$

The *weak* ancestral relation  $R^{*\bar{=}}$  of  $R$  can then be introduced and defined in terms of the ancestral of  $R$  as follows (ibid: §26):

$$R^{*\bar{=}}(x, y) \equiv_{\text{df}} R^*(x, y) \vee x = y$$

Since Frege has defined ‘0’ and the immediately preceding relation ‘ $P(m, n)$ ’, we can introduce ‘Natural number( $n$ )’ and define it in terms of the weak ancestral of the immediately preceding relation, such that  $n$  is a natural number just in case  $n$  is the same as 0, or follows it in the  $P$ -sequence (1884: §83):

$$\text{Natural Number}(n) \equiv_{\text{df}} P^{*\bar{=}}(0, n)$$

By these definitions, each natural number falls under every concept under which 0 falls and which is hereditary with respect to the immediately preceding relation. So we see that Frege has defined the numbers in such a way that a version of mathematical induction is true of them. Frege then sketches how he can use the aforementioned version of induction to derive formulae representing the axioms of arithmetic, such as that every number has a successor (ibid: §78-83).

#### 6. Frege's views on analysis

At the end of section 4 I raised the question of whether Frege's definitions ensure that the formulae he derives *really* express the senses of the axioms of arithmetic. Patricia Blanchette separates this into two related questions.<sup>51</sup> Firstly, what are the conditions under which two sentences express the same thought? Secondly, how are the analysandum and analysans of a successful analysis related? Following the order of Blanchette's discussion, I begin with her first question.

Blanchette discerns two candidates to be Frege's condition for when two sentences express the same thought. Firstly, Frege sometimes endorses the view that two sentences to express the same thought if and only if they are in his words "equipollent:"

*If both the assumption that the content of A is false and that of B true, and assumption that the content of A is true and that of B false lead to a logical contradiction, and if this can be established without knowing whether the content of A or B is true or false, and without requiring other than purely logical laws for this purpose, then nothing can belong to the content of A as far as it is capable of being judged true or false, which does not also belong to the content of B (1906: 70).*

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<sup>51</sup> Blanchette (2012: Ch. 2 & 4).

According to this proposal, two sentences express the same thought just in case it can be shown by logic that the assumption that the content of one is false and the other true leads to a contradiction. In other words, they express the same thought just in case they are logically equivalent, standing in exactly the same derivational relationships to all other sentences. This would explain why intellectual work is still needed to see that two sentences express the same thought; it is because one would have to undertake the logical work necessary to appreciate that they stand in exactly the same derivational relationships with all other sentences.<sup>52</sup> Nevertheless, this criterion cannot be correct, since it threatens to collapse all logical equivalents into the same thought, and is incompatible with Frege's oft repeated view that for example ' $2^4 = 4^2$ ' and ' $4 \cdot 4 = 4^2$ ' express different thoughts.<sup>53</sup>

The other criterion of Frege's that Blanchette considers is one she calls the "cognitive criterion:"

[T]wo sentences to express the same thought iff a speaker who understands both of them and assents to one must, on pain of incoherence, also be disposed to assent to the other (2012: 33)

She rejects this on the grounds that it is in tension with Frege's reason for believing that there are objective and publicly graspable thoughts, which is that this doctrine explains how it is possible for scientific knowledge —such as that of the Pythagorean Theorem— to be transmitted from generation to generation. She writes:

The picture of temporally distant scientists investigating the same thoughts fits well with Frege's remarks concerning the possibility of a common science and of his repeated claims that the theorems of a science are a determinate collection of thoughts. The difficulty with this approach from a Fregean point of view is that it

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<sup>52</sup> Blanchette (ibid: 45).

<sup>53</sup> See for example Frege (1891, 1893).

doesn't sit neatly with the fine-grained individuation of thoughts given by the cognitive criterion. For on that criterion, Newton and Weierstrass's sentences express the same thoughts only if they are something like easily recognizable synonyms, sentences with respect to which it would be obviously incoherent to affirm one without affirming the other. And this strong kind of semantic equivalence is, it seems, considerably too demanding: whatever mathematicians aim for over the course of mathematical development, obvious synonymy with their predecessor's sentences is clearly not it (ibid: 34-5).

In a moment I will show that there is another strong kind of semantic equivalence that does not require such equivalence to be obvious. But first I turn to Blanchette's discussion of the relation between the analysandum and analysans of a successful analysis.

As Blanchette notes, Frege's view is that speakers often understand language imperfectly, hence his talk of the enormous intellectual work that is required to acquire knowledge of a concept in its purity (1884: vii). Given this, it is tempting to ascribe to Frege the following two views: (a) that the relation between the analysandum and analysans should be *synonymy*, and (b) that synonymy need not be obvious to speakers whose understanding of the analysandum is imperfect. As Blanchette points out, this fits nicely with Frege's view that conceptual refinement in some areas of mathematics proceeds in part by analysis, through which mathematicians gain an increasingly clear understanding of the same stock of thoughts. Despite this, Blanchette offers three reasons for thinking that (a) and (b) are jointly incompatible with the cognitive criterion:

Firstly, there is the implausibility of the idea that ordinary speakers fail to understand what is meant by such simple sentences as "3 is greater than 2." Secondly, this picture makes it entirely mysterious how the sentences of arithmetic could have come to



express the senses they do, if these senses had been, prior to Frege's work, grasped by nobody. Finally... if the criterion of a successful analysis is thought identity, but the identity... is not something that can be straightforwardly assessed by those who have an ordinary grasp of what those sentences say (when this falls short of the rarefied level of "understanding" suggested by Frege...), then the criterion is of no use for assessing the correctness of a given analysis (ibid: 81).

I will now respond to each of these points in turn.

Firstly, Frege is not committed to the view that ordinary speakers fail to understand arithmetical sentences. For he can say that such speakers have what Burge calls "competence understanding," which is the kind of minimal understanding required to use expressions correctly (see chapter 1, section 12). That is, ordinary speakers can have enough consciously accessible knowledge of the sense or objective condition to be the referent of an expression, to use it correctly, without thereby having reflective understanding of this condition, such that they can articulate it in an analysis.<sup>54</sup> For example, a mathematician can have perfect competence understanding of particular numerals and of the general term 'natural number' —including competence that extends to numbering "the ideal as well as the real, concepts as well as objects" (Frege: 1885: 1)— without being able to articulate in an analysis the condition that numbers apply to kind-concepts. For this reason, as Frege is fond of pointing out, competent mathematicians often speak falsely when they try to articulate what they understand 'natural number' to mean.<sup>55</sup> The reason that they lack reflective understanding may simply be that they have not performed the necessary analysis of how particular numerals and 'natural number' are

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<sup>54</sup> See Burge (2013).

<sup>55</sup> The corresponding point can be made in defense of Frege's definition of ' $P(m, n)$ ' as well, but doing so is harder. I will return to it in section 7.

used, and so are unable to define these terms so as to articulate the condition that numbers apply to kind-concepts. Or, as is the case with some ancient mathematicians like Euclid, it may also be that the post-algebraic conception of generality that is necessary for logical analysis is not available at the time (see section 2).<sup>56</sup>

To Blanchette's second point, if the distinction between competence understanding and reflective understanding is accepted, then there is arguably no problem for Frege about how arithmetical expressions come to express senses that are "grasped by nobody" (ibid). For by being used in ordinary arithmetical practice, arithmetical expressions come to express senses, of which ordinary speakers have competence understanding. For example, because the expression 'one-to-one correlation' is used by people who have some knowledge of logic, understand count nouns and can correlate the members of their extensions one-to-one, it comes to express the sense of 'Equinumerous( $F$ ,  $G$ )', of which said people have competence understanding. Admittedly it is somewhat unclear how, on Frege's account, we can deduce from this understanding an understanding of particular numerals. But I believe that an account of this is in prospect, as I will explain in the final chapter.

Finally, one can agree with Blanchette that thought identity is not something that can be straightforwardly assessed by those who have an ordinary grasp of what the relevant sentences say, while disagreeing with her claim that the cognitive criterion is of no use for assessing the correctness of a given analysis. For, in my view, an "ordinary grasp" of what a sentence ' $S$ ' says is competence understanding of ' $S$ ', when it is reflective understanding that is needed to assess the correctness of a given analysis by the cognitive criterion. One cannot assess an analysis

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<sup>56</sup> The case of Newton and Weierstrass is harder. It may be that Newton not only lacked the theoretical tools to articulate Weierstrass' analysis of the concept of the derivative, but also lacked full competence understanding of the concept itself. I have yet to give this case the attention it deserves.

anthropologically, by asking ordinary speakers whether the analysis is correct. Rather, as I have already indicated, one must instead reflect on how expressions are correctly used in mathematical practice, so that one can articulate, in an analysis, the conditions that guide their correct usage. It is only *after* such analysis has been performed that the cognitive criterion should be applied. In Frege's words:

The fact is that if we really do have a clear grasp of the sense of the simple sign, then it cannot be doubtful whether it agrees with the sense of the complex expression. If it is open to question although we can clearly recognize the sense of the complex expression from the way it is put together, then the reason must lie in the fact that we do not have a clear grasp of the sense of the simple sign, but that its outlines are confused as if we saw it through a mist. The effect of the logical analysis of which we spoke will then be precisely this —to articulate the sense clearly (1914: 228).

This suggests the following condition that I will call “the strict cognitive criterion of synonymy for simple expressions:”

Two expressions express the same sense iff one is syntactically simple and a speaker who fully reflectively understands both of them, cannot, on pain of incoherence, doubt that they express the same sense.

While I am sympathetic to the strict cognitive criterion, I do *not* accept the corresponding version for sentences:

Two sentences to express the same thought iff a speaker who fully reflectively understands both of them and assents to one must, on pain of incoherence, also be disposed to assent to the other.

This will not do for the following reason. Consider a speaker who understands the recursive definitions of the arithmetical operations, including the definition of exponentiation in terms of repeated multiplication. Suppose this speaker fully reflectively understands both ‘ $2^4 = 4^2$ ’ and ‘ $4 \cdot 4 = 4^2$ ’. It seems reasonable to say that she cannot assent to one without being disposed to assent to the other. Yet on Frege’s mature view these express different thoughts. So we are still in need of a Fregean criterion for when two sentences are synonymous.

A proposal that Blanchette does not consider is the one that I call “the strict Fregean criterion for sentences:”

Two sentences to express the same thought iff one sentence can be obtained from the other by the substitution of synonyms for synonyms, in accordance with the strict cognitive criterion of synonymy for simple expressions.<sup>57</sup>

Obviously this criterion accords closely with Frege’s remarks about definition and analysis. Further, and putting analysis to one side for a moment, it can explain why ‘ $2^4 = 4^2$ ’ and ‘ $4 \cdot 4 = 4^2$ ’ express different thoughts. Since ‘ $2^4$ ’ and ‘ $4 \cdot 4$ ’ are both syntactically complex, no substitution of one for the other is allowed by the strict cognitive criterion of synonymy for simple expressions. Rather, the senses that they express are determined by the senses of their parts and how these are combined. These senses are in turn conditions to be the referents of these parts. Further, the condition to be the referent of the expression ‘ $\_ \_ ^4$ ’ is not the same as the condition to be the referent of the expression ‘ $\_ \cdot \_$ ’; and, correspondingly, these expressions are not used correctly in the same way. So one cannot simply replace one with the other. The strict Fregean criterion for sentences reflects that *this fact remains*, even in the event that a

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<sup>57</sup> This is closely related to Alonzo Church’s Alternative (0). According to Church alphabetic change of bound variables is also allowed. See Church (1946). See also Salmon (2010). I am grateful to Salmon for teaching me about Church’s work on the topic of sense identity.

speaker who fully reflectively understands both ‘ $2^4 = 4^2$ ’ and ‘ $4 \cdot 4 = 4^2$ ’, and assents to one, must, on pain of incoherence, also be disposed to assent to the other. The criterion thus yields more insight into the theory of sense than does the cognitive criterion for sentences. (By the way, the above story about ‘ $2^4$ ’, ‘ $4 \cdot 4$ ’ etc. provides —as it must— a solution to the most interesting examples of Frege’s puzzle (those concerning the content of mathematical equations), since it can explain why ‘ $2^4 = 4^2$ ’ and ‘ $4 \cdot 4 = 4^2$ ’ are both true and informative.)

Returning to the topic of analysis, I will now illustrate how two non-trivial Fregean analyses can be argued to preserve the thought expressed according to the strict Fregean criterion. First of all I use the example of the transitivity of following in the  $R$ -sequence:

- i.  $\forall xyz [(y \text{ follows } x \text{ in the } R\text{-sequence} \wedge z \text{ follows } y \text{ in the } R\text{-sequence}) \rightarrow z \text{ follows } x \text{ in the } R\text{-sequence}]$
- ii.  $\forall xyz [(R^*(x, y) \wedge R^*(y, z)) \rightarrow R^*(x, z)]$  (Symbol of Bgr)
- iii.  $\forall xyz [(\forall G(\text{Her}(G, R) \rightarrow (\forall u(Rxu \rightarrow Gu) \rightarrow Gy))) \wedge (\forall G(\text{Her}(G, R) \rightarrow (\forall u(Ryu \rightarrow Gu) \rightarrow Gz))) \rightarrow (\forall G(\text{Her}(G, R) \rightarrow (\forall u(Rxu \rightarrow Gu) \rightarrow Gz)))]$   
(Def of  $R^*(x, y)$ )

This allows debate to focus on whether ‘ $\forall G(\text{Her}(G, R) \rightarrow (\forall u(Rxu \rightarrow Gu) \rightarrow Gy))$ ’ expresses a correct analysis of ‘ $y$  follows  $x$  in the  $R$ -sequence’ by the strict cognitive criterion. If it does, then Frege can reasonably claim to be refining our understanding of the same thought about transitivity that was grasped but not reflectively understood by prior mathematicians.

The second example is Frege’s analysis of ‘0 is the predecessor of 1’, which Blanchette claims does not preserve the thought expressed.<sup>58</sup> Here it will be helpful to recall that

$$P(m, n) \equiv_{\text{df}} \exists F \exists x (Fx \wedge [\#y: Fy] = n \wedge [\#y: Fy \wedge y \neq x] = m)$$

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<sup>58</sup> Blanchette (ibid: 100).

and that

$\#x: Fx =_{\text{df}}$  the extension of the concept *equinumerous with the concept F*.

In what follows I will represent the definiens of ' $\#x: Fx$ ' as ' $\text{ext}(\approx F)$ ':

1.  $P(0, 1)$
2.  $\exists F \exists x (Fx \wedge [\#y: Fy] = 1 \wedge [\#y: Fy \wedge y \neq x] = 0)$  (Def of  $P$ )
3.  $\exists F \exists x (Fx \wedge \text{ext}(\approx F) = 1 \wedge [\#y: Fy \wedge y \neq x] = 0)$  (Def of  $\#y: Fy$ )
4.  $\exists F \exists x (Fx \wedge \text{ext}(\approx F) = \text{ext}(\approx \text{identical with ext}(\approx \text{non-self-identical})) \wedge [\#y: Fy \wedge y \neq x] = 0)$  (Def of 1)
5.  $\exists F \exists x (Fx \wedge \text{ext}(\approx F) = \text{ext}(\approx \text{identical with ext}(\approx \text{non-self-identical})) \wedge \text{ext}(\approx \text{falling under } F \text{ but not identical with } x) = 0)$  (Def of  $\#y: Fy \wedge y \neq x$ )
6.  $\exists F \exists x (Fx \wedge \text{ext}(\approx F) = \text{ext}(\approx \text{identical with ext}(\approx \text{non-self-identical})) \wedge \text{ext}(\approx \text{falling under } F \text{ but not identical with } x) = \text{ext}(\approx \text{non-self-identical}))$  (Def of 0)

Thus the Fregean analysis of '0 is the predecessor of 1' can be shown to preserve the thought expressed by its analysandum, assuming that Frege's definitions are synonymous with the corresponding ordinary notions in accordance with the strict cognitive criterion of synonymy for simple expressions.

A potential obstacle to accepting the strict Fregean criterion of synonymy for sentences is that it precludes the two sides of an abstraction principle from being synonymous. Consider for example the Hume-Cantor principle known as "HP:"

$\#x: Fx = \#x: Gx \leftrightarrow \text{Equinumerous}(F, G)$ .

The problem is that ' $\#x: Fx = \#x: Gx$ ' is syntactically complex, and so its sense—relative to an assignment of concepts to ' $F$ ' and ' $G$ '—is determined by the senses of its parts and how these

are combined; therefore, by the strict Fregean criterion, it cannot be synonymous with the following:

$$\exists R [\forall x \forall y \forall z \forall u (Rxy \wedge Rzu \rightarrow x = z \leftrightarrow y = u) \wedge \forall x (Fx \rightarrow \exists z (Gz \wedge Rxz)) \wedge \forall z (Gz \rightarrow \exists x (Fx \wedge Rxz))]$$

However, the objection continues, the two sides of an abstraction principle *must be* synonymous, since this is required for Frege to give an implicit contextual definition of ‘#x: Fx’ that supplements his failed attempt at a recursive definition of ‘number’ at *Grundlagen* §56.

This objection results from the tendency to think of (consistent) abstraction principles like HP as implicit definitions,<sup>59</sup> and of their role in Frege’s project as definitions of the second kind discussed in section 5, the kind that Frege claims should really be regarded as axioms but nevertheless assign sense and reference to their definienda.<sup>60</sup> But, as Nathan Salmon argues, this view of abstraction principles is not a view that is shared by Frege, being at odds with his very clear remarks on definitions, in which he requires that definienda be simple. Further, Salmon argues, HP *cannot* define ‘#x: Fx’ because of the Caesar problem properly understood. According to Salmon, the Caesar problem is not that HP fails to provide a criterion of identity and individuation for numbers, but that, like all improper definitions, HP together with the non-semantic facts fails to specify the sense and reference of ‘#x: Fx’. On this reading of Frege, he is as opposed to HP *qua* definition by abstraction as he is to the improper definition offered at *Grundlagen* §56, and for the same reason: neither determines sense and reference.<sup>61</sup> This explains why he raises the Caesar problem in both instances.

<sup>59</sup> See for example Linnebo (2004), Wright (1983).

<sup>60</sup> See for example Blanchette (ibid: 91), Beaney (1997: 316, fn 9).

<sup>61</sup> See Frege (1893, vol II: §66; 1914: 224-7). See also Salmon (forthcoming).

So what *does* Frege suppose the role of HP to be? In my view, abstraction principles like HP place constraints on how *Begriffsschrift* is to be extended, by requiring that the new terms on the left hand side be defined so as to refer to things that are identical if and only if the entities indicated on the right hand side stand in the relevant equivalence relation. For example, HP requires that ‘ $\#x: Fx$ ’ and ‘ $\#x: Gx$ ’ be defined so as to refer to things that are identical if and only if  $\text{Equinumerous}(F, G)$ . If I am correct about this, then HP is merely a condition that any acceptable definition of ‘ $\#x: Fx$ ’ must be shown to satisfy, by substituting the definition into HP and deriving the resulting sentence as a theorem, just as Frege tries to do at *Grundlagen* §73. Finally, to return to the main point of contention, laying down such a condition does not require that the two sides of HP be synonymous, given that the right hand side is not supposed to assign sense and reference to the left hand side.

To take stock, I can see no obstacle to saying that if Frege’s analyses of the arithmetical primitives are correct, then derived sentences of *Begriffsschrift* can be synonymous with their corresponding ordinary arithmetical sentences, without this being easily recognizable or obvious to someone who has not yet undertaken an analysis of the ordinary arithmetical sentences. Further, if such synonymy is achieved, then Frege can claim to have demonstrated the analyticity of *arithmetic*—the subject studied by mathematicians throughout history— as opposed to demonstrating the analyticity of another logically equivalent theory.

Now we have a clearer view of what Frege’s aims and methods are, I propose to evaluate his derivation by asking the following questions:

- (Q1) Which axioms are needed to derive the formulae of *Begriffsschrift* necessary to demonstrate that arithmetic is analytic?
- (1.a) Are these really primitive truths of pure logic?



(1.b) Are they self-evident, like Frege's like Basic Law I:  $Q \rightarrow (P \rightarrow Q)$ ?

And recalling the discussion of fruitfulness in section 4:

(1.c) If the axioms are not self-evident but fruitful, why should we accept them as primitive truths?

Turning to Frege's definitions of the arithmetical primitives:

(Q2) Do the definitions in *Begriffsschrift* explicate the senses of their ordinary arithmetical correspondents accurately?

(2.a) Do the definiens include anything arbitrary or *ad hoc*?

(2.b) Do the definiens omit anything?<sup>62</sup>

I begin with Frege's definitions.

### 7. Assessing Frege's definition of number

Beginning with (Q2), the first issue is whether we should accept Frege's claim that the notion to be defined is 'the number which belongs to the concept *F*'. In this regard, Frege argues persuasively that a statement of number is an assertion about a concept (1884: §46). He then supplements his argument for this conclusion by pointing out that it also explains the generality of or "extensive applicability of number" (ibid: §48). This analysis of the use of number-expressions in ordinary language is persuasive.

Matters are complicated by Frege's conviction that numbers are objects, not concepts. This conviction is partly based on his observation that numbers are referred to with definite descriptions like 'the number 1', as well as in arithmetical statements like '2 is prime' and '1 + 1 = 2' (ibid: §57). But it is also based on a consideration relating to the general applicability of numbers, since it is in part because numbers themselves can be counted that Frege believes that

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<sup>62</sup> The distinction between (2.a) and (2.b) is emphasized helpfully by Burgess (2005).

they are objects. Further, since the need to account for this fact is one of the reasons that Frege introduces extensions, the next question we must answer is then, whether his definition of ‘the number which belongs to the concept  $F$ ’ as

the extension of the concept *equinumerous with the concept  $F$* ,

explicates the sense of its target notion accurately.

A point in Frege’s favor is that according to his definition, 0 does not have “second-rate” status as a pseudo-number, but is the extension of the concept *equinumerous with the concept not identical with itself*. Another point in Frege’s favor is that his definition of ‘ $P(m, n)$ ,’ preserves the thought that the predecessor of  $n$  is the number belonging to a concept under which falls *exactly one less thing* than falls under a concept to which  $n$  belongs. For example, since 5 belongs to the concept *surviving members of White Rhinoceros species as of June 2015*, the predecessor of 5 belongs to the concept *surviving members of White Rhinoceros species as of the present day*, under which falls exactly one less rhino, due to the recent death of Nabire at a zoo in the Czech Republic.

However, the following questions still need to be addressed:

(2.a) Do the definiens include anything arbitrary or *ad hoc*?

(2.b) Do they omit anything?

Beginning with (Q2.a), the obvious worry is that Frege’s definitions of ‘the number which belongs to the concept  $F$ ’, ‘0’ and ‘ $P(m, n)$ ’ do include content that is *ad hoc*. Firstly there is the notion of an extension, the use of which Frege says explicitly is something to which he “attaches no decisive importance” (ibid: §107). Secondly, there is the fact that instead of defining ‘ $\#x: Fx$ ’ so as to refer to the extension of the concept *equinumerous with  $F$* , one could instead define it to

refer to the extension of the concept *equinumerous with F or some sub-concept of F*.<sup>63</sup> This proposal is incompatible with Frege's, and yet there would seem to be no reason to favor one over the other. But this shows that the choice between the proposals is arbitrary and to this extent *ad hoc*.

I think that the following response is open to Frege. The sense of an expression is the objective condition to be its referent, which is discovered by analyzing how that expression is used in scientific discourse. Analysis does not reveal a condition that uniquely determines the referent of 'the number belonging to the concept *F*', but it does reveal a conjunction of conditions that all acceptable definitions must meet if they are to be sufficiently faithful to ordinary usage:

- (i) Numbers are objects which belong to kind-concepts
- (ii) For any number *n*, the predecessor of *n* is the number belonging to a concept under which falls *exactly one less thing* than falls under a concept to which *n* belongs
- (iii) HP

My proposal is that so long as Frege's definition meets these conditions, he can adopt a mathematician's indifference regarding exactly which definition he chooses. This proposal is analogous to the one in section 4 about how to establish which truths are primitive, which, it will be recalled, is that all acceptable variants of Frege's system must be founded on some subset of the same set of consistent truths, which are therefore understood to be the primitive truths. The analogy is that while there is some flexibility regarding the choice of both primitive truths and definitions, analysis reveals that there are conditions that all acceptable choices must satisfy.

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<sup>63</sup> Benaceraff (1965), Fine (2002).

I now turn to a more serious objection to Frege's definition, which is that it is in tension with his other theoretical commitments, as can be seen in the following way. The extension of the concept *equinumerous with the concept F* is the extension of a second-level concept, containing exactly those first-level concepts that are equinumerous with *F*. This presupposes that one can collect concepts into extensions, which in turn requires treating them as objects. The same presupposition is also made when Frege argues for the general applicability of number, by using the example of "counting concepts as well as objects" (1885: 1). But the claim that one can count concepts and collect them into extensions is at odds with Frege's claims that anything that one can denote with a complete expression is an object.<sup>64</sup> The problem is that if one can count concepts or collect them into extensions, then one can also denote them with a complete expression. But in that case, according to Frege, one treats them as objects, and so counts or collects the corresponding extensions instead. This is why, according to Frege's later proposal, ' $\#x: Fx$ ' refers to the extension of the first-level concept containing exactly those *extensions* that are equinumerous with *F*.

By making this move, Frege encounters another problem relating to the general applicability of numbers.<sup>65</sup> Numbers themselves can be counted, as Frege is fond of illustrating with the example of how many roots a given equation has. Suppose that 3 is a root of an equation *E*. Then 3 is included in the extension of the concept *being a root of E*. Further, since numbers can be counted, suppose that the number of roots of equation *E* is 3. By this supposition and by definition, the extension of the concept *being a root of E* is included in 3, since 3 is by definition the extension that includes all and only three-membered extensions. But then later Frege's extensions are non-well-founded in the most elementary way: one can have two extensions *x* and

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<sup>64</sup> Frege (1892b). This is the notorious "concept horse" problem. I will return to this in chapter 6.

<sup>65</sup> I learned of this objection from Kripke (forthcoming). I am not sure to whom it is due.

$y$  such that  $x$  is included in  $y$  and  $y$  is included in  $x$ . So later Frege must either give-up the claim that numbers can be counted, or the well-foundedness of extensions. This is not a problem for early Frege, since by definition his extensions contain *concepts* not extensions. But then early Frege must tell us how it is that we can count and collect concepts rather than their corresponding extensions.

Matters are complicated further by the fact that early Frege concedes that relational predicates that apply to extensions —such as ‘is wider than’ and ‘is included in’— *do not* apply to numbers (ibid: §69). This is especially clear in the case of 0, which is most certainly *not* included in 1. Since Frege concedes this point, and yet nevertheless proposes to identify numbers with things he calls “extensions,” it is somewhat tempting to conclude that he is using this term to indicate something that is not subject to the inclusion relations that logicians ordinarily associate with extensions, and thus should not be called “extensions.” Instead they might be labeled “extension<sup>#</sup>.” Could *later* Frege use this to block the inference, from the definition of ‘3’, to the conclusion that the extension of the concept *being a root of E* is included in 3? I don’t think so, since numbers, being members of extensions of concepts like *being a root of E*, must be included in other numbers, for otherwise they could not be counted and numbered. I will say no more about this objection for the moment, and will instead discuss a set-theoretic response to it, as well as the topic of well-foundedness, in chapter 4.

There is yet another argument showing that Frege’s definition is incorrect, by invoking modal considerations, which reflect Kripkean developments in semantics that are, to my knowledge, usually ignored in the philosophy of mathematics. These considerations make trouble for Frege, when taken together with the fact that for Frege, the referent of a numeral such

as ‘1’ is an extension, and this extension contains either extensions or concepts with extensions, *that in turn contain contingently existing objects*.<sup>66</sup> The argument can be stated as follows:

- (1) Extensions of concepts are individuated by their members.
- (2) Extensions of concepts contain the same members in every possible world in which they exist. (By 1.)
- (3) Extensions of concepts do not contain non-existent objects.
- (4) Actual extensions of concepts only exist in other possible worlds in which all of their actual members also exist (By, 2, 3.)
- (5) There is a possible world  $w$  in which Richard Carpenter does not exist.
- (6) The actual extension of the concept *surviving member of the Carpenters* does not exist in  $w$ . (By 4, 5.)
- (7) The actual extension containing all and only one-membered extensions (which I will refer to as ‘ $C$ ’) contains the actual extension of the concept *surviving member of the Carpenters*.
- (8)  $C$  does not exist in  $w$ . (By 4, 6, 7.)
- (9) The number 1 exists in  $w$ . (Assumption.)
- (10) The number 1 is not identical with  $C$  in  $w$ . (By 8, 9.)

Thus, on Frege’s definition, different entities are identical with 1 in different possible worlds.

This is excessively implausible, since 1 surely does not vary from world to world. Moreover, together with Frege’s doctrine that the sense of ‘1’ is the condition to be its referent, it commits

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<sup>66</sup> This argument is a slight modification of one due to Hambourger (1977). Salmon discovered a version of this argument independently, but has not published it.

him to the equally implausible consequence that ‘1’ refers to different entities in different possible worlds, and is consequently a non-rigid designator.<sup>67</sup>

One alternative to accepting these implausible results is to identify 1 with the same extension in all possible worlds, while denying that (9) and (5) can be true together. This is to deny that there exists a world in which 1 exists and Richard Carpenter does not exist. But since the choice of Carpenter was arbitrary, this is to deny that there exists a world in which 1 exists and some contingently existing object does not exist. This result is also excessively implausible. Of course one could sidestep the whole issue, if one could provide a set-theoretic definition of number, since *pure* sets do not contain contingently existing objects, but only other sets. But discussion of such definitions will have to wait until chapter 4.

Turning finally to (Q2.b), one might worry that when analyzing our ordinary concept of number, Frege over emphasizes the cardinal aspect of numbers while neglecting their ordering. John Burgess responds on Frege’s behalf that given the latter’s aims and methods, he has to privilege either cardinality or order and define it in logical terms, before using the privileged notion so-defined to introduce the other notion.<sup>68</sup> While this is certainly true, the fact is that *Frege simply cannot privilege cardinality, by using his definitions, while also establishing logicism*, because of the assumptions that he needs to derive that every number precedes some number, using his definitions. I will now remind the reader why at least one of these goals must be given up.

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<sup>67</sup> A rigid designator is a term which designates the same object  $x$  with respect to every possible world in which  $x$  exists. See Kripke (1980).

<sup>68</sup> Burgess (ibid)

### 8. The need for a non-logical axiom

Recall that Frege defines ‘ $\#x: Fx$ ’ and ‘ $P(m, n)$ ’ in such a way that if the number which belongs to the concept  $F$  is  $n$ , then its predecessor is the number which belongs to the concept *falling under  $F$  but not identical with some  $x$  that is  $F$* :

$$P(m, n) \equiv_{\text{df}} \exists F \exists x (Fx \wedge [\#y: Fy] = n \wedge [\#y: Fy \wedge y \neq x] = m)$$

Let me give an example, using “*being an  $F$* ” as a more succinct abbreviation of Frege’s “*falling under  $F$* .” For 7 to precede 8, there must exist a concept  $F$  such that there is an  $F$ , which we will call  $a$ , and the concept *being an  $F$  but not identical with  $a$*  has 7 objects falling under it. This requires that there are 8  $F$ ’s. For example, there must exist a concept such as *solar planet*, such that there exists a solar planet, e.g. Neptune, and the concept *being a solar planet but not identical with Neptune* has 7 objects falling under it. And happily, there are indeed 8 solar planets. The problem is that if every finite number  $m$  is to precede some number  $m+1$ , then there must exist a concept  $G$  that has  $m+1$  objects falling under it. But this requirement may not be fulfilled, because there may not be enough objects in the world, but only finitely many, and so not enough to meet this requirement. One option is to simply assume that there exist infinitely many objects by accepting an axiom of infinity, from which it would follow that there exists a concept  $G$  under which  $m+1$  objects fall.<sup>69</sup> But accepting this axiom is an admission of defeat for logicism, since it is not an axiom of logic. By using this axiom to derive that every finite number  $m$  precedes some number, one simply raises the question of what non-logical source of knowledge justifies the assumption that there exist infinitely many objects.

According to Frege’s Basic Law V

$$\text{extension}(\Phi) = \text{extension}(\Sigma) \leftrightarrow \forall x (\Phi x \leftrightarrow \Sigma x)$$

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<sup>69</sup> This observation is due to Russell (1911, 1919: Ch XIII).



every concept has an extension. In particular, the concept  $G$  to which  $m+1$  belongs, which is required for a finite number  $m$  to precede some number, allegedly has an extension. But as is well known, Basic Law V is not an axiom of logic, since it leads to Russell's paradox in Frege's system. This is because we can form the concept *being an extension of a concept that is not true of its own extension*, and ask whether this concept is true of its own extension, since if it is, it isn't, and if it isn't, it is.

To this various people have responded that Frege doesn't need to use Basic Law V in his sketch of how to derive formulae representing the axioms of arithmetic.<sup>70</sup> Rather, it is argued that the axioms are derivable from HP together with Frege's definitions, a fact known in the literature as Frege's Theorem. I will discuss this claim in the following chapter, but for now it will suffice to remind the reader that HP is not considered by Frege to be an axiom of logic (nor is it by Boolos), but a condition that any acceptable definition of ' $\#x: Fx$ ' must be shown to satisfy, by substituting the definition into HP and deriving the resulting sentence as a theorem. However, in order to do this Frege uses Basic Law V.<sup>71</sup>

Since this exhausts Frege's options, the answer to (Q1.a) is that he must accept an axiom that is not a primitive truth of pure logic. Again, Frege cannot privilege cardinality, by using his definitions, while also establishing logicism. It follows that at least one of these goals must be

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<sup>70</sup> See Boolos (1998: Ch. 11-13), Heck (2011), Parsons (1965) and Wright (1983).

<sup>71</sup> After Frege substitutes his definition of number into HP, he tries to prove that if the  $F$ 's and the  $G$ 's are equinumerous, then the extension of *equinumerous with  $F$*  = the extension of *equinumerous with  $G$* . (He does not prove the left to right direction but mentions that it can be proven.) Frege proceeds by assuming that the  $F$ 's and the  $G$ 's are equinumerous, and then picking an arbitrary concept  $H$ , to show that  $H$  is a member of the extension of *equinumerous with  $F$*   $\leftrightarrow$   $H$  is a member of the extension of *equinumerous with  $G$* . This assumes that to show that the extension of *equinumerous with  $F$*  = the extension of *equinumerous with  $G$* , it suffices to show that these extensions have the same members, i.e that  $\forall X (FX \leftrightarrow GX)$ , where  $X$  is a variable ranging over first-level concepts. But this is just an application to extensions of second-level concepts of Basic Law V: extension( $F$ ) = extension( $S$ )  $\leftrightarrow$   $\forall x (Fx \leftrightarrow Sx)$ . See Frege (ibid: §73).

given up. But it still leaves open the possibility that one can continue Frege's progress while *only* giving up the goal of establishing logicism, by attempting to explain arithmetic's general applicability in terms of something non-logical but still sufficiently general. This is the line that I will pursue in the following chapters.

I will close by recapping the objections to Frege's definition of ' $\#x: Fx$ '. Firstly, early Frege's proposal, that ' $\#x: Fx$ ' refers to the extension of the second-level concept containing exactly those first-level concepts that are equinumerous with  $F$ , is undermined by the fact that counting concepts and collecting them into extensions forces one to treat concepts as objects, and so to count and collect the corresponding extensions. Secondly, later Frege's proposal, that ' $\#x: Fx$ ' refers to the extension of the first-level concept containing exactly those extensions that are equinumerous with  $F$ , is undermined by the fact that such extensions are non-well founded. Thirdly, Frege's doctrine that the sense of ' $\#x: Fx$ ' is the condition to its referent, commits him to the implausible view that ' $\#x: Fx$ ' refers to different entities in different possible worlds. Finally, the view that numbers are essentially quantifiers is in tension with the goal of establishing logicism, since it requires that there are infinitely many individuals.

### Chapter 3: Frege's theorem

#### 1. Introduction

Second-order logic is an augmentation of first-order logic that quantifies over properties and relations as well as objects. What is sometimes called “Frege Arithmetic” is an augmentation of second-order logic with HP as a non-logical axiom. Second-order *arithmetic* is a formal theory that characterizes the natural numbers as any progression satisfying the usual Dedekind-Peano axioms. Here I follow Richard Heck's presentation,<sup>72</sup> using ‘ $P$ ’ to facilitate comparison with Frege's ‘ $P(m, n)$ ’, and using ‘ $N$ ’ for ‘natural number’. The axioms are then:

$$(1) N0$$

$$(2) Nx \wedge Pxy \rightarrow Ny$$

$$(3) \forall x \forall y \forall z (Nx \wedge Pxy \wedge Pxz \rightarrow y = z)$$

$$(4) \forall x \forall y \forall z (Nx \wedge Ny \wedge Pxz \wedge Pyz \rightarrow x = y)$$

$$(5) \sim \exists x (Nx \wedge P(x, 0))$$

$$(6) \forall x (Nx \rightarrow \exists y Pxy)$$

$$(7) \forall F (F0 \wedge \forall x \forall y (Fx \wedge Pxy \rightarrow Fy) \rightarrow \forall x (Nx \rightarrow Fx))$$

Frege's Theorem is that correspondents of these axioms can be derived, in Frege Arithmetic, using the following definitions of the arithmetical primitives (due to Frege):<sup>73</sup>

$$0 =_{\text{df}} \#x: x \neq x$$

$$P(m, n) \equiv_{\text{df}} \exists F \exists x (Fx \wedge [\#y: Fy] = n \wedge [\#y: Fy \wedge y \neq x] = m)$$

$$\text{Natural Number}(n) \equiv_{\text{df}} P^{*\text{=}}(0, n)$$

<sup>72</sup> Heck (2011). Heck's work is a refinement of work by Boolos and others. See for example Boolos (1998) and Wright (1983).

<sup>73</sup> Regarding the following definitions, it will be recalled from the previous chapter that  $P^{*\text{=}}$  is the weak ancestral of  $P$ .

Note that ‘ $\#x: Fx$ ’ is an undefined primitive in Frege Arithmetic, because no specific definition of it can be given by means of HP, given that we accept Frege’s constraints on definitions (see section 6 of the previous chapter). For this reason, Heck attempts to argue for the philosophical significance of Frege’s Theorem by establishing the relevance of HP to our ordinary concept of number, rather than arguing from the accuracy of a proposed definition of ‘ $\#x: Fx$ ’.

After discussing the status of HP as a primitive truth, I will argue that Heck fails in his attempt, in part because he falls into the same anthropological trap as Blanchette, of basing his views on an overly anthropological approach to conceptual analysis, and a failure to distinguish between different degrees and kinds of understanding. Turning to the comprehension axioms of Frege Arithmetic, I will show that the cost of deriving Frege’s Theorem using predicative comprehension is that Heck has to take ‘ $P(m, n)$ ’ as well as ‘ $\#x: Fx$ ’ as primitive. This further undermines the philosophical significance of his derivation. The alternative that I think Heck should pursue is to use impredicative comprehension. However, I argue, impredicative comprehension axioms are not primitive truths of pure logic, since they presuppose concepts from combinatorics and set theory.

## 2. Is HP a primitive truth?

I begin with the question of whether HP is a plausible candidate to be a primitive truth. Following the presentation of the previous chapter, the questions to be addressed are as follows:

(Q1.a) Is HP a primitive truth of pure logic?

(Q1.b) Is it self-evident?

(Q1.c) If it is not self-evident, why should we accept it as a primitive truth?

Regarding (Q1.a), George Boolos argues, convincingly, that while logical truths are true in all models, HP is false in a model with a finite domain, and so is not a logical truth.<sup>74</sup> With that said, I turn to the question of whether HP is self-evident. There is a temptation to say that it is, because it can be accepted on the basis of being fully understood, which seems sufficient for a proposition to be self-evident. However, as I will show, the requirements for fully understanding HP are high enough to make one worry that it is *not* self-evident. This, I will claim, together with the mathematical considerations that recommend HP, suggest that it is better thought of as justified by the method of reflective equilibrium.

The issue is that Frege is following Cantor, whom he cites, in intending HP to be true of the sizes of infinite sets as well as finite ones. However, if HP is construed in this way, then the requirements for fully understanding it are high enough that it cannot be fully understood by the folk. This is because the folk, prior to being taught otherwise, accept Euclid's fifth common notion that the whole is greater than the part,<sup>75</sup> and, correspondingly, expect the number of rooms in an infinite hotel to be greater than the number of even-numbered rooms, because the latter set is a proper part of the former. But by HP, the set of natural numbers is of the same cardinality as the set of even numbers, because the natural numbers and the even numbers are equinumerous. More generally, by HP, an infinite set can be of the same as cardinality as one of its proper subsets. Further, given that HP has this consequence, if one is to fully understand it, one must understand that an infinite set can be of the same as cardinality as its proper subsets, and so make a significance conceptual advance in one's understanding of the concept of cardinal number.<sup>76</sup> (Of course the same point applies to the axiom of infinity, since to understand it one must

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<sup>74</sup> Boolos (*ibid*)

<sup>75</sup> Euclid (*ibid*, *Book I*)

<sup>76</sup> I now use 'concept' in accordance with modern usage to mean something like 'sense.'

understand that the number of rooms in an infinite hotel is of the same cardinality as the number of even-numbered rooms. I will return to this axiom in chapters 4 and 6.)

It is helpful here to contrast the folk's partial understanding of Cantor's concept of number with their understanding of 'finite number.' In the latter case they *are* competent to number "the ideal as well as the real, concepts as well as objects" (Frege: 1885: 1), but merely unable to articulate the condition that explains this. In contrast, prior to learning about Cantor's insights into the concept of number, they are *not* competent to apply it correctly to all cases, as shown by their expectation about the infinite hotel.

To one who is inclined to accept both HP and Euclid's fifth common notion, the situation appears paradoxical, since both principles seem acceptable and yet they cannot be accepted together.<sup>77</sup> The solution is to recognize that one is missing something significant by cleaving to the latter principle, and to consequently reject it while continuing to accept HP, and thus that an infinite set can be put in one-to-one correspondence with one of its proper subsets. Phillip Kitcher argues that the reward for doing so is

an explanatory generalization of finite arithmetic. Note first that the ordinary notions of order among numbers, addition of numbers, multiplication of numbers, and exponentiation of numbers are extended in ways which generate theorems, analogous to those of finite arithmetic... By contrast, because he cleaves to the intuitive idea that a set must be bigger than any of its proper subsets, Bolzano is unable to define even an order relation on infinite sets. The root of the problem is that, since he is forced to give up the thesis that the existence of one-to-one correspondence suffices

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<sup>77</sup> Galileo was one of the first to note this. See (1638/1954).

for identity of cardinality, Bolzano has no way to compare sets with different members (1984: 211).

Thus, by abandoning Euclid's fifth common notion for HP, we discover something about the concept of cardinal number of which prior mathematicians were unaware. Further, while this *appears* to be in tension with Frege's picture of temporally distant mathematicians investigating the same senses or concepts, Cantor's concept of number remains continuous with the one that was studied by prior mathematicians, while being more generally applicable. In particular, it extends Frege's observation that anything can be numbered, since by Cantor's denumerability results this observation applies not only to *discrete* collections of objects of a given kind, but also to subsets of real numbers. This provides theoretical reason to accept HP. Crucially, however, this theory can be bought into accord with our intuitive judgments somewhat, since it is an extension of Frege's intuitively plausible observation.

Having said all that, is HP self-evident? I suppose there are grounds for insisting that it is, because it should be accepted on the basis of being fully understood. However, since this in turn requires a full understanding of our concept of cardinal number and how it generalizes, which is acquired via the line of reasoning described in the previous few paragraphs, I am more inclined to say that HP is justified by the method of reflective equilibrium.<sup>78</sup> That is, HP should be accepted because of the above theoretical considerations, which can be bought into accord with our intuitive judgments.

Another response suggests itself. Arguably a restricted version of HP is self-evident, because it can be accepted without making the conceptual advance that is required of us by Cantor. This response has been developed by Heck, who appeals to our practice of counting in

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<sup>78</sup> Goodman (1955).

order to restrict HP, so that it only applies to the sizes of finite sets and does not have the aforementioned counter-intuitive consequence. Heck argues that such a restriction can be discovered by reflection on the practice of counting, because, intuitively, finite sets are just those that can be counted:

[T]he intuitive notion of a finite concept is that of one the objects falling under which can be *counted*, i.e., enumerated by a means of some process which eventually terminates (2011: 248).

Further, Heck claims, counting so conceived is governed by an application of HP. To show this, Heck appeals to an argument given by Frege in response to Husserl, whom Frege characterizes as advocating a theory of counting which he believes gets the cart before the horse:

The simplest criterion for equality of number is just that *the same* number results from our counting the sets to be compared.” Naturally; Just as the simplest test whether for a right angle is to use a set square! The author forgets that this very counting depends on a one-one correlation —namely between the numerals *1* to *n* and the objects in the set. Each of the two sets has to be counted. This makes the matter less simple than it is if we consider a relation that correlates the two sets with one another without numerals as intermediaries (1894: 319)

According to Heck, Frege’s point is as follows. The *F*’s and the *G*’s are assigned the same numeral and so the same number by counting, if and only if they are equinumerous - a clear application of HP. For if the *F*’s and the *G*’s are both assigned the same number by counting, then they are equinumerous with the same segment of numbers and so equinumerous with each other. And if the *F*’s and the *G*’s are equinumerous, then each collection is equinumerous with a segment of the numbers; and since the collections themselves are equinumerous, so are these



segments, which are thereby identical. Further, Heck reasons, since no infinite plurality can be assigned a number by a process which eventually terminates, the principle governing counting is really Finite HP (FHP):

$$\text{Finite}(F) \vee \text{Finite}(G) \rightarrow \#x: Fx = \#x: Gx \leftrightarrow \text{equinumerous}(F, G)$$

Heck then proceeds to prove a version of Frege's Theorem, by deriving the axioms of second-order arithmetic from a formalization of FHP in second-order logic. It is crucial to note however, that this version of Frege's Theorem will not establish that the axioms can be justified without appeal to intuition, and thereby explain the general applicability of numbers, unless the aforementioned intuitive notion of 'finite' can be defined without appeal to any other intuitive notion.

In the next section I want to discuss why Heck, in response to the objection that HP is not self-evident, eventually opts to pursue a different line than this one.

### 3. Just as many

Heck, in more recent work, distinguishes between HP in its original unrestricted form, which he holds to be a non-self-evident truth about cardinality generally, and another logically equivalent principle, which he claims is self-evident. Once again, the latter principle is claimed to be self-evident because it can be understood by reflection on our ordinary concept of cardinal number. But crucially, Heck argues that unlike FHP, neither the principle nor the ordinary concept of cardinal number to which it corresponds, have *any conceptual connection to counting or to one-to-one correspondence*. Before I describe this proposal, it will be helpful to remind the

reader of the various conditions that have been proposed to be requirements for transitive counting.<sup>79</sup>

The first condition is that the symbols in the count list must be recited once and only once in a stable-order. The second is that a one-to-one correspondence must be established between the symbols in the count list and the objects counted. The third is that the order in which the objects are correlated one-to-one with the symbols must not affect the outcome of the count, where the outcome is the last symbol correlated with the objects counted. The fourth is that one must be able to give the final symbol correlated in answer the question ‘how many?’. And the fifth condition is that anything that can be bought under a kind-concept (in accordance with Frege’s usage of ‘concept’) can be counted. With these conditions in mind, I now turn to Heck’s proposal.

Heck argues that although counting depends *logically* on one-to-one correspondence, it does not depend *conceptually* on this. What he appears to mean by this is that although counting requires *establishing* one-to-one correspondence, it does not require “understanding of one-to-one correspondence and its bearing upon questions of cardinality” (2011: 166). To motivate this claim, Heck points to the fact that one can count the F’s by establishing a one-to-one correspondence, without having to think of numerals as objects that are being corresponded with the F’s, or having to understand that a one-to-one correspondence between the numerals and the F’s is what one is establishing. Rather, Heck claims, establishing such a correspondence simply requires understanding that one should recite each numeral once and only once in a stable order, while looking at or demonstrating the objects counted. Heck is explicit that the relevant kind of

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<sup>79</sup> Gallistel and Gelmann (1978).

understanding is supposed to be conscious, rather than unconscious but consciously accessible by reflection.<sup>80</sup>

Heck also claims that counting does not depend conceptually on our concept of cardinal number, by which he means that counting does not require understanding its cardinal significance. To motivate this claim, Heck points to the evidence cited in chapter 1, that there is a stage during development when children can be said to count small pluralities in the sense of meeting the above four conditions, without understanding the cardinal significance of what they have done. This is shown by the fact that when instructed to ‘Give me  $m$  F’s’ after counting, where  $m$  is the last numeral recited, children give the experimenter a random number of F’s.<sup>81</sup> Heck thinks this shows that “mastery of the practice of (transitive) counting is compatible with one’s having no concept of cardinality at all” (ibid: 169).

Having argued that counting does not depend conceptually on one-to one correspondence or on our concept of cardinal number, Heck goes on to argue that our concept of cardinal number does not depend conceptually on counting, by which he appears to mean two things (ibid: 168-71). Firstly, that counting by meeting the aforementioned conditions is not necessary for assigning specific cardinals to pluralities, because in the case of small pluralities this can be done by *subitizing*: by immediately recognizing how many there are without counting (as discussed at length in chapter 1). Secondly, he claims that counting as characterized by Frege is neither necessary nor sufficient for assigning specific cardinals to pluralities. To see what Heck has in mind here, consider again another of Frege’s remark on counting, from the *Grundgesetze*:

[W]hen we count the objects falling under a concept  $\Phi(\xi)$ , we correlate these with the number-signs, one after the other, beginning with ‘One’ up to that number-sign ‘ $N$ ’

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<sup>80</sup> See Heck (ibid: fn. 16, 20 & 22).

<sup>81</sup> Wynn (1992).

which is determined by the correlating relation mapping the concept  $\Phi(\xi)$  into the concept “member of the series of number-signs from ‘One’ to ‘ $N$ ’ and the converse relation mapping the latter into the former. ‘ $N$ ’ then designates the desired cardinal number; i.e.,  $N$  is that cardinal number (1893: §108).

As Heck reads him, Frege is arguing as follows (I will use ‘ $F$ ’ for ‘ $\Phi(\xi)$ ’). Assume the objects falling under  $F$  are in one-to-one correspondence with the numerals ‘1’ through ‘ $n$ ’. Then, by HP, the number of  $F$ ’s = the number of numerals ‘1’ through ‘ $n$ ’. Further, the number of numerals ‘1’ through ‘ $n$ ’ = the number denoted by ‘ $n$ ’. So, by transitivity of ‘=’, the number of  $F$ ’s = the number denoted by ‘ $n$ ’.

According to Heck, counting so characterized is not necessary for the assignment of cardinals to pluralities, because “it presupposes a conception of numerals as objects that the ability to count, and even to make judgments of cardinality, does not require” (ibid: 175). To reiterate an earlier point in a slightly different context, one can understand ‘there are  $n$   $F$ ’s’ without having to think of the numerals as objects that are being correlated one-to-one with the objects falling under  $F$ , and without having to think that the number of numerals ‘1’ through ‘ $n$ ’ = the number denoted by ‘ $n$ ’.

Further, Heck argues, counting so characterized is not sufficient for assigning specific cardinals. To support this claim, Heck appeals to an argument due to Kripke—to be discussed at length in the following chapter—which Heck characterizes as follows. Assume one is able to recite the binary numerals in order, and is able to put the objects falling under  $F$  in one-to-one correspondence with the binary numerals ‘1’ through ‘11011’. Then, by HP, the number of  $F$ ’s = the number of binary numerals ‘1’ through ‘11011’. Further, the number of binary numerals ‘1’ through ‘11011’ =  $11011_2$ . So, by transitivity, the number of  $F$ ’s =  $11011_2$ . Now repeat the

same reasoning with decimal. It simply cannot be that knowing how many  $F$ 's there are is, as Frege claims, knowing that the number of  $F$ 's = the number of decimal numerals '1' through '27', for then one would also have knowledge of how many  $F$ 's there are by counting with binary numerals. The problem, according to Heck, is that counting, as Frege characterizes it, does not explain the apparent epistemic difference between assigning cardinals with the decimal system and doing so with other numerical systems like binary that we may also be well practiced at reciting in order. (One might object that there is no *genuine* epistemic difference, while attempting to explain the *apparent* epistemic difference in other terms. I will return to this in the next chapter.)

To take stock, Heck argues that counting does not depend conceptually on one-to-one correspondence or on our concept of cardinal number, and that our concept of cardinal number does not depend conceptually on counting. So what else could our ordinary concept of cardinal number depend on, if not counting? Heck claims that it depends conceptually on something coextensive with but conceptually distinct from one-to-one correspondence, namely a relation he calls *just as many*:

One will understand answers to how many questions *as* answers to how many questions —*as* ascriptions of number, rather than statements about the results of countings— only if one grasps the concept *just as many* and its relation to ascriptions of number (ibid: 172).

To motivate this, Heck argues that this relation is plausibly part of the conscious conceptual apparatus of children. For supposing that there are just as many cookies as children:

Then it follows that there are *just* enough cookies for each child to have one. From a logical point of view, that means that there is a one-one correspondence between the

cookies and the kids; but from the child's point of view, it need mean no more than that, if you start giving cookies to children, and you're careful not to give another cookie to anyone to whom you've already given one, then you won't run out, though you won't have any left (ibid: 171).

Heck codifies his claim that our concept of number depends conceptually on *just as many* using the following coextensive modification of HP, which he calls HPJ:

$\#x: Fx = \#x: Gx \leftrightarrow$  there are just as many  $F$ 's as  $G$ 's.

One might think that the distinction between this and HP is one without a difference. However, regarding HPJ, Heck claims that *unlike* FHP or HP:

An appreciation of the connection between sameness of number and equinumerosity that [HPJ] reports is essential to even the most primitive grasp of cardinal number (ibid: 176).

But exactly what is supposed to have been accomplished by supplementing HP with the coextensive HPJ?

For Heck, the interest of Frege's Theorem is that a theory that characterizes the natural numbers as any progression satisfying (1) – (7), is second-order logically entailed by HP and Frege's definitions, which characterize numbers as cardinals that apply to kind-concepts and that are identified in terms of equinumerosity. Heck also wants to show that HP *explains why* the axioms hold. Further, for it to do this he acknowledges that HP

does need to be *more fundamental*, in some significant way, than the axioms of PA if Frege's Theorem is to have any explanatory force, for not every derivation of a conclusion from premises counts as *explaining*, in terms of the premises' holding,

why the conclusion holds: And if HP itself were *less* fundamental than the axioms of PA, the explanatory value of Frege's Theorem would be nil (ibid: 179).

But the problem is that due to Cantor's influence, the concepts of number and of one-to-one correspondence contained in HP is "very sophisticated" (ibid: 170). Further, the supposition that FHP governs counting cannot ameliorate this problem in Heck's view, since he has argued that counting is conceptually independent of our ordinary concept of number. However, he thinks that the concept of there being *just as many* is conceptually necessary to our most elementary concept of number. Thus HPJ, in addition to being both logically equivalent to and similar in content to HP, is argued to be self-evident, since it can be understood by reflection on our most elementary concept of number. So it is supposed to be "more fundamental" than axioms of arithmetic. Putting axiom (6) to one side, since it can be derived from axiom (3), Heck concludes:

What I have argued here is that recognition of the truth of something *very much like* HP is required if one is even to have a concept of cardinality. If so, then what Frege's Theorem shows is that the fact that the finite cardinals form an initial segment of an  $\omega$ -sequence [i.e. satisfy the axioms (1) – (5) and (7)] is implicit in our very concept of cardinal number—'implicit' in the sense that their forming such a sequence is logically required by the character of our concept of cardinal number (ibid: 179).

Thus, Frege's Theorem is argued to offer a philosophical explanation of why the finite cardinals satisfy axioms (1) – (5) and (7), and so form an initial segment of an  $\omega$ -sequence. They do so because these axioms follow from a self-evident principle that the finite cardinals also satisfy, HPJ being a self-evident principle about our ordinary concept of cardinal number.

I now turn to my criticisms of Heck's line of reasoning. In what follows I will not address Heck's complaint that counting as Frege characterizes it does not suffice to assign specific cardinals to pluralities because its results are not, in Kripke's words "immediately revelatory," since this will be discussed in the next chapter. Rather, I will focus on his argument that our ordinary concept of number depends conceptually on HPJ rather than FHP. As will become clear, the essence of my disagreement with Heck is similar to the essence of my disagreement with Blanchette. I believe that Heck's judgments regarding what is overly sophisticated, and so inessential to our ordinary concept of number, are based on an overly anthropological approach to conceptual analysis.

First of all, I do not accept Heck's argument that counting does not depend conceptually on either one-to-one correspondence or our concept of number. Here once again is it helpful to distinguish between competence understanding—the minimal, consciously accessible knowledge of the sense of an expression that is required to use that expression correctly—and reflective understanding, as well as between *partial* and *full* competence understanding (see chapter 1 section 12 and chapter 2 section 6). Of course children and many adults do not have reflective understanding of 'counting', being neither conscious of nor able to articulate all of the requirements for counting. Further, children and some adults only have *partial* competence understanding of 'counting,' for example by knowing that they should obey the once-and-only-once rule, but not knowing that they should also obey the one-to-one correspondence rule. If so, then *partial* competence understanding of 'counting' does not require knowledge of the one-to-one correspondence rule. But this is only a stage in the development of *full* competence understanding, which does require one to have consciously accessible, although not conscious knowledge of the one-to-one correspondence rule. If so, then one may agree with Heck that we



need not be consciously aware of there being a one-to-one correspondence between numerals and objects, while also insisting that one-to-one correspondence is an essential conceptual ingredient of the concept of counting, one that is found not in what we are conscious of but *by reflection on our practice*.

For the same reason I do not accept Heck's argument that counting does not depend conceptually on our concept of cardinal number, i.e. does not require understanding its cardinal significance. For the fact that infants —and the clinical patients mentioned in chapter 1— can count in an overlearned fashion without understanding the cardinal significance of what they have done, does not show that “mastery of the practice of (transitive) counting is compatible with one's having no concept of cardinality at all” (ibid: 169). Rather, *partial* competence understanding of ‘counting’ is compatible with having no concept of cardinality or of the cardinal significance of counting, while *full* competence understanding does require this.

Nor do I accept either of Heck's arguments that counting is not necessary for our concept of cardinal number. One of these was that counting, as characterized by Frege in terms of one-to-one correspondence, presupposes thinking of numerals as objects in a way that assigning cardinals does not require. But once again, one may agree with Heck that we need not be consciously aware of the status of numerals as objects, while also insisting that this status is an essential conceptual ingredient of the concepts of counting and cardinality.

I now turn to Heck's other argument that counting is not necessary for our concept of cardinal number: that cardinals can be assigned by immediately *recognizing* how many things there are in a plurality without counting. The problem with this argument is that, as I showed in chapter 1, this sort of recognition only guides the assignment of cardinals to pluralities *on the*

*assumption that the subject already has concepts of specific cardinals*, and it remains to be shown that acquiring such concepts does not require counting.

A related problem is that Heck's argument that our ordinary concept of number satisfies HPJ but not FHP, involves a fallacious inference to a conclusion about our concept of number from evidence about how we acquire it. This is because Heck appeals to evidence concerning children, who may still be in the process of learning the concept of number, with the result that what they have learned so far is not sufficient to understand the concept that is understood by adults. The point is that even if the connection between sameness of number and *just as many* that HPJ reports is something that the child consciously recognizes, while the connection between sameness of number and one-to-one correspondence is not, the latter connection may nevertheless be essential to the adult conception. Obviously this objection is reminiscent of Frege's polemic against Mill's "pebble or gingerbread arithmetic" (1884: v).

The conclusion that I draw from the previous four paragraphs is that Heck has not succeeded in establishing that HPJ is a self-evident principle about the ordinary concept of number that is understood by adults; so he is not justified in appealing to HPJ to ensure the explanatory value of Frege's Theorem. Further, he has not succeeded in undermining the view to which I am sympathetic, that counting requires the establishment of a one-to-one correspondence between *numbers* and objects counted, and that one-to-one correspondence is fundamental to our concept number as well as of counting. I will say more about this view in the final chapter.

As we will see in the next section, there are further objections to the claim that Frege's Theorem has the kind of explanatory value that Heck claims for it. Before I turn to these objections, I must describe the other axioms of Frege Arithmetic, namely those of second-order

logic, and summarize which of the axioms are needed to derive axioms (1) – (7) of second-order arithmetic.

#### 4. Comprehension axioms

The expressive power of second-order logic is obtained by laying down comprehension axioms, which are axioms stating that a formula  $\Phi$  defines a second-order entity such as a relation, concept or class in the domain of second-order variables. Comprehension axioms have the following form:

$$\exists P \forall x_1 \dots x_n [Px_1 \dots x_n \leftrightarrow \Phi(x_1 \dots x_n)]$$

A comprehension axiom is *impredicative* just in case  $\Phi$  contains at least one bound second-order variable, and *predicative* otherwise. To give an example of an impredicative axiom, consider the relation of identity expressed by ‘ $x = y$ ’, which can be defined using Leibniz’s law of the identity of indiscernibles in the following axiom:

$$\exists R \forall x \forall y [Rxy \leftrightarrow \forall X (Xx \rightarrow Xy)]$$

Since ‘ $X$ ’ ranges over all properties and relations,  $X$  can have the value *is identical with y*. So the relation expressed by ‘ $x = y$ ’ is defined by quantifying over a totality that includes the relation to be defined. To give another example, consider Frege’s definition of ‘natural number’ in terms of the weak ancestral of the immediately preceding relation:

$$\text{Natural Number}(n) \equiv_{\text{df}} P^{*\ast} (0, n)$$

By this definition, a natural number is something that falls under *every* hereditary concept that 0 falls under (using ‘concept’ as Frege does). Further, *natural number* is itself a hereditary concept that 0 falls under. So ‘natural number’ is defined by quantifying over a totality that includes the

entity to be defined. This is the essential feature of impredicative definitions, informally speaking.<sup>82</sup>

Full second-order logic has an axiom for every formula containing at least one bound second-order variable. At the other end of the spectrum, predicative second-order logic has an axiom for every formula containing *no* bound second-order variables. Suppose  $\Phi$  is such a formula; then  $\Phi$  is called a  $\Sigma^1_0$  formula. Further, a formula in which  $\Phi$  occurs, of the form

$$\exists X_1 \dots X_n \Phi,$$

with all its occurrences of second-order quantifiers occurring in a block at the beginning of the formula is called a  $\Sigma^1_1$  formula. (In general, if  $\psi$  is  $\Sigma^1_n$ , then

$$\exists X_1 \dots X_n \psi$$

is  $\Sigma^1_{n+1}$ .) So-called  $\Sigma^1_1$  second-order logic has a comprehension axiom for every  $\Sigma^1_1$  formula, and is thus impredicative. (Corresponding definitions can be given for universal quantifiers by replacing ‘ $\Sigma$ ’ with ‘ $\Pi$ ’.)

I now turn to the question of which axioms of Frege Arithmetic are needed to derive axioms (1) – (7) of second-order arithmetic (for these axioms see section 1 of this chapter). Heck reports that axioms (1) and (2) follow from Frege’s definitions using only predicative comprehension, and that axioms (3), (4) and (5) follow from HP, also using only predicative comprehension. However, according to Heck,  $\Sigma^1_1$  comprehension is required to derive axiom (6) that every number precedes some number, from axiom (3). It is also required to derive concerning the operations of addition (and multiplication) that every pair of numbers has a sum

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<sup>82</sup> Another point that the second example brings out is that one will accept an impredicative definition of an entity more readily, if one is already a realist about that entity.

(product) that is a number.<sup>83</sup> This is because one must derive these propositions by induction on predicates containing suitably Fregean definitions of ‘ $P(m, n)$ ’, ‘+’ and ‘ $\cdot$ ’. These in turn contain occurrences of second-order existential quantifiers at the beginning and so are expressed by  $\Sigma^1_1$  formulae.<sup>84</sup> For example, recall that Frege’s definition of ‘ $P(m, n)$ ’ as it occurs on the right hand side of the relevant comprehension axiom is:

$$\exists F \exists x (Fx \wedge [\#y: Fy] = n \wedge [\#y: Fy \wedge \sim(y = x)] = m)$$

To give another example, Frege’s informal definition of ‘the sum of  $k$  and  $m$  is  $n$ ’ must be something like:

There exist concepts  $F$  and  $G$  such that the number which belongs to the concept  $F$  is  $k$ , and the number which belongs to the concept  $G$  is  $m$ , and no object falls under both  $F$  and  $G$ , and the number which belongs to the concept  $F$  or  $G$  is  $n$ .

Using modern notation, this will have to be formalized in something like the following way:

$$\exists F \exists G ([\#x: Fx] = k \wedge [\#y: Gy] = m \wedge \sim \exists z (Fz \wedge Gz) \wedge [\#u: Fu \vee Gu] = n)$$

Clearly these are both  $\Sigma^1_1$  formulae, requiring impredicative  $\Sigma^1_1$  comprehension. Finally, Frege’s definition of the ancestral relation is also  $\Pi^1_1$ , with the result that the derivation of the induction axiom (7) requires  $\Pi^1_1$  comprehension.

##### 5. Does Frege’s theorem require predicative or impredicative comprehension?

I am puzzled by Heck’s claim that axioms (3), (4) and (5) follow from HP using only predicative comprehension, given that the right hand side of HP quantifies over a relation:

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<sup>83</sup> Heck (2011: 162-3).

<sup>84</sup> Burgess shows that one *can* get these results using only the resources of predicative comprehension, but doing so requires one to adopt the kind of *ad hoc* definition of ‘number’ that we are trying to avoid. See Burgess (ibid).

$$\text{Equinumerous}(F, G) \equiv_{\text{df}} \exists R [\forall x \forall y \forall z \forall u (Rxy \wedge Rzu \rightarrow x = z \leftrightarrow y = u) \wedge \forall x (Fx \rightarrow \exists z (Gz \wedge Rxz)) \wedge \forall z (Gz \rightarrow \exists x (Fx \wedge Rxz))]$$

Heck does not have anything to say about this fact. However, Øystein Linnebo —with whom Heck shares the credit for establishing how much second-order arithmetic can be derived using predicative comprehension— makes the following remark:

It may seem problematic that this theory allows the N-operator to occur in comprehension formulas that are supposed to be predicative. For the implicit definition of this operator relates it to [the above] formula that quantifies over relations. However, the restriction to predicative comprehension is compatible with a stepwise extension of the language in which the comprehension formulas are given as we come to understand new expressions. Now, on the view in question, HP is solely responsible for fixing the meaning of the N-operator. It therefore makes sense to allow the operator to occur in predicative comprehension formulas (2004: fn. 9).

I find this argument unclear. Why is the restriction to predicative comprehension compatible with such an extension of the language? In any case, what *is* clear is that Linnebo is assuming that HP fixes the meaning of ‘#x: Fx’. However, in my view ‘#x: Fx’ is an undefined primitive in the system under discussion, because no specific definition of it can be given by means of HP, assuming that we accept Frege’s constraints on definitions. Rather, HP only places *constraints* on how the language is to be extended (as I argued at the end of section 6 of the previous chapter). In which case, HP does not justify the occurrence of ‘#x: Fx’ in predicative comprehension formulae in the way that Linnebo claims. For this reason, I am suspicious of the claim that axioms (3), (4) and (5) follow from HP (or from FHP or HPJ) using only predicative

comprehension. That said, I will now argue that a similar problem afflicts Heck's attempt to avoid using impredicative comprehension to derive axiom (6).

Heck is prompted to argue that (6) is derivable using only predicative comprehension in response to a criticism due to Linnebo, who argues that Frege's definition of ' $P(m, n)$ ' cannot be accurate, given that induction on it requires  $\Sigma^1_1$  comprehension, since

it is hard to see why [second-order induction axioms] talking about such a basic arithmetical relation as succession should depend on impredicative comprehension...

In contrast, on the alternative view that regards the natural numbers as ordinals, the successor relation is primitive, and addition and multiplication are predicatively defined in terms of it by the standard recursion axioms. So on this view we trivially get induction on formulas talking about succession, addition, and multiplication without invoking impredicative comprehension (ibid: 172-3).

As Heck reads it, Linnebo's objection concerns the logical complexity of Frege's definition.

Heck writes:

Øystein Linnebo (2004: 172-3) suggests that there is something seriously wrong with Frege's definition of predecessor. It simply does not seem reasonable to suppose that a notation as simple as that of predecessor should be logically so complex (2011: 271).

In my view, if Linnebo's objection is read this way, then it has no force, because it is based on the overly anthropological approach to conceptual analysis that I have already criticized in section 3. (But of course Heck himself takes this anthropological approach, so it is unsurprising that he takes Linnebo's objection so seriously.) To see this, it will be helpful to recall my partial defense of Frege's definitions. In my view, the sense of an expression is discovered by analysis

of how that expression is used in scientific discourse. In the case of ' $P(m, n)$ ', such analysis reveals a condition that all acceptable definitions must meet if they are to be sufficiently faithful to ordinary usage: that for any number  $n$ , the predecessor of  $n$  is the number belonging to a concept under which falls *exactly one less thing* than falls under the concept to which  $n$  belongs. This is the condition of which the folk have competence understanding, without thereby being able to articulate it in an analysis. Furthermore, Frege's definition of ' $P(m, n)$ ' meets this condition. Finally, even though the surface simplicity of ' $P(m, n)$ ' makes it hard to see why induction on this definition should require impredicative comprehension, this is *not* sufficient reason to say that the definition is too complex to be faithful to ordinary usage. In Frege's words:

We often need to use a word with which we associate a very complex sense. Such a sign seems, so to speak, a receptacle for the sense, so that we carry it with us, while being always aware that we can open this receptacle should we have need of what it contains. It follows from this that a thought, as I understand that word, is in no way to be identified with a content of my consciousness. If therefore we need such signs — signs in which, as it were, we conceal a very complex sense as in a receptacle— we also need definitions so that we can cram this sense into the receptacle and also take it out again (1914: 226).

If Frege is correct, then one cannot infer that a sense is simple (complex), based on the surface simplicity (complexity) of language. For the surface properties of language may be adapted to our practical limitations. If so, then Linnebo's objection to Frege's definition of ' $P(m, n)$ ' does not succeed, since it assumes that one can infer the simplicity of a sense from the surface simplicity of language.



Another way of developing Linnebo's objection is that although there is nothing *intrinsically* wrong with the complexity of Frege's definition of ' $P(m, n)$ ', it nevertheless undermines the explanatory significance of Frege's Theorem. This is because proof by induction on Frege's definition requires impredicative comprehension, which in turn requires prompts us to ask: what are the possible values of the second-order variables in the aforementioned comprehension axioms, under their classical extensional interpretation? To which the usual answer is that any given second-order variable ' $F$ ' ranges over *all sub-sets* of the domain of first-order objects that ' $F$ ' is true of. This threatens to undermine the explanatory significance of Frege's Theorem, for the following reason. Linnebo writes:

I claim that our understanding of the notion of an arbitrary subcollection is based on the combinatorial idea of running through the domain, making an independent choice about each element whether or not it is to be included in the subcollection being defined... To see why the claim is plausible, it is useful to imagine that you are explaining the notion of an arbitrary subcollection to someone entirely innocent of the notion. It won't do to explain to your pupil that he is to divide the collection in two. For this will either fall short of the idea of an arbitrary subcollection or presuppose it. Rather, you will need to explain to your pupil that, given any collection, he is to make a series of steps, each involving the consideration of one element of the collection and a decision whether or not to include this element in the subcollection.

[Further] I claim that the combinatorial idea of running through a domain step by step presupposes an ordinal counterpart of the Successor Axiom (ibid: 170).

Here the idea appears to be that in order to understand the content of 'all sub-sets of a domain', one must understand that one can check each object in the domain, to verify whether or not it is

to be included in a given sub-set, as determined by the property or relation that defines that sub-set. But this requires understanding that for every element checked, there is another that can be checked. And this in turn requires understanding the more general claim that for every step performed, there is another iteration that can be performed. Further, it is not enough that one has the basic logical *ability* to iterate a step; rather, to understand the content of ‘all sub-sets of a domain’, one must understand *that* for every step performed, there is another iteration that can be performed. This undermines the explanatory significance of Frege’s Theorem, because its *raison d’être* is the derivation of axioms (1) – (7); but the derivation of axiom (6) turns out to assume a proposition about indefinite iteration – one that, as Linnebo points out, is essential to the claim that every ordinal number has a successor.

I will offer my own response to Linnebo’s argument in the next section. For the moment I want to discuss Heck’s response, which is to give up Frege’s definition of ‘ $P(m, n)$ ’ Heck writes:<sup>85</sup>

Although the definition of precession is undeniably  $\Sigma^1_1$  in form, it is not, I want to suggest,  $\Sigma^1_1$  in spirit. The definition one would really like to give is this one:

(*P-lite*)  $P(\#x:Gx, \#x:Fx) \equiv \exists y(Fy \wedge \#x:Gx = [\#x: Fx \wedge x \neq y])$ .

To be sure, (*P-lite*) is not a proper definition. It does not tell us when  $P(m, n)$  but only when  $P(\#x:Gx, \#x:Fx)$ : Nothing in (*P-lite*) tells us whether Julius Caesar, that same familiar conqueror of Gaul, precedes 0 or not (2011: 271-2).

(*P-lite*) is not a definition of ‘ $P(m, n)$ ’, but merely defines the relation that relates two *concepts* iff one has one fewer things in its extension than the other. Further, it is not a proper definition of ‘ $P(m, n)$ ’ for the by now familiar reason that ‘ $\#x: Fx$ ’, which occurs in the definiens, is an

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<sup>85</sup> I have taken the liberty of replacing Heck’s ‘ $N$ ’ and ‘ $Pab$ ’ with ‘ $\#$ ’ and ‘ $P(m, n)$ ’ respectively.

undefined primitive in Heck's system. Further, since ' $\#x: Fx$ ' and ' $\#x: Gx$ ' occur in *both* the definiendum and the definiens, (*P-lite*) is also circular. In all these respects it resembles the failed attempt at a recursive definition of 'number' at *Grundlagen* §56, against which Frege first raises the Julius Caesar problem. Nevertheless, Heck insists that if the Caesar problem can be solved:

Then (*P-lite*) tells one everything one needs to know about predecession. How would that allow the Neo-logician to avoid appealing to comprehension? Well, the Neo-logician might regard predecession as primitive and regard (*P-lite*) as analytic of that notion (ibid: 272).

What I take Heck to be proposing is that (*P-lite*) should be elevated to the status of a non-logical axiom, along with HP. The upshot of this would be that Heck has *two* undefined primitives in his system: ' $\#x: Fx$ ' and ' $P(m, n)$ '. This would surely undermine the philosophical significance of his results, unless he can correctly diagnose and solve the Caesar problem. Further, I think that it is unwise to pin the explanatory significance of the project on his ability to do this. For the Caesar problem may simply be that improper definitions, including the proposed ones based on axioms like HP and (*P-lite*), fail to specify sense and reference for their definienda.<sup>86</sup> If so, then the problem would be fatal to (*P-lite*).

To take stock, I think that Heck fails to show that axiom (6) can be derived using only predicative comprehension; further, I doubt that (3), (4) and (5) can be derived in this way either. For the cost of doing so is that Heck has to take ' $\#x: Fx$ ' and ' $P(m, n)$ ' as primitive. Furthermore, all parties are agreed that deriving axiom (7) requires impredicative comprehension. Assuming that all this is correct, it follows that impredicative comprehension is *crucial* to the establishing

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<sup>86</sup> See Salmon (forthcoming), as discussed in chapter 2 section 6.

Frege's Theorem in a philosophically significant way. Given this, my next task is to evaluate whether the requisite comprehension axioms are general primitive truths.

#### 6. Are impredicative comprehension axioms primitive truths?

Without further ado, I turn to my three questions:

(Q1.a) Are these axioms primitive truths of pure logic?

(Q1.b) Are they self-evident?

(Q1.c) If they are not self-evident, why should we accept them as primitive truths?

Regarding (Q1.a), I will begin by revisiting Linnebo's argument that the content of 'all sub-sets of a domain' assumes a proposition about indefinite iteration that is essential to the claim that every ordinal number has a successor. I do not propose to take issue with his claim that

the combinatorial idea of running through a domain step by step presupposes an ordinal counterpart of the Successor Axiom (ibid: 170).

Rather, my objection is to his claim that

our understanding of the notion of an arbitrary subcollection is based on the combinatorial idea of running through the domain, making an independent choice about each element whether or not it is to be included in the subcollection being defined (ibid).

I will argue that our understanding of the concept of an arbitrary subcollection is *not* based on this idea. Then I will argue that it is based on another idea that is also combinatorial.

Despite the impression engendered by the comprehension axioms, the concept of a set or collection is not that of a collection of things having some common feature or satisfying a condition. Rather, it is simply the concept of a collection of things, which may or may not share

anything in common. Further, one should not understand the concept of an *arbitrary* subcollection as the product of a process of choosing elements from a given collection. For thinking of things in that way encourages what I have just claimed is a false view, whereby the elements of a subcollection are chosen on the basis of some common feature (and so not chosen arbitrarily). Furthermore, one need not understand the concept of an arbitrary subcollection as the product of a process of choosing elements from a given collection. For one can instead think of a subcollection as a collection that exists independently of any collection of which it is happens to be a subcollection (other than itself). One can then simply compose the resulting concept of subcollection with the concept of arbitrariness to obtain the concept of an arbitrary subcollection.<sup>87</sup>

However, there is no way to understand the concept of a collection, and so the concept of a subcollection, other than through its relation to its elements. How then can the concept of a collection be understood, without recourse to a process of choosing the elements that form it? The answer lies in the fact that whereas a collection is one thing formed out of many elements, these elements are a plurality, where a plurality is many things, not one. Further, in order to understand the concept of a collection through its elements, without choosing them, one quantifies over them as a plurality. Explaining this further will require a very brief excursion into plural logic.

The vocabulary of plural logic contains plural variables ‘ $xx$ ’ and plural quantifiers ‘ $\exists\exists xx$ ’ and ‘ $\forall\forall xx$ ’ (for brevity I will write ‘ $\exists xx$ ’ and ‘ $\forall xx$ ’). These are read as “there are some objects, the  $x$ ’s” and “for any objects, the  $x$ ’s.” For example:

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<sup>87</sup> Thanks to Nathan Salmon for setting me straight on this issue.

There are some Bostonians who speak only to one another.<sup>88</sup>

The vocabulary also contains a two-place logical relation symbol ‘<’, the first and second argument-places of which are, respectively, singular and plural, and where ‘ $u < xx$ ’ is read as “ $u$  is one of the  $x$ ’s.” For example, relative to an assignment of values to free variables, we get constructions such as

Serena Williams is one of the Williams Sisters.

Finally, there is the symbol ‘==’:

$$xx == yy \equiv_{\text{df}} \forall u(u < xx \leftrightarrow u < yy)$$

Using this apparatus, one can obtain a collection, without recourse to a process of choosing the elements that form it. Rather, one simply quantifies over these elements, by saying “some elements  $xx$  of the domain.” One then “lassos” them into a collection, this being one thing formed out of a plurality of many. Since all of the elements of the resulting collection are elements of the domain, they form a subcollection of it.

Before I proceed with my argument I will recap. The case of second-order logic prompted me to ask after the possible values of the second-order variables in the impredicative comprehension axioms, under their classical extensional interpretation. Having used plural logic to understand what these values are, I am now prompted to ask after the values of the plural variables in axiom of plural comprehension:

$$\exists xx \forall u_1 \dots u_n (u_1 \dots u_n < xx \leftrightarrow \Phi u_1 \dots u_n)$$

As Linnebo himself points out, the answer is surely that ‘ $xx$ ’ ranges over *all pluralities*, where a plurality is many things, not one collection.<sup>89</sup> This raises the question of what is required to understand the concept of all pluralities. It seems to me that this requires—in addition to an

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<sup>88</sup> Boolos (1998: 57).

<sup>89</sup> Linnebo (2003).

understanding of ‘all’— an understanding of the concept of *combinations of individuals*. Further, while this is not the same as the idea of a process of choosing, it is nevertheless not logical but *combinatorial*, in the sense of concerning unordered arrangements of objects. If Linnebo and I are correct about this, then non-logical content is assumed in the impredicative comprehension axioms.

My analysis also appeals explicitly to the idea of “lassoing” elements  $xx$  of the domain into a set or collection, since I make no claim to have dispensed with sets by appealing to pluralities.<sup>90</sup> So an assumption governing the operation of set-formation is required. So is an assumption governing power-set formation, since the domain is itself a set of all subsets. (More on set theory in the next chapter.) Thus, my answer to (Q1.a) is that in the final analysis, the impredicative comprehension axioms, under their classical extensional interpretation, are not primitive truths of pure logic, since they presuppose concepts from plural logic and thus from combinatorics, as well as from set theory. However, and contrary to what Linnebo claims, they *do not* presuppose a proposition about indefinite iteration that is essential to the claim that every ordinal number has a successor. Moreover, I would add that the aforementioned concepts are still suitable candidates to explain the general applicability of arithmetic, since abstract as well as concrete objects can be combined into pluralities and sets.

I now turn to (Q1.b): are the impredicative comprehension axioms self-evident primitive truths? In answer to this, my analysis of ‘all sub-sets of a domain’ suggests that these axioms are not self-evident primitive truths of arithmetic. For even if they can be accepted on the basis of a full understanding of the concept of a set or collection, and of a powerset, such understanding is surely not required to accept that every number is the predecessor of some number.

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<sup>90</sup> Contrast this with Boolos (ibid: ch 4-5).

Turning to (Q1.c), why else should we accept the comprehension axioms? One might instead appeal to the argument from fruitfulness (see chapter 2, section 4), to justify accepting as primitive axioms that are not self-evident. The argument is that one should accept axioms if doing so allows one to discover what we would call “the right modeling” of arithmetic; that is, if doing so allows one to derive correspondents of axioms (1) – (7) *which preserve the thoughts expressed by the latter axioms*. However, this argument is hostage to the accuracy of one’s definitions, and as such is undermined by the fact that Heck has no definition of ‘ $\#x: Fx$ ’, which he has to take as primitive.

Another argument that Heck could deploy is that the propositions to be derived when establishing Frege’s Theorem are, in the final analysis, so sophisticated, that it is of no concern that the comprehension axioms required to derive these propositions are not self-evident. Heck writes:

[T]he concept of a natural number—that is, the concept of finitude—is really very sophisticated. I am not at all sure that most of the undergraduates I have been privileged to teach have had more than a very tenuous grasp of it. Sure, they can wave their hands, but what do they really know about finitude? The fact that the existence of sums and products is (probably) un-provable in any predicative form of Frege arithmetic thus leaves me unfazed (2014: 28).

This response may be correct as regards knowledge of the existence of sums and products. But there remains the problem that axiom (6) –the existence of successors– is also underivable, in a philosophically significant way, in predicative Frege Arithmetic (see the end of section 6).



## Chapter 4: Set-theoretic logicism

### 1. Introduction

In this chapter I turn to Kripke's proposal to represent numbers in set theory, which promises to avoid the problems that plague Frege's analysis, as well as those that plague other attempts to represent numbers in set theory. My discussion of Kripke's proposal draws on his forthcoming *Whitehead Lectures*, at the beginning of which he announces his intention to proceed as he did in *Naming and Necessity*, by alternating between discussions of apparently unrelated topics in a way that allows each discussion to shed light on the others. For the most part I will avoid this strategy, and will present the material in a rather more linear fashion.

Kripke also explains that his approach will be "dialectical," in the sense that his proposal will be a "synthesis" of various the grains of truth he sees in views that he has otherwise rejected in the course of his philosophical development. The first such view is logicism. Since I have already discussed Frege's logicism and Kripke's objections to it in chapter 2, I will now focus on set-theoretic logicism.

The aim of the set-theoretic logicist program is to represent arithmetic in set theory, not just for the sake of representing it within a comprehensive deductive framework, but also for the sake of gaining some insight into the nature and/or justification of arithmetical propositions.<sup>91</sup> In section 3 I will expand on this characterization, and show why Kripke's proposal to represent numbers in set theory is intended as a contribution to the set-theoretic logicist program. But before doing this, I will remind the reader of some basic facts about the textbook way of

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<sup>91</sup> It is arguable that in addition to thinking that sets are all that is needed for a comprehensive deductive system, Zermelo thinks they are the only *basic* assumption of mathematical thinking, in terms of which all others should be explicated. See Zermelo (1909ab in Zermelo 2010). Another example of a set-theoretic logicist is Quine (1969). Set-theoretic logicism is discussed at length but not endorsed by Potter (2004: Ch. 2).

representing arithmetic in set theory, and make some brief remarks about the status of the axioms, so that the issues surrounding the set theoretic variant of logicism can be made clear.

## 2. The derivation of arithmetic in ZF

A set is one object, formed from many, on which operations can be performed. The industry standard theory of sets is ZF, which is a comprehensive, rigorous deductive framework for mathematics.<sup>92</sup> Every object in the domain of the variables of ZF is a set, the only primitive non-logical symbol being ‘ $\in$ ’, which represents the two-place relation *x is an element of y*. The Axiom of Extensionality then tells us that any sets with the same elements are identical:

$$\forall x (x \in y \leftrightarrow x \in z) \rightarrow y = z$$

The presumed consistency of ZF in part due to the Axiom Scheme of Separation. This says that given there is already a set  $z$ , then for any condition  $\Phi x$  that can be stated using classical logic together with ‘ $=$ ’ and ‘ $\in$ ,’ there exists a set  $y$  of members of  $z$  that satisfy  $\Phi x$ :<sup>93</sup>

$$\exists y \forall x (x \in y \leftrightarrow x \in z \ \& \ \Phi x)$$

Assuming that we want as much set theory as we can get without falling prey to Russell’s paradox, this axiom scheme represents the grain of truth in Frege’s Axiom V, which from a set-theoretic perspective is that anything subject to conceptual thought can be collected into a set  $y$ , *so long as one already has a set  $z$  from which to separate  $y$ .*

The textbook derivation of arithmetic in ZF proceeds by developing various ideas that are originally due to Dedekind.<sup>94</sup> One of these is to provide a set-theoretic representation of a progression  $\omega$  satisfying the usual Dedekind-Peano axioms. These are that zero is a member of

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<sup>92</sup> I will elaborate on this statement later.

<sup>93</sup>  $y$  is a subset of  $z$  iff every member of  $y$  is a member of  $z$ . Thus Separation is also known as Subset: there exists a set  $y$  of any subset of  $z$  one can describe.

<sup>94</sup> See Dedekind (1888). Some of these ideas were refined by Zermelo. See Zermelo (2010).

$\omega$ , that every member of  $\omega$  has a successor, that different members of  $\omega$  have different successors, and that zero is not a successor. Finally, there is the induction axiom scheme that if zero has some definite mathematical property, and for any member  $n$ , the successor of  $n$  also has that property whenever  $n$  does, then all members of  $\omega$  have that property:

$$P(0) \wedge \forall n [P(n) \rightarrow P(S(n))] \rightarrow \forall n P(n)$$

The requisite progression satisfying these axioms is usually represented using a proposal due to von Neumann, by defining each number  $m$  as a set with  $m$  members, each of which are themselves sets. This requires one to accept the Null Set Axiom, that there exists a set with no members:

$$\exists x \forall y (y \in x \leftrightarrow y \neq y)$$

Since only the null set  $\emptyset$  has no members, 0 is defined as the null set. Then for each number  $m$ , the successor of  $m$  is defined as the union of  $m$  and its singleton. Note that, for reasons that will be discussed presently, the subscript of '=' *does not* indicate identity of sense as it did in the previous chapters:

$$0 =_{\text{df}} \emptyset$$

$$S(m) =_{\text{df}} U\{m, \{m\}\}$$

Since this proposal requires us to form  $U\{m, \{m\}\}$ , it must appeal to the Union Axiom that for any set  $x$  there exists a set  $z$  of members of members of  $x$ :

$$\forall x \exists z (y \in z \leftrightarrow \exists u (y \in u \& u \in x))$$

In the present application of this axiom,  $x = \{m, \{m\}\}$ , and  $z = U\{m, \{m\}\}$ . Further, since the existence of singletons follows from the axiom of pairing,

$$\exists y \forall x (x \in y \leftrightarrow x = u \vee x = v),$$

we must assume this axiom too.

Given how 0 and the successor of  $m$  are defined, 1 is  $U\{\emptyset, \{\emptyset\}\}$ . Since  $\emptyset$  has no members,  $U\{\emptyset, \{\emptyset\}\}$ —which is the set of members of members of  $\{\emptyset, \{\emptyset\}\}$ —is simply  $\{\emptyset\}$ . 2 is then defined as  $U\{\{\emptyset\}, \{\{\emptyset\}\}\}$ , which is  $\{\emptyset, \{\emptyset\}\}$ . 3 is defined as  $U\{\{\emptyset, \{\emptyset\}\}, \{\{\emptyset, \{\emptyset\}\}\}$ , which is  $\{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$ . Thus by applications of the above definitions and the Union Axiom we obtain the progression:

$$\begin{aligned} 0 &= \emptyset, \\ 1 &= \{0\} = \{\emptyset\}, \\ 2 &= \{0, 1\} = \{\emptyset, \{\emptyset\}\}, \\ 3 &= \{0, 1, 2\} = \{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\} \dots \end{aligned}$$

Clearly each member  $m$  of this progression is a set with  $m$  members.

Another proposal due to Zermelo, is to define each number  $m$  as the singleton of its unique predecessor, which is also a set:

$$\begin{aligned} 0 &=_{\text{df}} \emptyset \\ S(m) &=_{\text{df}} \{m\} \end{aligned}$$

Given how 0 and the successor of  $m$  are defined, 1 is  $\{\emptyset\}$ , 2 is  $\{1\}$ , which is  $\{\{\emptyset\}\}$ , and 3 is  $\{2\}$ , which is  $\{\{\{\emptyset\}\}\}$ . Thus we obtain the progression:

$$\begin{aligned} 0 &= \emptyset, \\ 1 &= \{\emptyset\}, \\ 2 &= \{\{\emptyset\}\}, \\ 3 &= \{\{\{\emptyset\}\}\} \dots \end{aligned}$$

The next requirement is to show how the numbers can serve as a measure of cardinality, and so serve to answer ‘how many?’ by counting. To do this we appeal to the fact that if we count the F’s by putting them in one-to-one correspondence with the members of a progression,

then the last member that corresponds with an F will be as Cantor puts it, the same “in every arrangement of its [the set of F’s] elements as a ‘well-ordered set’ ” (1885). But if the last member that corresponds with an F will be the same in every arrangement, then the last member that corresponds with an F does not depend on the order (or “arrangement”) in which the F’s are counted. Further, if the last member that corresponds with an F does not depend on the order in which the F’s are counted, then it must depend on something else, and the only other thing on which it can depend is the cardinal size of the F’s as a whole. But if the last member that corresponds with an F depends on the cardinal size of the F’s as a whole, then it is a measure of the cardinality of the F’s. Given this, and assuming that we start counting from 0, instead of ‘the number of F’s is  $m$ ’ we can say: *the number of F’s is the least member of a progression the predecessors of which can be put in one-to-one correspondence with the F’s, irrespective of the order in which the F’s are counted.*

I now turn to the definition of ‘natural number.’ Here one first defines the notion of an *inductive set* as containing 0 as a member, as well as the successor of any of its members:

$$\text{Inductive}(x) \equiv_{\text{df}} \emptyset \in x \wedge \forall y (y \in x \rightarrow \text{Suc}(y) \in x)$$

If one’s ultimate goal is to derive other mathematics in ZF *from* the representation of arithmetic, it is necessary to help oneself to the Axiom of Infinity, that there exists an inductive set:<sup>95</sup>

$$\exists x (\emptyset \in x \wedge \forall y (y \in x \rightarrow \text{Suc}(y) \in x))$$

One can then define the notion of a natural number as follows:

$$\text{Natural number}(n) \equiv_{\text{df}} n \in \text{every inductive set.}^{\text{96}}$$

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<sup>95</sup> This was first noted explicitly by Zermelo. See his (ibid).

<sup>96</sup> This is essentially Dedekind’s idea of defining the natural numbers as a set that is a member of any set closed under the successor operation, where a set  $Y$  is *closed* under an operation  $f$  if for every  $x$  in  $Y$ ,  $f(x)$  is also in  $Y$ . See Dedekind (ibid).

Then it can be shown that the natural numbers form a set. For by Infinity there already exists an inductive set, that we will call ‘ $A$ ’, from which to separate the numbers; and since the numbers are by definition members of every inductive set, by Separation they form the set:  $\{x: x \in A \ \& \ x \in \text{every inductive set}\}$ . But since  $A$  is inductive, by Extensionality  $\{x: x \in A \ \& \ x \in \text{every inductive set}\} = \{x: x \in \text{every inductive set}\}$ .

Fortunately however, the Axiom of Infinity is not needed to represent arithmetic itself. Rather, one can define the notion of a natural number as follows:

Natural number( $n$ )  $\equiv_{df}$   $n$  is a von Neumann ordinal &  $n \in$  every inductive set.

If the axiom of infinity is false, then one can define the numbers as finite von Neumann ordinals and say that these are members of every inductive set, because statements about every inductive set are vacuously true.<sup>97</sup>

The set-theoretic correspondent of mathematical induction can also be shown to be true of the natural numbers, by the above definition. For if zero has some definite mathematical property, and for any member  $n$ , the successor of  $n$  has that property whenever  $n$  does, then the set of things satisfying that property is an inductive set, and so the property in question is true of every number. Induction can then be used to derive formulae representing the other axioms of arithmetic.

Defining the natural numbers, explaining their application in counting, and deriving the correspondents of the axioms is only part of the project of representing arithmetic in ZF. For one must also define the arithmetical operations of addition, multiplication and exponentiation. Once again this can be done using ideas originally due to Dedekind, by defining these operations implicitly, by specifying a function using the following pairs of recursion equations, where in

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<sup>97</sup> Parsons (1987) discusses this and other ways of representing arithmetic without Infinity.

each case the first equation gives the value of the function for 0 as argument, and the second gives the value of the function for  $S(y)$  in terms of the value for  $y$ . To define addition we have:

$$(i) \quad x + 0 = x$$

$$(ii) \quad x + S(y) = S(x + y)$$

To define multiplication:

$$(iii) \quad x \cdot 0 = 0$$

$$(iv) \quad x \cdot S(y) = (x \cdot y) + x$$

And to define exponentiation:

$$(v) \quad x^0 = 1$$

$$(vi) \quad x^{S(y)} = (x^y) \cdot x$$

From these equations one can derive the relevant arithmetical laws as theorems of set theory, by using induction to show these laws hold for all  $x, y, z$  contained in the set of natural numbers. For example, consider the associative law for addition:

$$x + (y + z) = (x + y) + z$$

Firstly we have to show that the set  $\{z: x + (y + z) = (x + y) + z\}$  contains 0: in more familiar terms that  $x + (y + 0) = (x + y) + 0$ . This is done using equation i. as follows:

$$\begin{aligned} x + (y + 0) & \\ &= x + y && \text{(by i)} \\ &= (x + y) + 0 && \text{(by i)} \end{aligned}$$

Then we have to show that if the aforementioned set contains  $z$ , then it contains  $S(z)$ : in more familiar terms that  $x + (y + S(z)) = (x + y) + S(z)$ . Assuming this set contains  $z$  then:

$$\begin{aligned} x + (y + S(z)) & \\ &= (x + S(y + z)) && \text{(by hypothesis)} \end{aligned}$$

$$= S(x + (y + z)) \quad (\text{by ii})$$

$$= S((x + y) + z) \quad (\text{by ii})$$

$$= (x + y) + S(z) \quad (\text{by ii})$$

To take stock, the axioms used in this representation of arithmetic are Extensionality, Separation, Null Set, Union and Pairing, or, if one prefers, Weak Pairing, that for any objects  $x$  and  $y$ , there is a set  $z$  containing  $x$  and  $y$  (and maybe something else):

$$\exists z (x \in z \wedge y \in z)$$

The derivation of arithmetic requires no appeal to Infinity. Nor does it require appeal to the Powerset Axiom that for any set  $x$ , there is a set  $z$  such that  $y$  is a member of  $z$  iff  $y$  is a subset of  $x$ :

$$\forall x \exists z (y \in z \leftrightarrow y \text{ subset of } x)$$

However, Powerset and Infinity, along with the other axioms mentioned, are required to derive other mathematics in ZF *from* the above representation of arithmetic.

Plainly most of the axioms of ZF that I have described are not *obviously* self-evident primitive truths. Why else should we accept them? The comprehensiveness of ZF is the basis for an argument from fruitfulness for its axioms. According to this argument, one should accept the axioms because they provide an apparently consistent comprehensive framework for other mathematics. However, this way of justifying the axioms of ZF raises two problems. Firstly, it assumes that one already accepts other mathematics, and as such immediately calls into question whether the axioms can provide *foundational* justification for this mathematics. Secondly, as Geroge Boolos remarks, such a justification does not explain the presumption that ZF is



consistent; for in the case of a system that is justified only by its fruitfulness, such as Quine's NF, the inconsistency of the system would be unremarkable. This is not the case with ZF.<sup>98</sup>

I have claimed that the axioms of ZF cannot be accepted as primitive based on considerations of obvious self-evidence, or fruitfulness. However, the possibility remains that they are self-evident without being obviously so, because they are entailed by primitive truths about an underlying conception of set that is known as "the iterative conception" (Boolos: 1998).<sup>99</sup> This can be summarized by the following four claims. (i) Sets are organized into stages. (ii) At stage 0 there are either all possible sets of ur-elements, or there is the null set (if there are no ur-elements). (iii) At every stage  $\alpha$  there are all possible sets of the ur-elements and sets that exist at stages previous to  $\alpha$ . (iv) This is still the case for  $\alpha = n$ , for  $\alpha = n+1$ , for  $\alpha = \omega$ , for  $\alpha = \omega + 1$ , *and so on*; there is no last stage.

This conception can be argued for using the method of reflective equilibrium, as follows.<sup>100</sup> Firstly, conceptual analysis leads one to the naive conception of a set according to which every concept determines a corresponding set. Secondly, by exploring the consequences of this assumption in a deductive system, one sees that it leads to paradox, showing that the original analysis is missing something fundamental. Thirdly, one concludes that the analysis is missing that *a set in some sense depends on its members*, an insight that can be understood and accepted by someone with the concept of a set as a one object formed out of many. One takes this insight as the basis of a new analysis of the concept of set, the iterative conception. Fourthly, one explicates the new conception, in this case by spelling out (i) – (iv) in an informally rigorous way; from this it follows that sets cannot contain themselves, because, by (iii), they contain the

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<sup>98</sup> Boolos (1998).

<sup>99</sup> Boolos acknowledges that his first learned about the iterative conception from Kripke.

<sup>100</sup> See Goodman (1955). For the application of the method to set theory see Scanlon (2014: lecture 4).

ur-elements and sets that exist at stages *previous* to the stage at which they are formed. Finally, one derives the axioms of ZF from (i) – (iv), also using informal mathematical rigor.<sup>101</sup> On this approach then, it is (i) – (iv) that are candidates to be primitive truths.

One reason that the iterative conception is of interest to the logicist is that, arguably, the analysis of the relation of dependency that it provides does *not* appeal to spatiotemporal intuition. For this relation can be argued to be transitive and irreflexive and well-founded using semantic analysis. I will focus on the most controversial aspect of this claim: that dependency can be shown to be well-founded using semantic analysis.<sup>102</sup> To defend this claim one begins with the premise that conceptual content can be grasped by thinking. In Frege's words:

I understand objective to mean what is independent of our sensation, intuition and imagination... but not what is independent of reason,—for what are things independent of reason? To answer that would be as much as to judge without judging, or to wash the fur without wetting it (1884: S26).

Secondly, the argument continues, the fact that conceptual content can be grasped by thinking places constraints on what it is like. One of these constraints is that no mathematical thought, or stock of thoughts, that successfully represents its subject matter, can fall into a backwards infinite-regress of contents. For if it did, then there would be no way to calculate its truth-value. Further, no set that is the subject of successful mathematical thought can fall into a backwards infinite regress of sets, since if it did, then the corresponding thought about it would fall into a backwards infinite regress and so be devoid of truth-value. Thus the advocate of the iterative conception can argue that dependency is well-founded, by appeal to an analysis of conceptual

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<sup>101</sup> The fact that the conception does not provide reason to accept the axiom of Replacement will not concern me, since my concern is a set-theoretic analysis of arithmetic, for which Replacement is not needed.

<sup>102</sup> See Potter (2004: 40), inspired by Wittgenstein (2.0211).

content, rather than by appeal to spatiotemporal intuition.

Even if dependency can be analyzed in a satisfactory way, there remains the following worry. By (iii) and (iv), the iterative hierarchy is as *large as possible*, containing at every stage  $\alpha$  all possible sets of the ur-elements and sets that exist at stages previous to  $\alpha$ , and continuing to  $\alpha = \omega + 1$ , and so on. Why should we accept these claims as primitive? Not because of their fruitfulness, since, as we have already seen, this calls into question whether set theory is to provide foundational justification for other mathematics.

Once again we have to appeal to the insight that a set in some sense depends on its members, as follows. Suppose that  $x$  is at stage  $\alpha-1$ . Since all *elements* of  $x$  are at a stage previous to  $\alpha-1$ , all *subsets* of  $x$  are at stage  $\alpha-1$ . So *the set of all subsets* of  $x$  is at stage  $\alpha$ . To the extent that this argument makes (iii) seem true or more plausible, it justifies it by reflective equilibrium. However, it is not clear that this justification transmits to (iv). So there is the worry that the iterative conception cannot be accepted as primitive, and so is unsuitable to explain why something as basic as arithmetic is justified.

This objection is not supposed to be fatal, since there are various other motivations that one might have for representing arithmetic in ZF. Next I will describe some of these, before saying what I think Kripke's motivations are.

### 3. What are the motivations for representing arithmetic in ZF?

(a) *Comprehensiveness and rigor*: Earlier I described ZF as “a comprehensive, rigorous deductive framework for mathematics.” By this I mean that one can represent the axioms and primitives of other mathematical theories in ZF, in the way just illustrated. Not only is ZF is extremely comprehensive in this regard, but it is also widely thought to be consistent. For these reasons, a mathematician working in subject area  $a$  can appeal to theorems of  $a$ , in order to prove

theorems of subject area  $b$ , while being confident that he is not inadvertently appealing to assumptions concerning  $a$  that are inconsistent with those concerning  $b$ . Representing arithmetic is absolutely crucial to establishing the comprehensiveness of ZF, because so much else is derived in ZF *from* the representation of arithmetic.

Although the comprehensiveness and rigor of ZF are in themselves sufficient to ensure its centrality to work in the foundations of mathematics, there are even more ambitious goals that one might want to pursue by representing arithmetic in ZF:

(b) *New insight*: The search for rigor can yield new insights into mathematical concepts in the form of new definitions; and, as we saw in chapter 2, reflection on arithmetical practice at least *suggests* something like a set-theoretic definition of number. This is enough to motivate the search for a set-theoretic definition.

(c) *Foundational justification*: The search for rigor might lead one to want a rigorous set-theoretic justification for arithmetic, in terms of the latter being derivable from set-theoretic principles and definitions.

(d) *Explanation*: If one thinks that arithmetic needs no such justification, one might still want a set-theoretic answer to the question of why arithmetic is justified, in terms of it being derivable from set-theoretic principles and definitions.

This brings me to the question of what Kripke himself intends to achieve by giving a definition of number in ZF. Some light is shed on this during the Q&A that follows *the*

*Whitehead Lectures*, when he says in respect to his own set theoretic definition of number, that

if we look for a set-theoretic foundation as revelatory of our *practice*, the numbers I'm talking about have significant advantages over the traditional numbers...

... there *is* a set-theoretic representation that, within the limits of such [set theoretic] statements, arguably captures our actual practice with the decimal system. Now, in what sense of “captures?”

To this Kripke responds that he is

trying to show whether a given ordinary concept *can* be put in some way into a framework as nearly as possible. That is the kind of claim that can sort of weakly be made here.

These remarks suggest that Kripke believes that the senses —or conceptual contents— of the arithmetical notions *cannot be preserved entirely* by set-theoretic definitions, but that one should nevertheless try to preserve as much as possible. Where does this leave us regarding (a) – (d)? As a philosopher-mathematician who is also a set-theorist, Kripke is surely in pursuit of (a) and (b). Further, although he does not say that he is in pursuit of (c), he does, as we will see, rebut Wittgenstein’s objection to (c), which constitutes progress on behalf of one in pursuit of (c). Given all this, I think it is reasonable to conclude that his intention is to make some progress in the pursuit of (a) - (c). So, even though I can find no reference to (d) in the Whitehead Lectures, they count as a contribution to set-theoretic logicism, by my lights. With that said, I now turn to two objections to the usual set-theoretic definitions of number, to which Kripke responds in the course of developing his synthesis.

#### 4. Benacerraf’s dilemma again

The set-theoretic definitions of number that were described in section 2 contain artificial content that is extraneous to the task of defining our *ordinary* concept of number. To see this, consider that it is a consequence of von Neumann’s definition but not Zermelo’s that each number includes all of its predecessors as both members and subsets, so that 0 is both a member

and a subset of 1 which is both a member and a subset of 2, and so on. On the other hand, it is a consequence of Zermelo's definition but not von Neumann's that each number includes its only immediate predecessor as a member (although not a subset). But neither of these consequences is part of our ordinary concept of number. To make this vivid, Benacerraf points out that if an advocate of von Neumann's definition were to insist that 0 is *both* a member and a subset of 2, while an advocate of Zermelo's definition were to insist that 0 is *neither*, there would be no basis in mathematical practice for deciding who is right. Further, the definitions are incompatible, since if  $2 = \{\{\emptyset\}\}$  and  $2 = \{\emptyset, \{\emptyset\}\}$ , then by transitivity it would follow that  $\{\{\emptyset\}\} = \{\emptyset, \{\emptyset\}\}$ . Benacerraf concludes that since we cannot reasonably say from the point of view of mathematical practice *which* of these incompatible set-theoretic definitions is correct, *neither* of them can be, and this is because both contain unnecessary content.

Of course neither definition is intended to describe our ordinary concept of number, only to represent a progression. So Benacerraf's dilemma is not supposed to embarrass them. However, the dilemma does serve to remind us that these definitions are artificial, and that consequently the resulting set-theoretic derivation does not preserve sense, and so does not help us in the pursuit of the goals described in section 3 by (b) – (d).

In response to his dilemma, Benacerraf—using ideas that are once again essentially due to Dedekind—proposes to define the natural numbers in the following way. Ordered sets or “structures” are isomorphic just in case there is an isomorphism between them, where an isomorphism is a one-to-one correspondence between the members of the sets that accords with the ordering on them. For example, consider a set  $A$  under an ordering  $<_a$  and another set  $B$  under an ordering  $<_b$ . There is an isomorphism between  $A$  and  $B$  just in case: (i) there is a one-to-one correspondence between their members such that (ii) if  $x$  and  $y \in A$  correspond with  $u$  and  $v$

$\in B$ , then  $x <_a y$  iff  $u <_b v$ . For instance, consider the following sets of positive integers and of even numbers, both under the ordering  $<$ :

$\{1, 2, 3, 4, \dots\}$

$\{2, 4, 6, 8, \dots\}$

There is an isomorphism between these two structures, because there is a one-to-one correspondence between their members such that, for example,  $1 < 2$  iff  $2 < 4$ . Yet another result due essentially to Dedekind is that *all structures that satisfy the following conditions are isomorphic*:

*Trichotomy: For every  $x$  and  $y$  either  $x < y$ , or  $x = y$ , or  $y < x$ .*

*Transitivity: For every  $x$  and  $y$  and  $z$ , if  $x < y$  and  $y < z$ , then  $x < z$ .*

*Zero: There is a least element  $x$  such that for every  $y$  other than  $x$ ,  $x < y$ .*

*Successor: For every  $x$  there is a next element  $y$  such that  $x < y$ , but there exists no  $z$  such that  $x < z$  and  $z < y$ .*

*Induction: For any set  $X$  of elements, if the least element belongs to  $X$  and the next element after any element belonging to  $X$  belongs to  $X$ , then all elements belong to  $X$ .*

Call any such structure “a progression.” Benacerraf’s proposal is then to define the numbers as *any progression* rather than as a particular progression, with the caveat that the progression must be useable for counting.

An objection to Benacerraf’s proposal is that it neglects features of the numbers that, although unnecessary for number theory, are evident in ordinary arithmetical practice. As will become clear, Kripke is sympathetic to this objection, being of the view it is part of our ordinary practice that some progressions are privileged. However, before I can explain why Kripke thinks

this, I must first discuss a Wittgensteinian objection to the claim that arithmetic can be justified in set theory, as well as a response due to Mark Steiner.

### 5. Wittgenstein's objections

First a word about exegesis is in order. In what follows I will talk as though Wittgenstein's objections are intended to apply to ZF, rather than to Russell's *Principia Mathematica* as Wittgenstein intended. Also, while Wittgenstein makes a number of objections to justifying arithmetic in *Principia*, I will focus on the ones attributed to him by Steiner,<sup>103</sup> since they anticipate the previous chapter's discussion of the need to preserve the sense of ordinary arithmetic. In order to justify this treatment, and to avoid further questions of Wittgenstein exegesis, I will attribute views to an imaginary philosopher called "Wittgensteiner," rather than to Wittgenstein himself. Finally, although Wittgensteiner's objections may appear to the reader to be based on confusion and so unworthy of serious consideration, they are nevertheless worth stating and engaging with, in order to extract the grain of truth that Kripke sees in them.

The first of Wittgensteiner's objections to be considered proceeds from the claim that the set-theoretic representation of arithmetic is not "surveyable." By this Wittgensteiner means that the set-theoretic representatives of the propositions of arithmetic are, in some cases, so hard to discern, that creatures subject to our limitations cannot know whether we have valid proofs of these propositions in set theory. In this regard, Wittgensteiner says:

[C]ould we also find out the truth of the proposition  $7034174 + 6594321 = 13628495$  by means of a proof carried out in [set theoretic] notation? — Is there such a proof of this proposition? — The answer is: no (1956: II, 3).

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<sup>103</sup> Steiner (1975: 16-18, 41-54)



Further, Wittgensteiner continues, a proof should convince us that a proposition is true. Furthermore, since some set-theoretic proofs fail this criterion, one must introduce non set-theoretic content into them, so that they become surveyable enough to produce conviction. In particular, Wittgensteiner claims, positional notation must be used, and addition must be defined in terms of repeated succession, multiplication in terms of repeated addition, and exponentiation in terms of repeated multiplication. Then we can prove that each unsurveyable proof, with all such definitions eliminated, is equivalent to a surveyable one. However, Wittgensteiner continues, this proof of equivalence must thereby draw on notions from outside of set theory. In which case, he concludes, the justification for the propositions of arithmetic cannot be provided by set theory alone.

Wittgensteiner is simply wrong that the aforementioned definitions require notions from outside of set theory. This is because the aforementioned implicit definitions can be supplanted with explicit ones, which show that there exist unique functions for which the aforementioned recursion equations hold. Equivalently, as Steiner points out, we can define the operations by showing there exist unique sequences representing each operation. For example, we can define ' $x \cdot y$ ' by showing that there exists a unique sequence whose first member is 1, of length  $y+1$ , in which each member is  $x$  plus the previous one; likewise, we can define exponentiation by showing there exists a unique sequence whose first member is 1, of length  $y+1$ , in which each member is  $x$  multiplied by the previous one. The upshot is that we *need not* treat the occurrence of ' $y$ ' in ' $x + y$ ' as a metalinguistic symbol abbreviating  $y$ -many iterations of successor. Nor need we treat its occurrence in ' $x \cdot y$ ' as abbreviating  $y$ -many iterations of  $x$  plus itself; nor need we treat ' $x^y$ ' as abbreviating  $y$ -many iterations of  $x$  multiplied by itself, as Wittgensteiner intimates that we must. One might worry about the appeal to the notion of a sequence in this explanation,

but as will become clear later, this notion can be defined in set theory without presupposing numbers.

Armed with these set-theoretic definitions of arithmetical operations, we are now in a position to understand Steiner's response to Wittgensteiner's claim that the set-theoretic representation of number is not surveyable. Steiner's response—adjusted slightly to fit the present context—is that as well as representing a progression in set theory as per von Neumann, Zermelo et al., one can also represent descriptions of numbers as polynomial descriptions. To return to Wittgensteiner's example, rather than representing '7034174' by producing the corresponding numeral in Zermelo's notation, we can instead first translate '7034174' into the corresponding polynomial:

$$7 \cdot 10^6 + 0 \cdot 10^5 + 3 \cdot 10^4 + 4 \cdot 10^3 + 1 \cdot 10^2 + 7 \cdot 10^1 + 4$$

Next we help ourselves to Zermelo's notation for the progression 0, 1, 2 ... 10:

$$0 = \emptyset,$$

$$1 = \{\emptyset\},$$

$$2 = \{1\} = \{\{\emptyset\}\},$$

$$3 = \{2\} = \{\{\{\emptyset\}\}\}, \dots$$

Finally, we can represent the above polynomial in the following way:<sup>104</sup>

$$\begin{aligned} & \{6\} \cdot \{9\}^{\{5\}} + \emptyset \cdot \{9\}^{\{4\}} + \{3\} \cdot \{9\}^{\{3\}} + \{3\} \cdot \{9\}^{\{2\}} + \{\emptyset\} \cdot \{9\}^{\{1\}} + \{6\} \cdot \{9\}^{\{\emptyset\}} \\ & + \{3\} \end{aligned}$$

There is of course the obvious objection that we have not here used *primitive* notation to represent the polynomial in question. To this Steiner responds that there can be no objection to

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<sup>104</sup> Or using superscripted numerals to abbreviate iterations of singleton:  
 $\{^7\emptyset\} \cdot \{^{10}\emptyset\}^{\{^6\emptyset\}} + \emptyset \cdot \{^{10}\emptyset\}^{\{^5\emptyset\}} + \{^3\emptyset\} \cdot \{^{10}\emptyset\}^{\{^4\emptyset\}} + \{^4\emptyset\} \cdot \{^{10}\emptyset\}^{\{^3\emptyset\}} + \{\emptyset\} \cdot \{^{10}\emptyset\}^{\{^2\emptyset\}} + \{^7\emptyset\} \cdot \{^{10}\emptyset\}^{\{\emptyset\}} + \{^4\emptyset\}$



In contrast, from the description

$$7 \cdot 10^1 = 70$$

one may infer the well-formed

$$\exists x (x \cdot 10^1 = 70).$$

I now turn to Kripke's objection to Steiner's proposal, which is that Steiner neither provides a set-theoretic representation of a progression of numerals, nor accounts for the way that numerals are used in mathematical practice. According to Kripke, it is part of our practice that numerals are not just surveyable, but *immediately revelatory* or *buck-stopping*. For example, if a decimal user calculates with decimal notation that the sum of two numbers is 70, he thereby knows what number is the sum in question – no further calculation or inference is necessary. Similarly, if he counts that there are 70 apples in the box, he thereby knows how many apples there are. Further, Kripke claims, the analysis of number should reflect the fact that numerals are immediately revelatory. However, Kripke argues, Steiner's proposal does not meet the latter requirement. For even if numerals and symbols of operation are represented in set theory in the way that Steiner suggests, the results of calculations will *not* be immediately revelatory. There are two reasons for this.

Firstly, Steiner's proposal, to represent polynomial descriptions in set theory, gives the impression that when one is given such a description one has to ask 'which *set* is that?' But, as Kripke points out, this is almost the opposite of our ordinary practice, in which polynomial descriptions are much closer to being immediately revelatory of numbers than are representations of sets. For example, in practice '7 · 10<sup>1</sup>' is much closer to being immediately revelatory of 70 than the corresponding Zermelo or von Neumann numeral. This is one reason to think that Steiner's proposal does not capture the way that numerals are used in arithmetical practice.

Secondly, Kripke claims, although polynomial descriptions are much *more* revelatory than are representations of sets, they not immediately revelatory either, because further calculation is required to know what they denote, by reducing them to numerals. Further, as already noted, Steiner makes no provision for reducing them to numerals in set theory.

As Kripke notes, the relevant difference between immediately revelatory numerals, and expressions such as polynomial descriptions, is anticipated by Felicia Ackerman, who characterizes it as follows:

The position of what a numeral refers to can be known directly simply by understanding the numeral, without having any mathematical knowledge beyond what is necessary to understand the numeral (1978: 151).

She proposes to explain this feature of numerals in the following way:

‘75’ can be understood only in the context of a system of numbers [she means “numerals”], and knowing and understanding a system of numerals seems to be a matter of knowing how to generate in order the progression of numerals and knowing how to count transitively (e.g. to count marbles) in accord with the progression (ibid).<sup>105</sup>

This dovetails nicely with the proposal due to Benacerraf, which was discussed at the end of section 4, and can now be amended as follows: our concept of number is of any progression satisfying the axioms that one knows how to generate in order and use for transitive counting. I now turn to Kripke’s counterexample to this amended proposal.

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<sup>105</sup> Assuming that one equates having a *de re* attitude towards a number with being immediately revelatory, as Ackerman appears to, one might also view this proposal as a condition for having a *de re* attitude towards a number.

## 7. Kripke's counterexample

Kripke offers a counterexample to this proposal, using an invented base-26 positional notation, in which the letter 'z' denotes zero, and the letters 'a' – 'y' in alphabetical order serve as digits denoting the numbers one through twenty-five. After the digits have been exhausted, 'az' denotes twenty-six. The rest of the numerals are generated by ordering letters into sequences by length and then lexicographically (in dictionary order), while disallowing sequences starting with 'z'. Thus 'aa' denotes twenty-seven, 'ab' denotes twenty-eight, 'ay' denotes fifty-one, 'by' denotes seventy-seven and so forth. Using table 1 (appended at the end of this chapter), the reader can now easily train herself to recite these numerals in order and use them for counting.

In order to appreciate Kripke's counterexample, it will help to recall that because we are trained in decimal notation, we can answer the question 'what is  $10^6$ ?' without the tedium of multiplying 10 by itself. Following the conventions of positional notation with '0', in which '0' functions as a placeholder for each power of the base, we simply write '1' followed by six occurrences of '0'. So we can conclude that the answer is '1,000,000'.<sup>106</sup> This answer is immediately revelatory, in that it makes no sense to think *I know that  $10^6$  is 1,000,000, but what number is that?* But notice that the reader who has trained herself in Kripke's notation can also answer the question 'what is  $26^6$ ?' with great ease, since by the conventions of this notation she knows that  $26^6$  must be 'a' followed by six placeholders, i.e. 'a,zzz,zzz.' Further, like '1,000,000' this numeral belongs to a progression that the reader knows how to recite in order and use for counting. So, by the proposal under consideration, 'a,zzz,zzz' should be immediately revelatory.

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<sup>106</sup> This is because we know that  $b^0 = 10^0 = 1$ , and that for any multi-digit numeral  $a_0 \dots a_k$ , each subsequent position  $k-1$ ,  $k-2$  etc. represents a higher power of  $b = 10$ . Finally, we know that 0 must be used as a placeholder for each power. So we can conclude that  $10^6$  must be 1,000,000.

But it is not, since it remains to compute the answer in decimal. So the proposal does not give a sufficient condition for a numeral to be immediately revelatory.

As Kripke sees it, such examples present us with a dilemma. The first horn is *Charybdis*, which is to say that our familiar decimal ciphered-positional numerals are the only ones that are immediately revelatory. The second horn of the dilemma is *Scylla*, which is to say that any progression of numerals that one knows how to generate in order and use for counting is immediately revelatory, so long as one is also *accustomed to* or *interested in* that progression, in the way that we are accustomed to decimal notation. Kripke characterizes the dilemma as follows:

Scylla is sort of cultural relativism, but many people may think Scylla is fine, see, and that there really is no difference. It's just a matter of our interests, our training and so on, but we are no closer to the numbers in one way than another. Charybdis would seem to have to be what? That the decimal system is privileged. But that is preposterous. What, *we* have hit on the right thing, and those other guys are wrong? Thus Charybdis is supposed to be false, because members of other cultures presumably *do* succeed in using their notations to represent the results of counting and calculation in a way that is immediately revelatory.<sup>107</sup>

At one point Kripke characterizes this dilemma as concerning acquaintance. In this regard, he says:

But how can a mere notation get me better acquainted with an object or not, especially if I conceded that I might just as well have been brought up in another one? How can that really be?

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<sup>107</sup> Ackerman describes the view Kripke calls Charybdis as “preposterous.” See Ackerman (ibid).

Putting the problem this way, Charybdis is the view that only the decimal system can acquaint us with numbers, while Scylla is the view that no notation acquaints us with numbers, because immediate revelation is just a matter of

an attitude we have, that we don't know the number. There's no such real thing as genuine acquaintance, sort of an objective thing, as Russell would have called it, acquaintance with the object, or [being] *en rapport* or what have you.

I will eventually propose a version of Scylla. But for now I will simply assume, for the sake of exposition, that neither horn of the dilemma is acceptable, so I may turn to Kripke's proposed middle way between Scylla and Charybdis.

### 8. Kripke's proposal

According to Kripke, for the purposes of set theory and other mathematics, one should, whenever possible, use a notation that is "structurally revelatory" – one that has a structural affinity with the subject matter it represents. Consider, for example, the hereditarily finite sets ('HF'), each of which are finite and contain all possible sets that have already been formed.<sup>108</sup>

These can be represented using the following notation:

$$\{\emptyset, \{\emptyset\}, \{\{\emptyset\}\}, \{\emptyset, \{\emptyset\}\} \dots\}$$

$$\{\emptyset, \{\emptyset\}\}$$

$$\emptyset$$

Plainly it is easier to discern the content of this notation for HF, than it is to discern the content of a notation that works on the opposite principle, such that ' $\{\emptyset, \{\emptyset\}, \{\{\emptyset\}\}, \{\emptyset, \{\emptyset\}\} \dots\}$ '

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<sup>108</sup> Compare the iterative conception of set, discussed at the end of section 2. It is an interesting question whether Kripke thinks that the iterative conception is chosen in order to make the standard notation for sets structurally revelatory. If so, does he also think that having the conception revealed to us in this way makes it self-evident to someone sufficiently trained in the notation, in a way that would help justify the axioms of set theory?



denotes the null set, and ' $\emptyset$ ' denotes the set of all possible sets of sets that have already been formed. This is because the standard notation unlike the reversed notation is structurally revelatory. But what exactly does this require?

This example shows that isomorphism is not necessary for a notation to be structurally revelatory, because the standard notation not quite isomorphic with HF. To see this, recall that there is an isomorphism between  $A$  and  $B$  just in case: (i) there is a one-to-one correspondence between members of  $A$  and  $B$  such that (ii) if  $x$  and  $y \in A$  correspond with  $u$  and  $v \in B$ , then  $x <_a y$  iff  $u <_b v$ . In the present case, let  $A$  be the set of symbols of the standard notation (which I will now represent in bold rather than in quotes), and let  $B$  be HF, where  $A$  and  $B$  are under the partial orderings  $E_a$  and  $E_b$  respectively:

$$\{\emptyset, \{\emptyset, \{\emptyset\}\}, \{\emptyset, \{\emptyset, \{\{\emptyset\}\}\}, \{\emptyset, \{\emptyset\}\} \dots\}$$

$$\{\emptyset, \{\emptyset, \{\emptyset\}\}, \{\emptyset, \{\emptyset, \{\{\emptyset\}\}\}, \{\emptyset, \{\emptyset\}\} \dots\}$$

There would be an isomorphism between these two structures if there were a one-to-one correspondence between their members such that, for example: (i)  $\emptyset$  and  $\{\emptyset, \{\emptyset\}\}$  corresponded with  $\emptyset$  and  $\{\emptyset, \{\emptyset\}\}$ , and (ii)  $\emptyset E_a \{\emptyset, \{\emptyset\}\}$  iff  $\emptyset E_b \{\emptyset, \{\emptyset\}\}$ . However, there cannot be a one-to-one correspondence, since there is more than one order in which the same set can be represented in the standard notation. For example, recalling that by the axiom of extensionality  $\{\emptyset, \{\emptyset\}\} = \{\{\emptyset\}, \emptyset\}$ , both  $\{\emptyset, \{\emptyset\}\}$  and  $\{\{\emptyset\}, \emptyset\}$  can denote  $\{\emptyset, \{\emptyset\}\}$  and so be corresponded with it. So there is not quite an isomorphism.

Nevertheless, there is a clear structural affinity between the two structures that is not present between HF and the reversed notation. For even if the symbols of the reversed notation  $\{\emptyset, \{\emptyset\}\}$  and  $\emptyset$  were in one-to-one correspondence with the HF sets  $\emptyset$  and  $\{\emptyset, \{\emptyset\}\}$  (which they are not, for the reason just given), it would not follow that  $\emptyset E_{aR} \{\emptyset, \{\emptyset\}\}$  iff  $\emptyset E_b \{\emptyset, \{\emptyset\}\}$ ,

where ' $E_{aR}$ ' is the partial ordering on the reversed notation. Rather, by the conventions of how the reversed notation represents HF,  $\{\emptyset, \{\emptyset\}\}E_{aR}\emptyset$  iff  $\emptyset E_b\{\emptyset, \{\emptyset\}\}$ . To take stock, isomorphism is not necessary for a notation to be structurally revelatory. What is necessary is that the notation is ordered in the same way as the subject matter represented.

I now turn to the relationship between being structurally revelatory and immediately revelatory. Here it is important to note that the former is not sufficient for the latter. This is evident from the fact that the standard structurally revelatory notation for HF is not immediately revelatory, since one can ask which set is denoted by  $\{\emptyset, \{\emptyset\}, \{\{\emptyset\}\}, \{\emptyset, \{\emptyset\}\}\}$ . The point can also be seen from the example of prefix (or Polish) notation. Consider again the set theoretic representations of the arithmetical operations of addition, multiplication and exponentiation, as they are given implicitly by recursion equations. These are usually stated in infix notation, as in:

$$\text{i. } x + 0 = x$$

$$\text{ii. } x + S(y) = S(x + y)$$

But they can also be stated in prefix notation. For example, the following are the prefix equivalents of equations i. and ii.:

$$\text{i.a. } f^+(x, 0) = x$$

$$\text{ii.a. } f^+(x S(y)) = S f^+(x, y)$$

I am surely not alone in finding the meaning of these equations much harder to discern than that of their infix equivalents. This is also the case with the infix and prefix statements of the associative law for addition:

$$x + (y + z) = (x + y) + z$$

$$f^+(f^+(x, y) z) = f^+(x, f^+(y, z))$$

Infix notation is thus much closer to being immediately revelatory than prefix notation – at least for me, and I suspect for my readers as well. Yet both notations are structurally revelatory of what they represent, since they have isomorphic parsing trees, with the same roots, labels and orderings.

So far I have given an idea of what is required for being structurally revelatory, and distinguished this from being immediately revelatory. I will now explain how all this relates to Kripke’s proposal. Granting that being structurally revelatory is a desirable property for notations to have, Kripke then uses this fact to argue for his proposal as follows:

On other theories of what the natural numbers are, our conventional decimal notation is not particularly structurally revelatory. The idea of a Frege-Russell number being by abstraction from the cardinality of any set is just best represented structurally by a corresponding number of strokes. The idea of an abstract progression, with no other structure to the progression, is similarly best represented by zero followed by a number of strokes.... The conventional decimal notation would be a highly structurally unrevelatory notation for either of these concepts.

A brief digression: The above quote indicates that Kripke regards the Frege-Russell numbers as equivalence-types —or the corresponding equivalence classes— that are abstracted from the equivalence relation of equinumerosity between sets. However, the problem of decimal notation being unrevelatory does not, to my mind, depend on their being abstracted in this way. For the problem also arises if they are identified with the extensions of what Salmon calls “numerically definite quantifier[s]: *nothing, exactly one more thing than nothing, exactly one more thing than exactly one more thing than nothing; etc.*” (forthcoming: 31). This is because a structurally revelatory notation for this numeric progression is a cumulative notation consisting of a symbol

for the extension of ‘nothing’, followed by a corresponding number of strokes: 0, 0<sup>l</sup>, 0<sup>ll</sup>....

Further, as Kripke points out, the notion of a progression of ordinals can be reflected in the same way.

Returning to the main line of argument, Kripke’s point is that decimal notation is neither structurally revelatory of the Frege-Russell numbers, nor of a progression with no other structure to the progression, because the notation is not cumulative stroke notation, but ciphered, positional and in dictionary order. That is, multi-digit numerals are finite sequences of one or more of the digits ‘0’ – ‘9’, ordered by length and then lexicographically —so that ‘2’ < ‘22’ < ‘23’— where it is stipulated that sequences cannot start with ‘0’. How then can decimal notation be structurally revelatory? Kripke is sympathetic to Benacerraf’s proposal that the natural numbers are any progression that can be used for counting. However, to ensure that decimal notation is structurally revelatory, he amends Benacerraf’s proposal so that the numbers are *any progression of finite sequences consisting of one or more of the ten objects referred to by ‘0’ – ‘9’, where these sequences are ordered by length and then lexicographically, and where sequences of two or more starting with 0 are excluded*. Like the notation, each sequence has a unique successor and predecessor, except for the first sequence, which has no predecessor. Further, the infinite sequence of all these sequences, under their lexicographical ordering, is a progression.

Kripke argues that while this proposal is cooked up to ensure that decimal notation is structurally revelatory, it is also a plausible analysis of our ordinary concept of number. This is because those of us who are trained in the decimal system learn to impose the aforementioned structure on the numbers. That is, we learn to parse, or identify and individuate numbers as finite sequences; for example, we come to identify and individuate 547 as the finite sequence

consisting of the numbers 5, 4, and 7. To be clear, Kripke's claim is *not* that we are consciously aware of identifying of numbers as finite sequences of small numbers. Rather, he appears to share the Fregean view endorsed in the previous chapter, that our ordinary concept of number is discovered by reflection on arithmetical practice:

[W]hether or not someone can come up with such a definition, in fact we are so trained that when we are given a number and we get asked a number, we immediately can say it as such a sequence. You see, the most primitive thing is just reading off the sequence; that is sufficient to generate the numerals themselves... And if you ask what number this is, what is its first digit, you should be able to say, right? You know, "What is the first digit of this number?" The person in the street can give an answer to that. To eliminate redundancies, the first digit is never zero... And so this is what I mean by saying that we think of numbers as finite sequences of such objects, which themselves are numbers less than ten: I mean that we can parse a number as such a sequence, and that if we have done that sufficiently, then if we're given the digit '526', then we've been given the number. If we don't know what the middle digit is, we don't know what number it is. And this I think is a very natural conception.

In this way, having argued that we identify and individuate numbers as finite sequences that make decimal notation structurally revelatory, Kripke uses this claim to explain why decimal numerals are immediately revelatory. The explanation is that our identification of numbers as finite sequences provides a standard for knowing which number we are confronted with, while decimal numerals present numbers as such sequences. Further, Kripke's proposal also provides a response to Wittgensteiner's claim that the set-theoretic representation of arithmetic is not

surveyable, since finite sequences of ten digits grow at a much slower rate than Zermelo's or von Neumann's notation, or, for that matter, than stroke notation for the Frege-Russell numbers.

As Kripke notes, it is a consequence of his proposal that members of cultures that use numerical systems with different bases, will thereby identify and individuate numbers differently than we do:

Now, consider the person trained in another culture, say with another base... Such a person also identifies numbers with sequences, but they identify numbers with a different system of sequences. See, no wonder they will differ from us, then, by what is the sufficient condition for numbers to be given to them, because I mean it's as simple as the fact that they have a different thing in mind (though both are useable, of course) as the natural numbers.

For example, a culture that uses the base-26 notation described in the previous section, will thereby identify and individuate numbers as finite sequences of any of 26 objects, where these sequences are ordered by length and then lexicographically, excluding sequences starting with 'z'. This is how Kripke proposes to steer a middle way between Scylla and Charybdis: Members of cultures, who use systems with different bases, will find numerals from different systems to be immediately revelatory, because their respective systems refer to different numbers, in virtue of the users of these systems identifying and individuating numbers differently, as different finite sequences.

### 9. Kripke's acquaintance theory

At the end of section 7 I pointed out that Kripke characterizes the above dilemma as concerning our acquaintance with numbers. Now I want to look at how his solution to the dilemma fares as a theory of acquaintance. This will be important in what follows, because I will

use the fact that Kripke's theory is to a significant extent a theory of Russellian acquaintance, in order to contrast his proposal with my alternative account of immediate revelation.

The reader will recall from chapter 1 that I characterize acquaintance in terms of Russell's doctrines of Immediacy, Privacy and Complete Revelation. Immediacy, it will also be recalled, is Russell's doctrine that acquaintance is not mediated by "inference or any knowledge of truths" (1912: 43). Obviously this is part of the grain of truth that Kripke sees in Russell's doctrines about acquaintance, and, as such, is part of the motivation for the distinction between being revelatory and immediately revelatory. I think it fair to say that in proposing to explain why a decimal user finds decimal numerals immediately revelatory, Kripke is thereby proposing to explain why the user satisfies Immediacy.<sup>109</sup>

Next I turn to Privacy, Russell's doctrine that we are not acquainted with entities in virtue of grasping inter-subjectively accessible, shareable concepts of these things, but by being privately aware of them. In my view this doctrine should *not* be congenial to Kripke, for the following reasons. According to Kripke, we find numerals immediately revelatory because we are trained to use decimal notation. Further, decimal notation has an objective structure. For this reason, the question of how the numbers are to be identified and individuated, so that the notation is structurally revelatory, is an objective question. Further, its answer is inter-subjectively accessible and shareable, since the notation is a public language. Thus the objective nature of numbers is revealed to users of a notation through training in a public language, not through private awareness.

Finally, I turn to Complete Revelation, Russell's doctrine that when one is acquainted with an entity one grasps it completely, and without the help of knowledge of truths concerning

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<sup>109</sup> In this respect Kripke is in agreement with Burge, although the two disagree about the conditions that are required to satisfy Immediacy. See chapter 1, section 13.

that entity. Regarding such knowledge of truths, Kripke is of the view that knowledge of arithmetical truths about numbers is unnecessary for immediate revelation. This is because

though the child doesn't understand the ideas of addition and multiplication, the child still *may know in some sense* what the number after twenty-six is and *know something* about how many people are in the room if told that there are twenty-seven people in the room. No particular further requirements seem to be reasonable (emphasis added).

The hedges that I have emphasized in the above statement suggest to me that Kripke would prefer to allow that the child's grasp of immediately revelatory numerals can be *partially* rather than *completely* revelatory. However, Kripke is committed to saying that as the child becomes fully numerate, she will, in addition to meeting Ackerman's requirements, eventually come to identify and individuate numbers as finite sequences that make decimal notation structurally revelatory. Further, according to Kripke's theory, if this last requirement is met, then it has been revealed to the numerate subject what numbers are, in so far as: (a) she thinks of numbers as finite sequences of ten objects ordered by length and then lexicographically, and (b) numbers themselves are such sequences. Together with the assumption that (b) describes what numbers are *completely*, it would follow that a numerate subject who meets Kripke's requirement thereby satisfies Complete Revelation.

This assumption is questionable, for the following reason. It will be recalled that part of Kripke's proposal is that the numbers are any progression of finite sequences consisting of one or more of the ten objects referred to by '0' – '9'. The point to which I want to draw the reader's attention, is that Kripke *remains silent about what these ten objects are*. For all that has been said, they could be ordinal numbers, cardinal numbers, or, more specifically, Frege-Russell



numbers, etc. It follows that (b) in the previous paragraph does *not* describe what numbers are *completely*, but only *partially*. Correspondingly, we have Partial rather than Complete Revelation

However, at one juncture Kripke considers making an amendment to his proposal, according to which numbers are finite sequences *of the first ten Frege-Russell numbers*, rather than finite sequences of any ten objects. The reason in favor of making this amendment is as follows. Suppose that we were to switch from our decimal notation to a decimal version of Kripke's invented alphabetic notation (for which see section 7), and thereby come to identify and individuate numbers as finite sequences of any of ten objects denoted by 'z', 'a' – 'i'. In this scenario, these digits would not be immediately revelatory, since we would still have to ask, for example, 'what number is *f*?'. According to Kripke, what explains this is that, in the case described, we have failed to associate small Frege-Russell numbers with the digits. To do this, Kripke claims that we can associate with each digit a "picture" of a surveyable sample set of the requisite cardinality. This picture is immediately revelatory of and so acquaints us with the corresponding small Frege-Russell number. It may even satisfy Privacy, if by "picture" Kripke means a visual image.

As Kripke explains in the second of the Whitehead Lectures, it would follow from this amendment to his proposal that our identification of larger numbers isn't as

sequences of any old objects but sequences of the first ten, that is, the numbers from zero through nine, Frege-Russell numbers, ordered in the same way as I was saying before.

This amendment to Kripke's view would yield the following "two stage" Russellian epistemology. Firstly, we are acquainted with small Frege-Russell numbers because we can picture them in the way just described. Secondly, this acquaintance, together with our training in

the decimal system, leads us to identify and individuate —and so acquaints us with— larger numbers as finite sequences of small Frege-Russell numbers that make decimal notation structurally revelatory. Assuming that this characterizes what numbers are *completely*, it would follow that we thereby satisfy Complete Revelation.

However, Kripke has strong reservations about making the above amendment, because the Frege-Russell numbers cannot be represented in ZF. For they would have to be represented as sets of equinumerous sets, these being the modern set-theoretic correspondents of Frege's extensions containing equinumerous extensions. But as noted in the previous chapter, such sets are non-well-founded, and as a result are disallowed in ZF by the axiom of foundation. Moreover, even if they were not explicitly disallowed by an axiom, there would still be something very strange about the relation of non-well-founded sets to their members, since the latter are not logically prior to the former. Kripke also notes another reason why the Frege-Russell numbers cannot be represented in ZF as sets of equinumerous sets, which is that the set of all sets equinumerous with a given non-empty set can be proved not to exist, because its union will be the universal set. For these reasons, Kripke seems inclined, although reluctantly, to rest with his claim that numbers are finite sequences of the ten objects referred to by '0' – '9', where what these objects are is left unspecified. This brings me to the next aspect of Kripke's proposal, which is his explicit definition of the natural numbers in ZF.

#### 10. The representation of Kripke's proposal in ZF

The proposal to be explicated is that the natural numbers are any progression of finite sequences consisting of one or more of the ten objects referred to by '0' – '9', where these sequences are ordered by length and then lexicographically. There is also the condition that the progression must be usable for counting, but this will not concern me for the moment, since I

have already shown (in section 2) how counting with a progression is supposed to be explained in set theoretic terms. Rather, my first concern is to explain what Kripke means by “finite sequence.” During the Q&A period that follows the Whitehead Lectures, Kripke indicates how he could define this notion in ZF:

[A] finite sequence without repetitions is a linear ordering which is a well-ordering in both directions. Now, that sounds fancier than it really is. I mean, it just means that, well, putting it more simply, every subset has a first and a last element. And so you never go further and further out in either of the two directions, which is the idea of infinity.

To unpack this, first we say that a relation  $<$  linearly orders a set  $X$  if the following conditions are met:

Irreflexivity: *For every  $x$ ,  $\sim x < x$*

Trichotomy: *For every  $x$  and  $y$  either  $x < y$ , or  $x = y$ , or  $y < x$ .*

Transitivity: *For every  $x$  and  $y$  and  $z$ , if  $x < y$  and  $y < z$ , then  $x < z$ .*

Assuming that  $Y$  is a non-empty subset of  $X$ , an  $<$ -least element in  $Y$  is an  $x$  such that for any  $y$  in  $Y$ ,  $x < y$ . Further, an  $<$ -greatest element in  $Y$  is an  $x$  such that for any  $y$  in  $Y$ ,  $y < x$ . Now we can say that  $<$  well-orders a set  $X$  if the following conditions are met:

(i)  $<$  linearly orders  $X$

(ii) Every non-empty subset of  $X$  has an  $<$ -least element.

Further, we can say that  $<$  conversely well-orders  $X$  if *instead of* (ii) being met:

(iii) Every non-empty subset of  $X$  has an  $<$ -greatest element.

Furthermore, if all three conditions are met, then we can say that  $<$  doubly well-orders  $X$ . Next, following Kripke's suggestion, we can say that a set is finite if it is doubly well-ordered.<sup>110</sup>

Moreover, we can say that a finite sequence is a set that is doubly well-ordered.

Kripke's definition of a finite sequence must be amended to allow that the elements of a given sequence can occur repeatedly in it. This must be so, because digits are abstract types of expressions, which can occur repeatedly in multi-digit numerals; for example the expression-type '2' occurs repeatedly in '227'. Correspondingly, on Kripke's proposal, the object to which '2' refers also occurs repeatedly in the sequence referred to by '227'. Kripke addresses this point with the following remark:

Now, this [definition of a finite sequence] really just defines finite ordering without repetitions, because that's a linear ordering of distinct objects. But then we could say that a finite sequence allowing repetitions is something indexed on such a thing, with a function that is allowed to repeat.

Taking a step back for a moment, the basic idea of indexing is to label or index the elements of one set using the elements of another, by defining a function  $f$ , also called an "indexed system of sets," whose domain is the indexing set  $I$ , and whose range is the indexed set  $J$ . In that case we say that  $J$  is "indexed by" or "indexed on"  $I$ . (We can also say that  $J$  is indexed by the function  $f$  whose domain is  $I$ .) For example, if we were not trying to define the natural numbers or allow for repetitions, then  $\{0 \dots 25\}$  could be our indexing set, the letters of our alphabet  $\{'a' \dots 'z'\}$  could be our indexed set, both under their usual orderings. The resulting indexed system of sets would be a set of distinct ordered pairs, for example:

$$\{(0, 'a'), (1, 'b'), (2, 'c') \dots (25, 'z')\}$$

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<sup>110</sup> This definition is also found in Weber (1906), Staeckel (1907), and Zermelo (2010). There are yet more definitions of "finite set." See Hrbacek and Jech (1999: 72-3).

In this example the letters of our alphabet are indexed by numbers, but not in a way that allows for repetitions.

For this purpose,  $f$  must be (non-trivially) *surjective* or *onto*. That is, for every element  $z$  in the indexed set  $J$ , there is a corresponding element  $x$  in  $I$  such that  $f(x) = z$ , but there is not necessarily a unique  $z$  corresponding to every  $x$  in  $I$ . So  $f$  may map more than one element of  $I$  to the same element of  $J$ . In that case, there will be a  $z$  that is indexed by more than one element of  $I$ , allowing repeated occurrences of  $z$  in the resulting set of distinct ordered pairs. To expand on the previous example, if we were not trying to define the natural numbers, then  $\{0 \dots n-1\}$  could be our indexing set, and the first ten letters of our alphabet  $\{‘a’ \dots ‘j’\}$  could be our indexed set. Then for every letter, there is a corresponding number such that  $f(0) = ‘a’$ ,  $f(1) = ‘b’$  etc., but not necessarily a unique letter corresponding to every number. So  $f$  may map more than one number to the same letter, and in that case the same letter will be indexed by more than one number. Supposing that this is the case for *each* letter, one resulting system might be the set of distinct ordered pairs:

$$\{(0, ‘a’), (1, ‘b’), (2, ‘c’) \dots (9, ‘j’), (10, ‘a’), (11, ‘b’) \dots\}$$

In this example the letters of our alphabet are indexed in a way that allows them to occur repeatedly.

Since Kripke’s task is to define ‘Natural number( $n$ )’ using the notion of a finite sequence allowing repetitions, he of course cannot use the natural numbers as his indexing set. Rather, as the above quote suggests, he proposes to use the more general idea of a doubly well-ordered and so finite set without repetitions as the indexing set. One might still think that a doubly well-ordered set is sufficiently similar to the natural numbers for circularity to threaten. However, this is to overlook a crucial difference between numbers and well-ordered sets more generally, which

is that not every element of a well-ordered set must have a unique predecessor. For example, in the following well-ordered set, the singleton of the null set,  $\{\emptyset\}$ , does not have a unique predecessor:

$$\{\emptyset, \{\{\emptyset\}\}, \{\{\{\{\emptyset\}\}\}\} \dots \{\emptyset, \{\{\{\emptyset\}\}\}, \{\{\{\{\{\emptyset\}\}\}\}\} \dots\}$$

This well-ordered set can be used to index a linearly ordered set in a way analogous to the previous example, thus allowing for repetitions. Further, since the resulting ordered pairs will themselves be used to obtain the finite sequences that make decimal notation structurally revelatory, the indexed elements of each pair must be one of ten objects, for which I will use ‘z’ followed by the first nine letters of our alphabet {‘z’, ‘a’...‘i’}. The resulting system is then the following set of distinct ordered pairs:

$$\{(\emptyset, \text{‘z’}), (\{\{\emptyset\}\}, \text{‘a’}), (\{\{\{\{\emptyset\}\}\}\}, \text{‘b’}) \dots\}$$

Following notational convention for indexed systems of sets, I will denote this system with ‘ $\langle (S_i, <) \mid i \in I \rangle$ ’. ‘ $\langle (S_i) \mid i \in I \rangle$ ’ is intended to indicate a function whose domain is the indexing set  $I$ , and ‘ $<$ ’ to indicate that  $I$  is doubly well-ordered.

Here we encounter a further issue concerning the exact nature of the desired finite sequences. On the one hand, if the sequences in question contain the above ordered pairs, then they will be unsurveyable, due to the inclusion of the first element of each pair from the indexing set. But on the other, if the finite sequences contain only the indexed elements of each pair, then some reason is needed to think that these elements remain indexed, after their indices have been excluded. Of course, in practice we do allow expression types to repeat without indexing them explicitly. But how can we justify this practice in set theory?

An answer may lie in how the aforementioned finite sequences are ordered lexicographically: Because they are well-ordered by  $<$ , they can first be compared and ordered

by their length, since it is a textbook theorem of set theory that if a relation well-orders  $V$ , and a relation well-orders  $W$ , then either  $V$  and  $W$  are isomorphic or one of them is isomorphic to an initial segment of the other.<sup>111</sup> In the event that sequences are of the same length, then they are compared and ordered lexicographically using the linear ordering  $<_J$  on the indexed set  $J$ . For example, suppose that we want to compare the sequences referred to by ‘11’ and ‘12’, which I will refer to as ‘Y’ and ‘Z’ respectively. To do this we have to form the Cartesian product  $Y \times Z$  of the two sequences. However, as already noted, the elements of these sequences are themselves ordered pairs. To deal with this, we can form  $Y \times Z$  from the second element of each pair – the indexed element. This provides a motivation for excluding indices from sequences: their lexicographically ordered *products* do not contain indices.  $Y \times Z$  is then, as usual, the set of all ordered pairs whose first element is from Y and second is from Z:

$$\{(y_1, z_1), (y_2, z_2)\}$$

To fix ideas we can pretend that these pairs contain numerals (although of course the elements of the pairs are really sets, as explained above):

$$\{(1, 1), (1, 2)\}$$

The lexicographic ordering of  $Y \times Z$  is the relation  $<_L$  defined by:

$$(y_1, y_2) <_L (z_1, z_2) \equiv y_1 <_J z_1 \text{ or } (y_1 = z_1 \text{ and } y_2 <_J z_2)$$

This ordering can then be generalized to products of finite sequences. For suppose that some  $k$  such that  $1 < k < n-1$  is the position of the first element such that  $y_k \neq z_k$ . Then

$$(y_1 \dots y_{n-1}) <_L (z_1 \dots z_{n-1}) \equiv y_k <_J z_k$$

Now ‘Natural number( $n$ )’ can be defined as follows:

$$\text{Natural Number}(n) \equiv_{df} n \in \text{the lexicographic ordering on } \langle (S_{(i)}, <) \mid i \in I \rangle$$

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<sup>111</sup> See Hrbacek and Jech (ibid: 105).

The immediately preceding relation can be defined as:

$$P(m, n) \equiv_{\text{df}} m <_{\text{L}} n \text{ and there exists no } p \text{ such that } m <_{\text{L}} p \text{ and } p <_{\text{L}} n.$$

Further, 'zero' can be defined as the least element:

$$0 =_{\text{df}} \text{the } <_{\text{L}}\text{-least element } z \in \text{the lexicographic ordering on } \langle (S_{(i)}, <) \mid i \in I \rangle$$

Now we can see why, on Kripke's proposal, the numbers form a progression. Recall that a progression is a structure that satisfies the following conditions:

*Trichotomy: For every  $x$  and  $y$  either  $x < y$ , or  $x = y$ , or  $y < x$ .*

*Transitivity: For every  $x$  and  $y$  and  $z$ , if  $x < y$  and  $y < z$ , then  $x < z$ .*

*Zero: There is a least element  $x$  such that for every  $y$  other than  $x$ ,  $x < y$ .*

*Successor: For every  $x$  there is a next element  $y$  such that  $x < y$ , but there exists no  $z$  such that  $x < z$  and  $z < y$ .*

*Induction: For any set  $X$  of elements, if the least element belongs to  $X$  and the next element after any element belonging to  $X$  belongs to  $X$ , then all elements belong to  $X$ .*

It is easily seen that the ordering described by Kripke's definition of number satisfies a special case of these conditions. Since his finite sequences are linearly ordered, they satisfy trichotomy and transitivity. Further, since they are length-and then-lexicographically ordered they satisfy the successor condition. Furthermore, since they are well-ordered they contain an  $<$ -least element, so they satisfy Zero. Finally, since they are well-ordered they also satisfy induction, since this is true of any well-ordered set. To see this, let  $Y$  be well-ordered, and let  $X$  be a subset of  $Y$ . We want to show induction: that if the least element belongs to  $X$  and the next element after any element belonging to  $X$  belongs to  $X$ , then all elements belong to  $X$ . It will suffice to suppose that  $((\forall u < x: u \in X) \rightarrow x \in X)$ , to show  $(\forall x: x \in X)$ . Assume for *reductio* that a subset of  $Y$ ,  $U = \{x: \sim$



$x \in X$  is non-empty. Then, since  $Y$  is well-ordered, there is a least element  $x \in U$ . So  $(\forall u < x (\sim u \in U))$ . Then, by the definition of  $U$ ,  $(\forall u < x: u \in X)$ . Then, by our supposition,  $x \in X$ . This contradicts our assumption that  $x \in U$ .

Based on this last result, I think it is fair to say that Kripke's proposal —like Frege's, and like the textbook set-theoretic one stated earlier— is to define the natural numbers in such a way that induction is true of them. Moreover, the proof just given does not appeal to induction.

The time has come to assess Kripke's proposal. Following the presentation of the previous chapters, I propose to do so by asking the following questions:

(Q1) Do Kripke's definitions of the arithmetical primitives explicate the senses of their ordinary arithmetical correspondents as accurately as possible?

(1.a) Do the definiens include anything arbitrary or *ad hoc*?

(1.b) Do the definiens omit anything?

### 11. Assessing Kripke's definition of number

Beginning with question (1.b), the reader will recall that an objection to Benacerraf's proposal is that it neglects features of the numbers that are part of our ordinary arithmetical practice, even if they do not need to be considered when doing number theory, for which the concept of an arbitrary progression will suffice. It should now be clear that, according to Kripke, what Benacerraf neglects is that it is part of the practice of decimal users that the numbers are not just any old progression, but any progression of finite sequences of ten objects that are ordered by length and then lexicographically, and usable for counting.

In what other respects does Kripke's proposal improve on Benacerraf's? One criticism that Kripke makes note of, which is applicable to Benacerraf, is that the notion of a progression

is not necessary for our ordinary concept of number, because one can have concepts of particular numbers without thinking of them in terms of their position in a progression:

I should say, in addition, if one took numbers to be just any old progression, in a way one seems false to our ordinary concept of cardinality, because, after all, someone could and some people do and sometimes we're told that some cultures do have the concept of five without correspondingly having the concept of an arbitrary progression.

To support this claim one might cite studies of small children, who for a period during development are able to recite the numerals up to 10 in a stable order, give you one F when instructed, and distinguish a picture of one F from a picture of any other number of F's by indicating which is which; however, they cannot do this with any other number. For example, when asked to give you two F's they respond by giving a random number of F's other than 1. Further, they cannot distinguish three F's from two F's, although they can distinguish these from one.<sup>112</sup> These so-called "one-knowers" appear to have a concept of the particular number denoted by "one," without thinking of it in terms of its position in a progression. Similar experiments provide evidence for the existence of "two-knowers" and "three-knowers."<sup>113</sup> And as Kripke says, it has been alleged that there are entire "one-knowing" (or "one-many") cultures, such as the Pirahã, who have a concept of the number we denote by "one," without thinking of it in terms of its position in a progression.

I do not believe that this objection can be sustained on the basis of the research just cited, which is subject to the same response that I offered in chapter 3, to Richard Heck's appeal to studies of children. According to this response, the explanation for the phenomenon of one-

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<sup>112</sup> Wynn (1992).

<sup>113</sup> Lecorre and Carey (2007).

knowing is that the subjects in question are either still in the process of learning the concept of number, or simply lack it, with the result that what they know is not relevant to the content of the modern adult conception. Similarly, the claim that the Pirahã have any concepts of numbers at all is hotly disputed.<sup>114</sup> What the proponent of this objection needs to refute Benacerraf and Kripke then, is an example of adults in a numerate culture, who have concepts of particular numbers without thinking of those numbers in terms of their position in a progression.

There is a related and more damaging objection to Benacerraf, which also poses a conundrum for Kripke. It will be recalled from previous chapters that there is evidence for a stage during development when children meet the conditions on counting small pluralities transitively with numerals—including giving the last numeral in answer to the question “how many?”—without understanding the cardinal significance of what they have done. This is shown by the fact that when instructed to ‘Give me  $m$  F’s’ after counting, where  $m$  is the last numeral recited, they give the experimenter a random number of F’s.<sup>115</sup> Rather than drawing erroneous conclusions about our concept of number from what these children know, the problem, in my view, is to explain what they don’t know. To put the point more theoretically, the fact that infants can count transitively, without understanding the cardinal significance of what they have done, shows that they still lack *full* competence understanding of numerals. But what is it that they don’t understand? It is tempting to say that they do not understand that the last numeral recited denotes a particular cardinal number, perhaps the Frege-Russell number. However, as we have already had occasion to note, Kripke has strong reservations about claiming that the digits refer to the Frege-Russell numbers, because these cannot be represented in ZF (see the end of section

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<sup>114</sup> Carey (2009: 302-4).

<sup>115</sup> Wynn (1992).

9). So there is a tension between the concept needed to explain ordinary practice and the one discovered by attempting to represent numbers using set theoretic rigor.

Regarding (1.a) and the charge of introducing *ad hoc* content, Kripke explicitly contrasts his proposal with what he calls the “artificial constructions” of Zermelo and von Neumann, pointing out that, unlike these proposals, his is motivated by reflection on our ordinary practice. However, it can still be objected that his claim that numerical systems with different bases refer to different number systems is excessively counter-intuitive. Of course this aspect of Kripke’s view is not *ad hoc*, since it is proposed to solve a philosophical problem. But, at least in my experience, philosophers balk at the fact that on Kripke’s view the following constructions come out false:

Erastothenesese believed 17 to be prime

$$27_{10} = 11011_2$$

I will now explain why Kripke nevertheless finds his proposal acceptable.

Firstly, one should not make too much of the intuitive hunch that different numerical systems refer to the same number system, or make too much of the corresponding hunch that the above constructions are true, if, like Kripke, one holds the view that competent speakers need not be unreflectively aware of the content of scientific language. In particular, Kripke will say, such speakers need not be unreflectively aware of identifying the numbers as finite sequences of small numbers. Further, this response continues, the aforementioned hunches may be explained away as unreflective prejudices, which fail to be a reliable guide to our concept of number, which in turn has to be discovered by reflection on the use of arithmetical language in our practice.

Secondly, Kripke argues that his proposal that different numerical systems refer to the different number systems accords with the way in which “different” is applied elsewhere in

mathematical practice. In the Whitehead Lectures, Kripke defends this aspect of his view as follows:

Now, we may think, as the mathematician does, that when one thinks of isomorphic structures, when one is considering them only as structures exemplifying a certain structure, one might say that elements across the structures are the same, or can be identified. And so here too we would think of this Frege-Russell number as the same number as this decimal number when these are actually different objects. We could even think of the decimal as denoting the Frege-Russell number corresponding to the decimal number also, since that is really the more primitive concept. But, on the other hand, looking at it another way, these are different set-theoretic objects. So I suggest the same thing in the cross-cultural case too, or the case of a different base. “Do these people who use the base twelve, are they thinking of the same numbers as we are?” Well, yes, because they are thinking of a structure where the important thing for counting, arithmetic and so on, is that it has a zero, a successor operation, and the same plus and times, satisfying the same recursion equations as ours. In that sense, these are the same numbers and their symbols are just two symbols for the same number. In another sense, however, they are thinking of different objects. That is, if one thinks of the structure as a different structure, not the structure of natural numbers but a set-theoretic structure of sequences, they are indeed given a different structure. There is a certain double-think here and Geach's philosophy of relative identity might be suggested, but we don't necessarily have to go so far. And it is a rather conventional mathematical form of double-think, which I think it is not really fair to press one too far on.

Here the idea appears to be that different number systems are in certain respects the same, in that they are isomorphic progressions on which the same operations can be performed. However, *they are ultimately different*, due to the extra structure that each has, in virtue of making numerical systems with different bases structurally revelatory. One may ignore these differences, and speak figuratively, *as if* there is such a thing as *the* progression of natural numbers, in the event that one is interested in the question of which operations can be performed and the identity of these operations. But if one is interested in the identity of objects rather than of operations, then one must attend to their differences.

I now turn to the question of what Kripke means by describing this form of double-think as “rather conventional.” I suspect that he is alluding to the practice of treating entities that are at some level of structure distinct, *as if* they were identical, in the event that any statement about one entity can be reinterpreted, harmlessly, to be about the other. For example, it is customary to, *so to speak*, identify the unordered pair  $(a, a)$  with the doubleton of  $a$ ,  $\{\{a\}\}$ , because any statement in set theory about the former can be reinterpreted to be about the latter. And yet for one who is interested in the identity of objects, these entities must be distinguished, because each has structure that the other does not; for example, the unordered pair contains a recurrence that the doubleton does not. Further, it is customary to identify a binary relation with the set of ordered pairs that are related by that relation, even though relations and sets are distinct. Furthermore, one can identify the following set-theoretic representations of ordered pairs:

$$\{\{x\}, \{x, y\}\}$$

$$\{\{x, \emptyset\}, \{y, \{\emptyset\}\}\}$$

And yet for one who is interested in the identity of objects, these pairs are distinct, since the latter but not the former represents the notion of two objects one of which comes first.<sup>116</sup> To give two numerical examples, it is customary identify a given real number with the partition or ‘cut’ that it makes among rationals, by partitioning them into the set of all rationals that are less than the real in question, and the set of all rationals that are not less. But for one who is interested in the identity of objects, the real number must be distinguished from the cut that it makes. It is also customary to identify the integers and the positive integers, because one can reinterpret statements about negative integers to be about pairs of positive integers. Finally, it is customary to identify the equivalence class of objects related by an equivalence relation, with so-called “abstracts” that are posited with respect to that equivalence relation. To use Frege’s familiar example, lines that are related by the equivalence relation *x is parallel to y*, thereby have in common their *direction*. This is the posited abstract, which is then identified with the corresponding equivalence class of lines, because any statement about the former can be reinterpreted to be about the latter. John Burgess describes this practice as follows:

The abstract with respect to an equivalence may be identified with the set of equivalents. The direction of a line may be taken to be the set of lines parallel to it. Similarly, extensions of second-level concepts can be used to serve any purpose that would be served by abstracts with respect to equivalences on first-level concepts (2005: 23).

And yet there is an irresistible temptation to say that such posited abstracts —the entities that equivalent objects allegedly have in common— are not identical to the corresponding classes of equivalent objects. Returning to Kripke’s proposal, I believe that his intention is to adopt this

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<sup>116</sup> The former is the textbook Wiener-Kuratowski definition, while the latter is due to Hausdorff. I am grateful to Kripke for explaining the latter notion.

pervasive attitude, regarding the identity of mathematical entities, towards natural number systems. They are distinct, because at some level of detail they differ in structure, but can be treated as identical, because statements about each system can be reinterpreted to be about any of the others.

To resist Kripke's line of thought, one could try and argue that he is pushing this pervasive attitude too far, by adopting it in the case of different numerical notations, in which the reinterpretation is almost trivial, and in which mathematicians are not interested in the identity of objects.

Another objection, which Kripke himself concedes, is that his proposal is not sufficient for arbitrary numerals to be immediately revelatory, because very long numerals in decimal ciphered-positional notation are not immediately revelatory. To this Kripke responds that this problem will arise for any notation sooner or later:

There is no notation for natural numbers that will be exempt from this. The advantage of the positional notation is that this problem arises much later than it does arise for the stroke notation, the Frege-Russell numbers, where the problem sets in much quicker.

What has always struck me about this problem is that it is overcome by finding *other* methods for representing very large numbers, such as exponentiation, new symbols, lexical symbols, and mixtures of lexical and numerical symbols. From this, together with the assumption that these other symbols are immediately revelatory, it follows that having a notation that is structurally revelatory of finite sequences is not necessary to explain how we represent numbers in a way that is immediately revelatory. This in turn suggests that there may be another way of explaining the



phenomenon of immediate revelation – albeit one that is deeply indebted to Kripke’s proposal. It is this line of thought that I will pursue for the remainder of the chapter.

The most obvious reason that having a notation that is structurally revelatory of finite sequences is not necessary, is that lexical numerals such as “three-hundred and twenty-seven” are immediately revelatory —as well as being part of a system that we know how to generate in order and use for counting— without being structurally revelatory of finite sequences. Kripke is presumably aware of this fact, and even remarks on the relationship between lexical numerals and decimal positional ones:

Someone in one of my seminars, an anthropologist in fact, emphasized, and certainly correctly, that of course people had the idea of tens, hundreds, thousands, before the invention of decimal, that is, positional, notation. And this was part of the verbal system of many languages. Now, what is this idea? That is really that, I mean, one is given an enormous set. It may be too large, but we know it better grouped in tens. And we can say, “Oh, the set is seven tens and a five.” This process can be iterated. You know, then how many tens are there can also be measured in tens and we call that hundreds, and so on. So that is another feature of – I think, I mean it's really sort of a way of measuring this by putting some structure in it, grouping it into tens, and it doesn't matter the order in which the grouping goes. One might represent this by a certain structural tree diagram, with the tens or some other representation. That this corresponds, as far as it goes anyway, to the positional notation is an important theorem, part of the original motivation of the thing. And it isn't really exactly the same thing as the powers of ten, explicit polynomial representation. But a leap forward is made when we see that all of this can be put in our notation by mere

position and that was the discovery of the Indian mathematicians, transmitted by the Arabs to Western culture.

This requires some unpacking. Here Kripke describes the structure of the decimal lexical system as carving up finite cardinals into powers of 10. These powers are then counted, as in the example he gives of “seven tens and a five,” i.e. seventy-five (assuming that the suffix ‘ty’ denotes 10). Then the symbol for how many powers there are is juxtaposed with the symbol for the power in question. As for the structural tree diagram that Kripke has in mind, this may be something like the following:

Phrase		Digit
Phrase		
Digit	Power	
SEVEN	TY	FIVE

Here I am assuming the following oversimplified grammar:<sup>117</sup>

Phrase  $\rightarrow$  {Phrase, Digit, Power}

Digit  $\rightarrow$  {ONE – NINE}

Power  $\rightarrow$  {TY, HUNDRED, THOUSAND, MILLION, BILLION}

The number denoted can be determined by multiplying the values of the juxtaposed digits and powers, and then adding the resulting products, together with the value of any remaining digits. Following the taxonomy of the anthropologist Stephen Chrisomalis, I will call this structure “multiplicative-additive” (2010: 11-12). In principle, multiplicative-additive lexical systems can be used to represent indefinitely large numbers, by iterating “million” or “billion.” However, this is not done in practice (except humorously), and part of the leap forward to which Kripke refers,

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<sup>117</sup> One way in which this is oversimplified is that it makes no provision for ELEVEN, TWELVE, or the TEENS.

is that ciphered-positional notational systems avoid the need for such iteration, in addition to using a single digit for each power of 10.

Despite the leap forward that ciphered-positional notational systems represent, the fact remains that some multiplicative-additive lexical numerals are immediately revelatory. Further, numerals from such systems might be thought to be immediately revelatory in cases in which positional numerals are not, since both lexical and mixtures of lexical and numerical numerals are more concise than positional numerals, in cases of very large numbers that are reached before one has to start iterating ‘million’ or ‘billion.’ For example, ‘six-hundred trillion’ and ‘600 trillion’ might be thought to be immediately revelatory while ‘600,000,000,000,000’ is not.

I now turn from multiplicative-additive lexical systems to multiplicative-additive numerical notations. One example is the Babylonian “common” system, so-called because it was used for over a period of more than fifteen hundred years (from roughly 2000 BC), for common purposes, including the recording of commercial information and dates.<sup>118</sup> I will now describe this system, as well as the positional system that was also used by a more select group of Babylonians, before discussing whether either constitutes a counterexample to Kripke’s proposal. This will lead me to an alternative explanation of immediate revelation.<sup>119</sup>

## 12. Babylonian notation

The Babylonians represented numbers using accumulations of cuneiform symbols for 1 (a vertical wedge) and 10 (a corner wedge). These symbols, together with further ones for 60,

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<sup>118</sup> See Chrisomalis (2010), Neugebauer (1927) and van der Waerden (1963).

<sup>119</sup> I have chosen to focus on Babylonian notation, rather than on Greek or Roman, because educated Greeks and Romans apparently knew Babylonian mathematics. See Chrisomalis (ibid: 117). Obviously this complicates matters.

100 and 1000, formed the alphabet of their common system. This system was decimal and *non-positional*, instead being multiplicative-additive:<sup>120</sup>

1	10	60	100	1000
∟	<	∟ or ≡<∟	∟	<∟
$305,412 = \text{∟∟∟∟} \text{∟} \text{≡≡≡} \text{<∟} \text{∟} \text{∟∟∟} \text{∟} \text{<∟∟}$ $((3 \times 100 + 5) \times 1000) + (4 \times 100) + 10 + 2$				

Although this notation may *seem* unwieldy to us, and far from immediately revelatory, the following facts should be noted. The complex symbol for 60 is identical to the written lexical numeral for that number, and the symbol for 100 is an abbreviation of the corresponding written lexical numeral; on the other hand, the symbol for 1000 is a multiplicative combination of the symbols for 10 and 100.<sup>121</sup> The upshot of all this is that the system contained only five distinct signs, two of which (the symbols for 60 and 100) were closely related to the lexical system, and one of which (the vertical wedge) was in itself immediately revelatory. This surely would have made long numerals containing these symbols easier to parse than might seem to be the case to us. Further, the system's decimal base and multiplicative-additive structure resembled that of the lexical numerals in the Semitic languages that were spoken at the time, which also had a multiplicative-additive structure.<sup>122</sup> This too would have made numerals belonging to the system easier to parse, since the system resembled another with which the user would already have been familiar. Notice, in this regard, how easy it is for one to say the corresponding English lexical multiplicative-additive numeral-phrase "three-hundred and five-thousand four-hundred and twelve," as one reads the numeral in the above table. Moreover, I see no reason why it would not

<sup>120</sup> Table reproduced with permission from table 7.13 in Chrisomalis (ibid).

<sup>121</sup> Chrisomalis (ibid: 247).

<sup>122</sup> Chrisomalis (ibid: 248). The shift to the common system corresponded with the political ascendancy of Semitic speakers. See Chrisomalis (ibid: 409).

have been equally easy to say the corresponding Semitic numeral-phrases as well, since the Semitic languages also contained words for “ten,” “hundred,” and “thousand,” as well as “ten thousand.”<sup>123</sup> Finally, the following developments from the earlier Sumerian notation would have helped with surveyability:<sup>124</sup>

	4	7	8	9	40
Sumerian					
Babylonian					

The fact that the notation is structured in part for surveyability implies that one should not overemphasize its resemblance to the lexical system. For example, the Babylonians did not say “ten ten ten ten” for 40.

The Babylonian common system must be distinguished from their sexagesimal cumulative-positional system, which was used by a select few working in astronomy and mathematics. In the latter system, numbers up to and including 59 were represented through the accumulation of the aforementioned cuneiform symbols for 1 and 10 (so the system had a sub-base of 10). The number 60 was the first number represented as a power of the base, by the occurrence of the cuneiform symbol for 1 in the second place, which represents 60. The positional principle continued to be used to represent multiples of powers of 60 (that is,  $60^2 = 3600$ ,  $60^3 = 216,000$ , etc.):

$$159,614 = \begin{array}{c} \lll \text{TTT} \lll \lll \text{TTT} \\ (44 \times 60^2) + (20 \times 60) + 14 \end{array}$$

<sup>123</sup> Lipinski (2001: 299-300).

<sup>124</sup> Table reproduced with permission from table 7.14 in Chrisomalis (ibid).

Both systems can in theory be used to generate infinitely many numerals. In the case of the positional system, this can be done in the usual way, and in the case of the common system by iterating symbols multiplicatively, in the way that one could extend our decimal lexical system by iterating “million” or “billion.” However, there is, to my knowledge, no evidence that either system was used for this purpose in practice. What is clear is that the positional system was used to allow the introduction of fractions, which, perhaps not coincidentally, the Babylonians never represented as infinitely recurring sexagesimal expansions, noting instead that the relevant numbers did not divide.<sup>125</sup> Having said that, it may have been obvious to users of the positional system that it could be extended by repeatedly placing symbols in a new column before the highest power. However, the lack of a symbol for zero, which can be concatenated with a numeral repeatedly in an obvious way, may have obscured this fact.

A final point is worth noting. The impressive competence understanding of numbers that the Babylonians must have possessed, in order to discover positional notation, fractions, proto-algebra, compound interest and astronomy,<sup>126</sup> was presumably developed largely by practice at calculating. But there is no actual evidence of calculation being *performed* with numerals, only of results being *recorded* using numerals. This may be because calculation was done with numerals on scratch-pads, which were then erased. But the fact remains that we do not know whether the Babylonians used numerals in calculation, or whether this was done only on tables, fingers and in the head.

I now turn to the question of whether either of their notations constituted a counterexample to Kripke’s proposal, being immediately revelatory without being structurally

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<sup>125</sup> See Neugebauer (ibid: 33). The number 60 has 12 divisors, being divisible by 1, 2, 3, 4, 5, 6, 10, 12, 15, 20, 30, and itself. It thus has more divisors than any number below it. Such numbers are now called “highly composite.”

<sup>126</sup> See Neugebauer (ibid) and van der Waerden (ibid).

revelatory of finite sequences. Since neither notation was ciphered-positional, neither could have been structurally revelatory of finite sequences of objects, in the way that Kripke claims that our decimal notation is (see section 9). That is, internalizing the notation would not have lead them to identify numbers as sequences of objects corresponding to sequences of digits in dictionary order, since the notations contained no such sequences. So the question is whether either of their notations was immediately revelatory. Beginning with the positional system, this surely was *not* immediately revelatory, since understanding it required multiplying by powers of a base of 60; it also would have been harder to parse than the common system, since it did not resemble the lexical numerals as closely, being cumulative-positional rather than multiplicative-additive. But there is evidence that the scribes who used the positional system were also familiar with the common system, since they dated mathematical texts containing position numerals using common numerals.<sup>127</sup> Being familiar with both, they could have translated numerals from the positional into the common one as required.

As for the common system, it seems reasonable to say that a Babylonian merchant or scribe was able to look at the numeral for 412, and find it immediately revelatory:

Although this would have become harder to do as the numerals grew, I see no reason why they could not have done the same for 305, 412:

I say this because, given the facts about this system that I have described in the first paragraph of this section, it is reasonable to say that in addition to knowing how to generate numerals from the common system in order, and use them in counting, they could also parse these numerals, and

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<sup>127</sup> Neugebauer (ibid: 17), Chrisomalis (ibid: 251).

translate them into lexical numerals, all *with little or no conscious effort*. It is also reasonable to say that they were able to meet the equivalent conditions for lexical numerals as well.

My claim is that here we have a plausible example of an immediately revelatory notation that is not structurally revelatory of finite sequences, and whose immediately revelatory nature is instead explained in terms of the user's ability to parse the notation, with little or no conscious effort. Next I will propose that the immediately revelatory nature of our decimal system can be explained in similar terms, without making a claim about how we identify and individuate numbers. But before I continue with this line of thought, I want to address two objections that may have occurred to the reader.

The first objection is that because the Babylonians were still in the process of developing an understanding of the concept of number, what they knew is irrelevant to the modern concept, which Kripke's proposal concerns. My response to this objection is that the Babylonians must have possessed an impressive degree of competence understanding of the concept of number — as well as of particular numbers— in order to discover positional notation, fractions, proto-algebra, compound interest and astronomy. This also shows that their numerals play a similar — although more limited— role in scientific language to ours. Given all this, the claim that what they knew is irrelevant to the modern concept of number requires further argument. Further, this argument must be independent of Kripke's doctrine that different notational systems denote different number systems, since this doctrine follows from the view that notations are structurally revelatory of finite sequences, which is at issue here.

The second objection is that I have here explained immediate revelation in terms of the user's ability to parse lexical and notational *systems*, rather than in terms of their immediate epistemic relation to numbers. It seems then, that I have failed to explain part of what is to be



explained. My response to this objection, which I will state baldly now, in advance of further elaboration, is that according to the version of Scylla that I endorse, the numerals that were understood by the Babylonians did not acquaint them with numbers. However, they found their common numerals immediately revelatory because, in addition to understanding them, they had a parsing facility with their familiar language and common numerals. This is the line that I will continue to elaborate in regards to our decimal system.

### 13. Decimal notation

In my view, ordinary speakers have competence understanding of lexical and decimal numerals (see chapter 2, section 6), for which it is sufficient to know how to recite numerals in order and use them in counting. (In my view full competence understanding of counting presupposes a grasp of numbers, so I will say how we can grasp numbers without acquaintance in the final chapter.) Further, ordinary speakers have a partial understanding of the multiplicative-additive structure of the decimal lexical system (see this chapter, section 11). In addition to all this, a *mathematically* competent user also understands a polynomial descriptive rule, the germ of which is contained in the multiplicative-additive structure of the lexical system. This rule describes the referent of each numeral as the sum of the place-values of its digits (where the place-value of a digit  $\alpha$  is the product of  $\alpha$ 's referent and the number  $b$  of the base to the  $n$ th power, where  $n$  is the number of other digits between  $\alpha$  and the decimal point):

$$\forall \alpha_0 \forall \alpha_1 \forall \alpha_2 \dots \forall \alpha_k \text{Ref}(\lceil \alpha_k \dots \alpha_2 \alpha_1 \alpha_0 \rceil) = \text{Ref}(\alpha_k) \cdot b^k \dots + \text{Ref}(\alpha_2) \cdot b^2 + \text{Ref}(\alpha_1) \cdot b^1 + \text{Ref}(\alpha_0) \cdot b^0$$

This rule suffices to fix the reference of each numeral. For example, it fixes the reference of '70' as follows:

$$\text{Ref}(70) = \text{Ref}(7) \cdot 10^1 + \text{Ref}(0) \cdot 10^0$$

Crucially, however, this rule determines the content of each multi-digit numeral, without itself being a part of that content, and is in this respect somewhat analogous to Kaplan's notion of *character*. Returning to the case of ordinary rather than mathematically competent speakers, it may seem miraculous that they are competent to perform calculations, given that they do not also understand the above rule. However, I claim, their ability to perform simple calculations can be explained by their being competent at pen and paper arithmetic, by writing out numerals in columns and stacking them.

Since our lexical system is multiplicative-additive ('three-hundred and twenty-seven'), while our decimal notation is ciphered-positional ('327'), there is a significant structural difference between the two systems. I claim that this difference exists because each system is structured for a different modality. On the one hand, the lexical system is structured for speech and hearing. (This seems to be true of lexical systems generally. It may be why, for example, cultures like the Babylonians and the Romans, with notations that accumulate symbols for 1, 5 or 10, *do not* have lexical systems with which they say "one one one," or "ten ten ten.") Decimal notation, on the other hand, is structured in such a way that they can easily be read and visualized. The number of digits is small enough to be well within the bounds of what we can remember easily, while still being large enough for the notation to remain so concise that we can, up to a point, survey and visualize multi-digit numerals. Then there is the fact that powers can be represented by position of a single digit. It is because of these structural features that decimal notation helps us to overcome the limitations of our parsing ability.

My next proposal is that a notation should be *visually revelatory*: it should reveal structural features of its subject matter visually, by helping one to see or visualize them. This

provides some grounds for resisting Kripke's claim that, whenever possible, we should ensure that a notation is structurally revelatory (see section 8). The reason is that there may be some tension between the demands of having a visually revelatory notation and the demands of having a structurally revelatory one, with the result that a trade-off between the two is required. For example, decimal notation is visually revelatory, because we can visualize the decimal numerals in order, and this reveals, visually, the ordering of a progression of numbers. Thus decimal notation is somewhat structurally revelatory. But as a result of also being visually revelatory, and so structured to be read and visualized (as described in the previous paragraph), it is not as structurally revelatory as it might otherwise be. For example, it is not as structurally revelatory as stroke notation, which is not structured to be read and visualized. This trade-off can explain why decimal notation is neither structurally revelatory of the Frege-Russell numbers nor of a progression. Thus there are grounds for insisting that decimal notation has structure that is not shared by the numbers, despite Kripke's reason for saying otherwise.

Notice that on this view, while numerals are finite sequences that allow us to represent numbers (in virtue of being visually revelatory), we are not required to conceptualize this feature of them in order to represent numbers, as we are on Kripke's view, by identifying and individuating numbers as finite sequences of objects (see section 8). I regard this as a positive feature of my theory, since it is perhaps overly intellectualized to say that we must have concepts of finite sequences of objects, in order to represent numbers. In other words, my theory is simpler, and so by an inference to the best explanation more plausible than the more intellectualized alternative.<sup>128</sup>

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<sup>128</sup> This general form of argument is indebted to Burge, who used it to argue against various theories that take the knowing subject to represent the conditions that make representation possible. See Burge (2010).

As an aside, a visually revelatory notation can also improve upon a structurally revelatory one, without the need for a trade-off between the two. For a structurally revelatory notation that is hard to read, is improved upon by an equally structurally revelatory notation that is also visually revelatory, and so read more easily. For example, the reader will recall (from section 8) that both infix and prefix notation are structurally revelatory of the recursion equations and the associative law for addition, since the relevant infix and prefix statements have isomorphic parsing trees. But because we are accustomed to parsing blackboard arithmetic and natural language in infix, not prefix, we find infix considerably easier to read, and can visualize repeated successions and additions in infix without conscious effort. We can also visualize these features by reading prefix notation, but only with more conscious effort.

I now turn to my explanation of why decimal notation is immediately revelatory. This is because in addition to having competence understanding of decimal numerals, in virtue of being able to recite them in order and use them in counting, *we can also parse them with little conscious effort*. I separate this into two empirical claims. Firstly, we have a tacit understanding of the dictionary rule that gives the ordering of our decimal notation, which we can apply, in order to locate numerals in relation to one another, without conscious inference. Secondly, we are also able to visualize the decimal numerals in their dictionary order, with relatively little conscious effort. Of course I concede that these abilities can be exercised for numerals from other systems that we are well practiced at reciting in order. The point is that doing so requires more conscious effort. For example, to return to Kripke's counterexample to Ackerman, even after some training in Kripke's base-26 notation, one has to discover where  $a,zzz,zzz$  occurs in relation to many other sequences in the ordering by reciting or calculating; this cannot be known in virtue of being able to apply understood rules and visualize the numerals without discursive

effort. Even after one is so familiar with base-26 notation that the question ‘which numeral is between  $yyy,yyy$  and  $a,zzz,zza$ ?’ is entirely trivial, there will be many other numerals that one cannot locate in relation to  $a,zzz,zzz$  without calculation. It is worth noting that my claim that these abilities can be exercised with little or no conscious effort receives some support from brain imaging studies of children. Subjects who are learning our decimal notation show high levels of prefrontal activity, which is indicative of effort. This activity then vanishes during development, as the notation is mastered.<sup>129</sup>

I have tried to explain why decimal notation has structure that is not shared by the numbers, and to explain why it is immediately revelatory without being structurally revelatory of finite sequences. If I have succeeded, then there is no good reason to say that as a result of being trained in decimal notation, we identify and individuate numbers in a way that reveals what they are. Returning to Russell’s theory of acquaintance (see section 9), it follows that a person who is trained in decimal notation does not satisfy Complete Revelation. Further, in my view such a person does not satisfy Privacy either. This is because they acquire competence understanding of numerals through being trained in a public language, not through private awareness. As for Immediacy, I have explained this in terms of our relative facility in parsing our familiar decimal notation, not in terms of an immediate relation to numbers. For these reasons, I take my proposal to be a version of Scylla, according to which numerals do not acquaint us with numbers. Moreover, if the distinction between an understood notation that is immediately revelatory and one that is not, is simply a matter of our relative facility in parsing decimal notation, then this distinction *does not justify drawing a corresponding distinction in conceptual content*, of the sort that should be reflected in a logicist or set-theoretic logicist system of definitions.

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<sup>129</sup> Dehaene (1997: 269-70).

## Chapter 5: Another application of Kripke's theory

### 1. Introduction

In this chapter I discuss another application of Kripke's theory of structural and immediate revelation, which he uses to defend his doctrine that a certain truth, known by the person who sets up the metric system, is both contingent and *a priori*. Then I begin to develop a version of Scylla, which can explain why users of the metric system find it to be immediately revelatory, without the claim that the system is also structurally revelatory. I also defend the view that the person who sets up the metric system knows how long *S a posteriori*, is in virtue of his facility with the name 'meter'. Defending this view requires a lengthy digression on the topic of context-sensitivity, so I ask for the reader's patience in this regard, especially since the morals that are drawn from this discussion will also be applied in the following chapter, to the topic of count nouns.

### 2. The contingent *a priori*

While Kripke's doctrine that there are necessary truths known *a posteriori* is widely accepted as a major insight, his claim that there are contingent truths known *a priori* is among the most disputed of his doctrines. (In my experience, even philosophers who don't regard it as false are unsure what to say about it.) Kripke's most famous example of a contingent truth known *a priori* is of the truth known by the person who sets up the metric system, by stipulating

(1) The length of stick S at  $t_0$  is exactly one meter.

Kripke's claim is that the person who sets up the metric system (call him 'Ralph') can fix reference of 'meter'—without giving its meaning—by stipulating that it is to be a rigid

designator of the length that is in fact the length of stick S at  $t_0$ .<sup>130</sup> (For brevity's sake I will suppress the qualification 'at  $t_0$ ' and will assume that S exists.) According to Kripke, fixing the reference of 'meter' in this way is sufficient for Ralph to know (1) *a priori*. During his discussion of this example, it is clear that by '*a priori*' a Kripke means simply in virtue of stipulation.<sup>131</sup>

Note that in (1) 'exactly' modifies 'one meter.' This accords with an assumption that I will make throughout this discussion, that 'exact' does not modify the expression 'length', but expressions of our attempts to measure or otherwise represent length.

Another example Kripke gives is that of Leverrier, who is imagined to have fixed the reference of 'Neptune' by stipulating that it is to be a rigid designator of the planet causing perturbations in the orbit of Uranus. (Again, I simply assume that such a planet exists.) According to Kripke, this allows Leverrier to know the contingent truth expressed by (2) *a priori*:

(2) Neptune is the planet causing perturbations in the orbit of Uranus.

Although the contingency of what (1) and (2) express is a puzzling feature of these particular examples, it has been noted that contingency is not required to generate similarly puzzling cases

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<sup>130</sup> Kripke (1980). 'The length of stick S' does not give the meaning of 'meter', since is not a rigid designator. A rigid designator is a term which designates the same object  $x$  with respect to every possible world in which  $x$  exists. With respect to a world in which S is heated sufficiently to expand to a greater than actual length, 'the length of stick S' will instead designate that length. So, while we can fix the reference of 'meter' using the description, these expressions are not synonymous.

<sup>131</sup> Kripke (1980: fn. 26; 1986: 67). What Ralph knows *a priori*, simply in virtue of stipulation, is not a trivial necessary truth but a contingent one, since it is false with respect to the world described in the previous footnote. For this reason, Kripke chooses not to call what (1) expresses an analytic truth, stipulating that analytic truths be both necessary and *a priori* (1980: fn. 21). For clarity's sake I will follow Kripke's terminology.

of knowledge in virtue of reference-fixing stipulation.<sup>132</sup> This point is suggested by something Kripke himself says, when he tentatively suggests that ‘ $\pi$ ’ is a *name* for an irrational number (rather than a rigid definite description), whose reference is fixed by stipulating that it refer to the ratio of the circumference of a circle to its diameter. As a result of this stipulation alone, the reference-fixer seems in a position to know *a priori* what (3) expresses, a truth that is not contingent:

(3)  $\pi$  is the ratio of the circumference of a circle to its diameter.

Kripke’s doctrine that (1) – (3) can be known simply in virtue of reference-fixing stipulation has been met with widespread skepticism, largely due to the following objection: Ralph and Leverrier have not learned new facts about the physical world, *a priori* from their respective stipulations. Rather, all they have learned is that (1) and (2) express truths. Further, the objection goes, in order to learn what (1) and (2) express, they must engage in observation, in which case their knowledge is *a posteriori* not *a priori*.<sup>133</sup>

In his forthcoming Notre Dame Lectures, Kripke responds that the supposition that (1) *cannot be known a priori* leads to the paradoxical conclusion that no one knows how long anything is:

Our eyes are not enough to tell us whether this stick is exactly a meter long. If they were, we wouldn’t have to have a measuring system with rods at all, we could just look at a thing and tell that it’s one meter long.

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<sup>132</sup> To my knowledge the point was first noted by Salmon (1987, 1993). See also Jeshion (2000). For reasons of space I cannot discuss Jeshion’s views on *a priori* knowledge in virtue of reference-fixing stipulation.

<sup>133</sup> To my knowledge this objection was first made in print by Michael Levin. See Levin (1975). See also Donnellan (1977), Salmon (1987), Schiffer (1977), Soames (2003).



And if this is not sufficient, nothing ever will be; that is, if one can't know *a priori* just by this ceremony and the fact this is in front of us (which gives an approximate length for it) that it's a meter long, we will never be able to find this out. Nothing will ever tell us this, even on the assumption that sticks have precise lengths.

I think the puzzle here is something like this. All that goes on here is with our eyes – we're looking at the thing. Then we go through a ceremony of baptizing the length of this thing 'a meter'. Can that really give us any additional information? If we didn't know how long it was just by our eyes alone (exactly), we still don't know how long it is. That is Salmon's thought. So we must need additional information to find out exactly how long it is. But though that thought is a very natural one, once one is in it, one will simply conclude that we can never know how long anything is. It's not that *a posteriori* information is never possibly going to be of any help. How could it be of any help?

Salmon has independently presented a similar argument as a paradox, which can be stated as follows.<sup>134</sup> Assume, for the sake of argument, that (I) Ralph doesn't know how long stick S is *a priori* simply by stipulation. By hypothesis, (II) Ralph doesn't know how long stick S is *a posteriori* by measuring it, since he is hypothesized to be setting up his first measurement system. Further, (III) Ralph doesn't know how long stick S is *a posteriori* by observing its length, since the accuracy with which Ralph is capable of observing and remembering the length of the stick is not exact enough for the purposes of setting up a measurement system. This is because when we observe the length of an ordinary middle-sized object, we represent it as being not exactly but more or less that long —within a certain range of lengths— something I take to

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<sup>134</sup> Salmon (1987: 210-17).

be an instance of the more general and uncontroversial fact that most veridical perception is only approximately accurate. But (IV) if Ralph doesn't know how long stick  $S$  is *a posteriori* by measuring it or by observing it, and doesn't know how long stick  $S$  *a priori*, then he doesn't know how long stick  $S$  is at all. And (V) if he doesn't know how long stick  $S$  is, then all lengths that are measured as a proportion of the length of  $S$ , are thereby proportions of a length we-know-not-what. And (VI) if all such lengths are thereby proportions of a length we-know-not-what, then no one knows how long anything is. Since this conclusion is absurd, one of the forgoing premises must be rejected despite its appearance of plausibility, and the mistake in our thinking that gives it this appearance of plausibility must be isolated.

As Salmon points out, an analogous puzzle arises in relation to  $\pi$ . Analogously to how we are unable to know exactly how long  $S$  is by looking at it, we also are unable to know exactly which number is the ratio of a circumference of a circle to its diameter. For we cannot perceive the ratio exactly, and neither can we calculate it, because the resulting decimal expansion fails to display a pattern that we know of. Should we conclude that all circumferences calculated using  $\pi$  are thereby proportions of something-we-know-not-what, with the result that nobody knows what the circumference of any circle is given its diameter? Surely not, since people are often credited with knowing such things.

### 3. Kripke's proposal

Kripke proposes to defend his doctrine that (1) can be known simply in virtue of reference-fixing stipulation, by showing that this doctrine can solve the above paradox. I will now describe and develop Kripke's proposal, beginning with the question of how it relates to his doctrine of immediate revelation.

In the previous chapter I characterized Kripke's proposal about numbers in the following way. We users of decimal notation learn to parse, or identify and individuate, numbers as finite sequences that make decimal notation structurally revelatory; further, having done so, we find decimal multi-digit numerals to be immediately revelatory. This is because our identification of numbers as finite sequences provides a standard for knowing which number we are confronted with, and decimal numerals present numbers as such sequences. I also characterized this proposal in terms of the following "two-stage" Russellian acquaintance theory. At the first stage we are acquainted with small numbers, because we can picture them. At the second stage our acquaintance with small numbers, together with our training in the decimal system, makes decimal numerals immediately revelatory, for the reasons given above, and so acquaints us with larger numbers.

Kripke motivates his proposal about the meter by drawing an analogy with his proposal about numbers.<sup>135</sup> He introduces it by discussing why we find decimal notation to be immediately revelatory, first describing Charybdis, Scylla, and then anticipating his proposal in the Whitehead Lectures, as

an absolutist position, so that, although knowing which depends on one's training, it's that knowing which is absolute, and, the position says, if you have such-and-such training, you will know which number it is. Something like that might be true, or it might be a picture that's in between. At any rate it depends, somehow, on the fact that decimal is our standard system of notation.

He immediately goes on to draw an analogy with the metric system:

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<sup>135</sup> This analogy is not appreciated in Steiner (2011), who was writing without knowledge of the *Notre Dame* lectures.

Something like that is going on in the case of a meter. If we're going to have a measurement system at all that is specified within enough limits – with exact enough limits to go beyond our eyes – we must do it by something like a rod, or by lines on a spectroscope or something – but something that goes beyond our eyes. We can baptize, then, this length, 'one meter' (or whatever). How does this give us [immediate revelation]? Because once we've come to think in the metric system and have the sufficient minimal acquaintance given by our eyes, then to know that something is two meters long is to know how long it is...

Shortly afterwards, he continues:

The objector thinks, 'Look there's something magical. Just as Wittgenstein ascribed an extraordinary property to the stick, so does Kripke. One can know by this stick, without making any measurement, exactly how long it is, whereas most ordinary sticks are not like that. Or, certainly one can know how long it is without measurement within a margin of error that is much less than is given by our eyes, yet no ordinary stick is to be like that. And what's so special about this stick?' Well, what's so special about this stick is that it is the basis of the measurement system we use.

This material was delivered prior to that in the Whitehead Lectures, and is arguably a less developed version of the view stated there. With this in mind, I will now develop Kripke's proposal about the meter in a way that is more clearly analogous with his later view, since this is consistent with how he has taught the material in seminar.

Kripke says that users of the metric system "come to think in the metric system" (ibid). As I understand him, this means that they learn to parse, or identify and individuate lengths in a

way that makes the metric system structurally revelatory. That is, they learn to identify and individuate lengths as length in meters. This is analogous to how users of the decimal system learn to identify and individuate numbers as finite sequences of ten objects, except that users of the metric system learn to identify and individuate lengths as having metric structure (not decimal structure), where metric structure consists of the length of S laid end-to-end. Further, as a result of making this identification, users of the metric system find measurements in the system to be immediately revelatory. Kripke says this is because “once we’ve come to think in the metric system and have the sufficient minimal acquaintance given by our eyes, then to know that something is two meters long is to know how long it is...” (ibid). On my development, this means that (a) the user’s identification of length as length in meters provides a standard for knowing exactly how long something is, and (b) a measurement of length in the metric system presents it as length in meters. This development, I am convinced, accords with Kripke’s intent. I will now discuss whether this is a “two-stage” Russellian acquaintance theory, and how acquaintance relates to having an identification length in meters. What follows is a slightly more speculative development of Kripke’s views than what came before.

Kripke is careful to draw a distinction emphasized by Burge (see chapter 1, section 10) between the way that a property is presented in perceptual experience, and the property itself. At the first stage, Ralph is introspectively acquainted with how the length of S is presented to him in perceptual experience. But such introspection of one’s own percepts does not satisfy the requirement of Complete Revelation (see chapter 1, section 8 and chapter 4, section 9). For Ralph is *not* introspectively acquainted with *the length* of S, but with *how* the length of S is presented to him in veridical perception; further, the length of S is not presented to him *exactly* in veridical perception. In order to increase precision, Ralph uses his introspective acquaintance to

fix the reference of ‘meter’ by use of the following definite description: ‘the length that is presented to me though *this* veridical perception of S.’

At the second stage, Ralph’s choice of S, and his introspective acquaintance with how the length of S is presented to him, lead him to parse, or identify and individuate length as length in meters. More precisely, Ralph has a combination of: (i) an introspective acquaintance with how the length of S is presented to him, and (ii) an understanding of the content of the description just mentioned. Together these suffice for him to have an identification of length as length in meters.

As a result of his having this identification, the metric system is now immediately revelatory for Ralph. This is because (a) his identification of length as length in meters provides the only standard for knowing exactly how long something is, and (b) metric measurement of length presents it as length in meters.

Another important point is that remembering how the length of S is presented—in the form of a approximately accurate visual image—is necessary to continue to *understand* ‘meter’, Kripke argues, for otherwise Ralph could go on to apply ‘1 meter’ to things that are an inch or a mile long:

Someone who thought that a meter was a mile long or, say, ten miles long (even though he remembered it was the length of a certain stick but somehow got a fantastic idea of how long that stick was, or had never seen the stick... even though he remembered the definition ‘*that*, the length of this stick’ (and so on)) probably shouldn’t be said to be using the term ‘meter’ properly. ‘Oh, look, you’re asking me to walk a *meter* just to get to this place. I have to go by car’. Such a person probably doesn’t know what a meter is and isn’t using the term correctly even though he remembers the stick but somehow has gotten this wild idea of how very long it was.

Further, given that remembering how the length of S is presented is necessary to continue to *understand* ‘meter’, it seems to be Kripke’s view that how the length of S is presented is in some way a part of the semantic content of ‘meter’, as well as being used to fix its reference.

It remains to say how the account offered in the last four paragraphs would solve the paradox stated in section 2, by allowing Kripke to deny its first premise, that Ralph doesn’t know how long stick S is *a priori* simply by stipulation. To this Kripke says (of himself, rather than of Ralph):

I can’t wonder, in this situation, whether it will turn out that I was under no illusion, had these experiences, and was looking at S, but S was not really one meter long.

Here this idea appears to be that Ralph knows *a priori* the contingent proposition expressed by the following:

(1’) The length that is presented to me through this veridical perception of S is exactly one meter.

The proposition expressed by (1’) is contingent, because with respect to a possible world in which S is indiscernibly longer than it actually is, ‘the length of S that is presented to me through this veridical perception of S’ will designate this indiscernibly longer length. Further, this proposition is arguably known *a priori*, because although perception is required for Ralph to *apprehend* the proposition, by introspection of how S is presented in perception, Ralph’s *justification* for the proposition is his reference-fixing stipulation.

To spell out why this is, it will help to think of the proposition in question as containing a complex, demonstrative-involving individual concept of how the length of S is presented to Ralph (I refer to this individual concept using Salmon’s carrot quotes). On this development (1) expresses (*P\**):

$\hat{=}$   $\hat{<}$  the length that is presented to me through this veridical perception of  $S^\wedge$   $>$ ,  
 $\hat{<}$  the length of  $S^\wedge$   $>$

Now it is easy to see how, according to this view, Ralph can come to know  $(P^*)$  *a priori*. By introspective acquaintance with his percepts and by his reference-fixing stipulation, he understands that ‘meter’ expresses  $\hat{<}$  the length that is presented to me through this veridical perception of  $S^\wedge$   $>$ . And by his understanding of the concept expressed by ‘veridical’, he can infer that the length presented to him through his veridical perception of  $S$  is indeed the length of  $S$ ; so he can infer  $(P^*)$ . This conclusion is reached based on introspection, his reference-fixing stipulation and reflection on relations among his concepts, and so is known *a priori*. What is crucial is that although veridical perception is required to apprehend  $(P^*)$ , it does not justify any step in the above line of reasoning.

If I have understood Kripke correctly, then his explanation of how Ralph knows  $(P^*)$  *a priori* requires attributing to Ralph grasp of concepts of the way that a property is presented in perceptual experience, including the concepts of veridical perception and of presentation to the subject. However, while veridical perception and presentation to the subject are presumably necessary conditions for fixing the reference of ‘meter’, it is perhaps overly intellectualized to say that ordinary speakers, like Ralph, must have the concepts of these conditions in order to fix a reference. Further, while this is not a conclusive objection to Kripke’s view, it does show that before we accept his view, good explanatory practice requires us to look for a simpler account that does not require attributing grasp of the aforementioned concepts, even if it follows from this account that Ralph knows  $(P^*)$  *a posteriori*. (Compare my remarks about Kripke’s theory of number, in chapter 4, section 13.)



#### 4. Scylla again

Recall that in the previous chapter I developed a version of Scylla, in order to explain why decimal users find the decimal system immediately revelatory, without the claim that decimal numerals acquaint us with numbers. Now I will begin to develop an analogous version of Scylla about the meter. (See chapter 4, section 7 for Kripke's description of Scylla, and sections 12 and 13 for my version of that doctrine.)

The basic idea behind my version of Scylla is that immediate revelation is a matter of our facility with language, rather than a matter of genuine acquaintance. In particular, my version of Scylla about decimal notation is based on my argument that decimal notation is visually revelatory, and so structured to be read and visualized, and so not as structurally revelatory as it might otherwise be. However, there is an important disanalogy between the case of the metric system and the case of decimal notation, since there seems to be no basis for saying that the metric system is visually revelatory. On what basis then, can I develop a version of Scylla in the present case?

In my view, the metric system is structured to be used in a society that already uses a decimal system. In particular, there is a strong analogy between the metric system and the *lexical* decimal system. For one thing, just as cultures with notations that accumulate symbols for 1, 5 or 10 do not have lexical systems with which they say 'one one one,' or 'ten ten ten,' so users of the metric system who measure length by laying meter sticks end to end, do not say 'meter meter meter,' or 'decameter decameter decameter.' For another, the metric system carves up distances into powers of 10 meters (decameters, hectometers, kilometers), analogously to the way in which the decimal lexical system carves up finite cardinals into powers of 10 (tens, hundreds,

thousands, etc.; see chapter 4, section 11). For example, just as ‘seven thousand’ arguably has the following tree structure:

Phrase	
Digit	Power
SEVEN	THOUSAND

so ‘kilometer’ arguably has this structure:

Phrase	
Unit	Power
METER	KILO

I think that this observation is the starting point from which to develop a version of Scylla, which can in turn explain why metric users find the metric system to be immediately revelatory, without the claims that the system is structurally revelatory, and that the metric measurements acquaint us with length in meters. Since measurement systems are not my main topic, I will only offer a sketch of what my account will look like, leaving the details for further work.

In my view, it is not that users of the metric system learn to parse, or identify and individuate lengths as length in meters, in a way that makes the metric system structurally and so immediately revelatory. Rather, they find the metric system immediately revelatory because it is easy to parse. (Again, compare chapter 4, sections 12 and 13.) This is because its structure resembles that of the decimal system, with which they are already familiar, and which makes converting from kilometers to meters extremely easy. In fact, because the metric system carves up distances into powers of 10, one can use features of decimal *notation* when converting from meters to kilometers: just add three 0’s. In contrast, while users of the imperial and U.S customary systems of measurement find simple measurements like ‘300 yards’ to be

immediately revelatory—they do not find the whole system to be so. How far is 3 furlongs? Answering this question requires applying a rule for conversion, because the imperial system does not carve up distances into powers of 12 inches, and so one is required to remember a hodge-podge of units and rules for converting among these.

I have not yet explained why users of the imperial system find simple measurements such as ‘300 yards’ immediately revelatory, while users of the metric system find ‘300 meters’ to be so. Since, in both cases, this can be partly explained by finding ‘300’ to be immediately revelatory, what remains to be explained is their preference for different units. Obviously this will be a function of their training, which in turn presupposes the introduction of a unit. Apropos of this, I have not yet solved the paradox about Ralph, who is supposed to set up the first system by introducing the term for a unit. Since the basic idea behind my version of Scylla is that immediate revelation is a matter of our facility with language, the basic idea behind my attempt at a solution to the paradox will be that Ralph knows how long S is because he understands the name ‘meter’ that he introduces. The challenge is then to explain how this can be so, without appealing to Kripke’s doctrine that Ralph identifies length as length in meters. It is to this that I now turn.

##### 5. A contextualist response to Kripke’s proposal

According to Salmon, whether one knows how long stick S is (i.e. in what amount Stick is long), is interest-relative, in the way that it is allegedly interest-relative whether one knows to whom Jones is married. Further, in Salmon’s view, to say that whether one knows to whom Jones is married is interest-relative, is to say that someone whose cognitive situation is unchanged, can be correctly described as knowing to whom Jones is married relative to some interests, but correctly described as *not* knowing to whom Jones is married relative to others,

because the truth conditions of such attributions vary in the context of utterance, being dependent on the interests and purposes of those who make them.<sup>136</sup> And the reason for saying this is that in reply to a piece of identifying information  $\phi$  about Jones' spouse, such as 'Jones is married to Mary Fisher, the historian', one can always ask 'but who is  $\phi$ ?' relative to an interest in some other way of identifying Jones' spouse. Call this "indexicalist contextualism," because the truth conditional contribution of 'knows wh' is always relative to some index  $i$ .

As for whether Ralph can be correctly described as knowing how long S is, Salmon's views on this accord well with my version of Scylla, according to which one knows how long an object is in virtue of one's facility with a name: Relative to the usual interests we have when making ascriptions of the form 'x knows how long y is', Ralph is correctly described as knowing how long an object is, if, and only if, he can produce the standard name of the object's length (e.g. '3 meters'), while understanding its meaning. Normally this would require measuring the object in question. However, in the special case under discussion in which Ralph sets up the first system, he can know how long S is just by (a') knowing his own intention in introducing the standard name 'meter' for the length of S, while (b') looking at the stick, which gives him the requisite understanding of the name. So, if non-philosophical interests are in play, then premise (III) can be denied and there is no paradox. Rather, Ralph knows how long S is *a posteriori*. On the other hand, if the interests of a skeptical philosopher are in play, then while the argument that generates the paradox is sound, the conclusion loses its paradoxical bite, and should not bother anyone who is interested in whether Ralph knows how long S is in the "ordinary" sense, but not interested in knowing this in the philosophical sense.

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<sup>136</sup> See Salmon (1987). See also Boer and Lycan (1986).

Similar considerations apply to the case of  $\pi$ . Relative to one set of interests, one is correctly described as knowing what number  $\pi$  is, if, and only if, one can produce the standard name ' $\pi$ ', while understanding its meaning. This will require knowing that ' $\pi$ ' designates the ratio of the circumference of a circle to its diameter, in which case many of us count as knowing what number  $\pi$  is. This is despite the fact that one is also correctly described as not knowing what number  $\pi$  is, relative to the interests of a skeptical philosopher. This is because there is no number,  $n$ , such that  $x$  can know, of  $n$ , that  $n$  is  $\pi$ , relative to these interests.

In order to explain why there appears to be a paradox in the first place, Salmon makes two claims. Firstly, as already noted, he claims that the truth conditions of attributions of knowledge-wh are sensitive to the interests of the person making the ascription in the context of utterance. Secondly, he claims that our interests can shift from the everyday to the philosophical “without our noticing it” (1987: 214). When this happens, Salmon claims, we find ourselves denying that Ralph knows how long  $S$  is, and denying that anyone knows what number  $\pi$  is, without realizing that we mean this in the philosophical sense but not in the ordinary sense. More about these claims in a moment.

Salmon's view must be distinguished from another kind of contextualist view, according to which the interests of the speaker are part of the circumstance of evaluation with respect to which attributions are evaluated for truth, rather than part of the context of utterance. On this view, while the contents of attributions of knowledge-wh are invariant, their interest-relativity is captured by the fact that they are true relative to some circumstances and false relative to others, because some circumstances contain features that compel us to judge them true while others do not. Thus, on this view, their truth-value varies, in the way that the truth-value of 'John is as tall as my father' varies with the time of evaluation even when its content is held fixed.

Kripke is dubious of the claim that attributions of knowledge-wh are interest-relative, and correspondingly unwilling to accept Salmon's solution to the paradox. He objects that in response to the attributions

(4) Ralph knows to whom Jones is married,

(5) Ralph knows how long S is,

(6) Ralph knows how many roots this equation has,

the audience will not say 'Are you sure? What are the relevant interests?' These questions simply do not arise. Rather, Kripke claims, the audience will find the attributions perfectly clear.<sup>137</sup>

Further, this point is not limited to cases where the audience already knows how to look for the relevant interests in the context or finds them obvious. Rather, it is that the audience does not have to look for interests *at all* to understand these reports. Against this, one might worry that even in cases where the indexicalist approach to epistemic paradoxes is somewhat plausible, the question 'What are the relevant interests?' would not naturally arise. For example, it would be a rather weak argument against David Lewis' epistemic contextualism to observe that the same question cannot naturally arise in the case of 'S knows p.' However, Kripke's point can be developed more forcefully as follows.

In general, we notice when the context of utterance or circumstance of evaluation shifts as we use context-sensitive expressions. Beginning with the context of utterance, suppose that somebody utters 'it's raining,' first in a context in which it expresses that it's raining in New York, and then in a different conversational context in which it expresses that it's raining in Boston. (She could be in New York, talking on the telephone about the weather to someone in Boston, before telling the person sitting next to her about the weather in Boston.) In this event,

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<sup>137</sup> Kripke (2011a)

the speaker will notice the shift in the context, as well as the corresponding difference in what is expressed. A more specific version of this point applies to epistemology. According to Salmon's proposal, (4) – (6) can express different truth conditions relative to different interests, without us realizing it. But this must be wrong, because we *are* sensitive to genuine contextual shifts in epistemic threshold, as is borne out by the fact that our intuitions shift. Further, since, for the most part, we *do* notice when the context of utterance shifts as we use context-sensitive expressions, but we very often *don't* notice when the interests shift when we make the attributions labeled (4) – (6), these attributions cannot be context sensitive, and the standards for knowing how long something cannot shift with the context of utterance. Further, the same point applies regarding shifts in the circumstance of evaluation. For example, we do notice why the truth-value of 'John is as tall as my father' varies with even when its content is held fixed.

Next I will consider whether the truth conditions of attributions like (4) - (6) are absolute and context invariant. Then I will return to the topic of interest relativity, in order to amend Salmon's proposal.

## 6. Invariantist and ambiguity theories

One proponent of an invariantist view is David Braun, who claims that the truth conditions of attributions of knowledge-wh are invariant and extremely lax, requiring that the subject simply be able to produce an answer to the relevant wh-question that is not simply a transformation of that question. For example, suppose that Ralph asks 'Who is Hong Oak Yun?'. According to Braun, if Ralph is given the answer 'A person over three inches tall', he thereby knows who Hong Oak Yun is!<sup>138</sup> Perhaps Braun would even claim to have solved the paradox

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<sup>138</sup> Braun (2006).

posed earlier. For Ralph can produce an answer that is not simply a transformation of the question ‘how long is stick S?’, viz. that stick S is 1 meter long.<sup>139</sup>

What Braun must explain is why ordinary speakers will deny that Ralph knows who Hong Oak Yung is, when on the invariantist theory he can be truly said to do so.

The usual response to this sort of objection is that speakers will refrain from asserting these things (e.g. ‘Ralph knows who Hong Oak Yung is’) to avoid the misleading implicature that Ralph knows an answer that is informative to the questioner. However, this response does not go far enough, since it can’t explain why we not only refrain from asserting that the subject knows who Hong Oak Yung is, but also deny this. In response to this, Braun proposes a radical error theory, claiming that speakers often deny or assert the negation of attributions of knowledge-wh when these attributions are literally true, because they confuse what is informative for what answers the question. The problem with this claim is that attributions of knowledge-wh are used to distinguish people with informative answers that are the basis for thought and action, from people who do not have such answers. But according to Braun’s theory, the meaning of such attributions comes apart from their use, as I have just described it, to an excessively implausible extent. Moreover, on Braun’s view, although the question ‘Do you know who Hong Oak Yung is?’ is meaningful, there is no reason to ask it, since upon hearing the question your interlocutor will automatically know the answer. But we can imagine an interrogator asking: ‘Do you know

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<sup>139</sup> I am not sure if Kripke has ever held an invariant and lax view. The Braunian solution to the paradox is obviously reminiscent of Kripke’s remarks about the meter in *Naming and Necessity*. Further, while Kripke does not commit himself to standards that are as lax as Braun’s, he does claim in *Naming and Necessity* that Ralph knows who Cicero is if he knows that Cicero is a famous Roman orator. See Kripke (1980: 83).



anything at all about who Hong Oak Yung is?' Braun will agree that the question is meaningful, and yet on his theory there is no reason to ask it.<sup>140</sup>

I have just argued that the truth conditions of attributions of knowledge-wh are *not* invariant and lax. So if these truth conditions are invariant, then they must be to some degree strict. One standard of strictness, proposed by David Kaplan, requires the subject to be able to recognize the object in question, in the sense of being able to apply antecedently held information to it.<sup>141</sup> Kripke himself is surely sympathetic to this standard of strictness in the case of the meter, since his proposal is that Ralph knows how long S is because he meets an identifiability requirement (see section 3). Ralph's identification of length as length in meters provides the standard for knowing exactly how long something is, and as such is antecedently held information that Ralph can apply to subsequent measurements of length.

As regards knowing who someone is, the problem for the strict theory is to explain why we can say that a subject knows who someone is, even though they fail to meet this standard. For example, I can be said to know who Payton Manning is, even though I am unable to recognize and identify him in the street because I have only caught a glimpse of his face beneath his helmet. Further, we can deny that a subject knows who someone is in certain contexts, even when she *is* able to recognize and identify him. For example, a subject who can be said to know who the thief is because she can recognize him in the street when he is wearing his thief-costume, can also be said not to know who the thief is when she fails to recognize him in the context of a police lineup.

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<sup>140</sup> Thanks to Gary Ostertag for these examples.

<sup>141</sup> Kaplan in Almog et. al (1989).

Kripke suggests, tentatively, that attributions of knowledge-wh possess a kind of ambiguity that is neither properly described as context-sensitivity nor as interest-relativity.<sup>142</sup> Anticipating this proposal, Boer and Lycan claim that such ambiguity would be “monstrous” (1986). This is because positing it would require a different reading for every interest, as shown by the fact (already noted) that in response to a piece of identifying information such as ‘Cicero is a Roman orator’, one’s interlocutor can always ask ‘Yes, but who is this Cicero, the Roman orator?’ relative to some interest.<sup>143</sup> That is, one might already know that Cicero is a Roman orator without being able to identify him further. So to be told ‘Cicero is a Roman orator’ will fail to identify him relative to the relevant interests.

Kripke points out that the kind of ambiguity he is inclined to think exists in ‘knows who’ is “much more rare in the case of ‘knowing which’ as applied to objects in general” (Kripke, 2011a, pp. 344-5). He also points out that a source of ambiguity is that attributions of knowing-who can be read *de re* as well as *de dicto*, a fact that may result from a scope ambiguity.<sup>144</sup> On its *de re* reading ‘Thelma knows who the thief is’ says that the thief is such that Thelma knows who he is, although Thelma does not know that he is the thief. Such an attribution may be true of, for example, the thief’s next-door neighbor, who may recognize and know her neighbor by name but have no idea that he is the thief. In what follows I confine my attention to the *de dicto* reading.

Kripke does not say exactly what form the posited ambiguity might take. If it is a straightforward lexical one, then we would expect the locution ‘knows wh’ to be disambiguated into distinct polysemes such as ‘knows wh<sub>1</sub>’ and ‘knows wh<sub>2</sub>’, expressing distinct ways of knowing who someone is, or which F something is. But it is doubtful that this is what Kripke has

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<sup>142</sup> Kripke (2011a).

<sup>143</sup> Boer & Lycan (ibid).

<sup>144</sup> Kripke (ibid). The *de re/de dicto* ambiguity in ‘knows who’ was first pointed out by Kvat (1982).

in mind, since the usual diagnostic tests, including ones used by Kripke himself, do not suggest that ‘knows wh’ is lexically ambiguous.<sup>145</sup> Surely there are not various senses of ‘knows who’ or ‘knows how long’ that are clear enough to be entered into a dictionary, and we would not expect that ‘knows wh<sub>1</sub>’ and ‘knows wh<sub>2</sub>’ translate into distinct words in some foreign language (although this test yields false positives).<sup>146</sup> Further, if we form a conjunction in which each polyseme occurs and then elide one of them, there is usually a noticeable oddness, resulting from the fact that the elided polyseme has a different meaning from the unelided one that that the former’s meaning is recovered from. (This phenomena is known as ‘zeugma’ or ‘syllepsis’). For example:

(7) While running in Pamplona, Javier was impaled on a horn. Later he performed ‘Stormy Weather’ on one.

But little or no oddness results in the following case, where we suppose that Ralph only knew that Cicero was a Roman orator:

(9) Ralph knew who Cicero was and Catiline did too.

This in itself is inconclusive, since some extremely clear cases of polysemy are not detected by this test either, as in Chomsky’s example ‘France is hexagonal and a republic.’ However, there is more data. If ‘knows who’ is ambiguous then, just as there is a non-contradictory reading of

(10) The bank of the river Hudson isn’t a bank,

so there should be a non-contradictory reading of

(11) Ralph knows who Cicero is and doesn’t know who Cicero is.

But this reading takes great effort to detect.

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<sup>145</sup> These are found in Zwicky and Sadock (1975) and Kripke (1977, 2011a, 2011b) and are summarized in Sennett (2011).

<sup>146</sup> For example, Croatian contains different lexemes corresponding to ‘Uncle on your father’s side’ and ‘uncle on your mother’s side’. But ‘uncle’ is not ambiguous. See Sennett (ibid).

I now turn to the question of whether the way we use hedging terms like ‘really’ and ‘strictly speaking’ can cast any light on the semantics of attributions of knowledge-wh, and the question of whether these attributions are in any way interest-relative.

### 7. Hedging

Immediately after his discussion of context-sensitivity and ambiguity, Kripke introduces his notion of a “toy-duck case,” giving an example in which a parent corrects a child by saying of a toy duck ‘that’s not a goose, it’s a duck’.<sup>147</sup> Kripke’s point is that we should not conclude from interest-relative uses like this, that the meaning of the expression ‘duck’ is interest-relative, ambiguous, context-sensitive, or broad enough to encompass both toy ducks and waterfowl. Kripke also proposes a way of testing for such cases, the felicity of the parent’s utterance of (12) being a sign that the above example is a toy-duck case:

(12) Of course it isn’t *really* a duck.

Since ‘duck’ does not express something encompassing both toys and water-fowl, (12) does not simply cancel a previous implicature that the thing in question is a real duck, or contract the previously dilated extension of ‘duck’. Rather, in uttering (12) the speaker signals that ‘duck’ is being used in accordance with either its dictionary or technical meaning, rather than being used in the interest-relative way it was when uttering ‘that’s not a goose, it’s a duck’. Thus the parent could also have said:

(13) Of course, *technically* it isn’t a duck.

This connects Kripke’s observation about toy duck cases with another observation for which he is more famous, namely that we use words in a way that is deferential to our linguistic

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<sup>147</sup> Kripke (2011a). See also Austin (1962). Thanks to Bob Fiengo for helpful discussion of hedging.

community. I will begin by describing the relation between hedging expressions and semantic deference, beginning with ‘technically’, ‘strictly speaking’ and ‘truly’ before returning to the more complex case of ‘really’.

Speaking technically can involve deference to experts, as in:

(14) *Technically*, nothing is flat.

Here the speaker uses ‘technically’ to signal that what follows is spoken in a way that is deferential to experts, thereby hedging against the risk that what follows will be taken as false.<sup>148</sup>

Speaking technically can also involve signaling that what follows is spoken in accordance with the primary meaning of an expression, of the sort that belongs in a dictionary. For example:

(15) *Technically*, a whale is a mammal.

Likewise, if John has just got divorced but is still living with his ex-wife, one can say:

(16) *Technically*, John is now a bachelor.

Speaking strictly or literally can also involve speaking in accordance with (one of) the dictionary meaning(s) of an expression.<sup>149</sup> For example,

(17) *Strictly speaking/literally speaking*, a comic book is a book.

(17”) *Strictly speaking/literally speaking*, a stable is not a farm.

But the speaker can also use ‘strictly speaking’ to signal that what follows is spoken in deference to experts. For example ‘strictly speaking’ can be substituted for ‘technically’ in (14). What is

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<sup>148</sup> See Kay (1983) on ‘Technically’ and semantic deference. George Lakoff claims hedging words are “words whose job is to make things fuzzier or less fuzzy” (Lakoff, 1973). While the role of hedging words in dilating and contracting the boundaries of vague terms is of great interest, I cannot discuss it here. Neither am I here interested in cases in which ‘really’ is used to intensify gradable adjectives, as in ‘John is not just tall but really tall’. Nor am I interested in cases in which ‘really’ and ‘literally’ are used for emphasis, as in ‘I’m literally dying to see you’, ‘I really need you to be on time’ and ‘John is my brother but he’s really more of a friend.’

<sup>149</sup> Arguably ‘book’ is now ambiguous. Witness the syllepsis in: “John downloaded a book and then spilt coffee on it.” Further, one can easily imagine ‘book’ translating into a different foreign words corresponding to ‘paper-book’ and ‘e-book.’

common to (13) - (17”) is that the speaker provides meta-linguistic information that is not part of the content asserted. Rather, this information contributes to a context, perhaps by establishing a register, in which the audience should interpret and evaluate what they hear in accordance with standards, these being entered in a dictionary or otherwise set by relevant members of the linguistic community.

‘Real’ and ‘really’ are more complex. ‘Real’ in (18) does the same job as ‘really’ in (12), allowing the speaker to signal that ‘diamond’ is to be understood in terms of its dictionary or technical meaning (despite there being no explicit indication that meta-linguistic commentary is taking place):

(18) Those white sapphires aren’t *real* diamonds.

Further, ‘Real’ and ‘really’ —like ‘true’, ‘truly’ and ‘actually’— can also be used to attest to the fact that something is not a hoax, since one can point to a holographic diamond and say:<sup>150</sup>

(19) That isn’t a *real* diamond.

Further, ‘real’ and ‘really’ —again, like ‘true’, ‘truly’ and ‘actually’— can be used, in contrast to ‘supposed’ or ‘alleged’, to attest that to the verified truth of what is said.<sup>151</sup> For example:

(20) Gettier cases are *true* counter-examples/*really* are counter-examples to the theory that knowledge is justified true belief.

(21) The Liar paradox is a *true/real* paradox, whereas the barber paradox isn’t.

‘Real’ and ‘really’ can also be used in a way that seems to be entirely dependent on the interests and purposes of speakers. For example, although ‘book’ has two dictionary meanings according to which a comic book is *literally* a book, a speaker can also say:

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<sup>150</sup> Austin (ibid). Frege makes this point about ‘true’. See Frege (1914).

<sup>151</sup> My impression is that people in the south of England now use “physically” in both of these ways, as well as for emphasis.

(22) A comic book isn't a *real* book, and in doing-so signal that the dictionary meaning of 'book' is not being adhered to, and that 'book' is instead being used in a way determined by the speaker's interests. So, having finished *The Adventures of Tin Tin*, I can insist that I have, *literally*, finished a book, even after being pressed to say otherwise, while conceding that I haven't finished a *real* book in the sense my interlocutor is interested in. Likewise, one might concede that although Cambridge MA, is literally speaking a city, isn't a *real* city. In this way the use of 'real' contributes to a context, perhaps by establishing a register, in which speakers can interpret and evaluate what they hear as interest-relative.

To take stock, hedging terms allow us to defer to the primary meanings of expressions of the sort that get entered in the dictionary, to meanings established by experts, and to each other's interests and purposes. Further, which of these things one should defer to is a matter to be determined by the speaker in the context of utterance. If all this is correct, then I think the general moral is that the semantics of expressions should not have to account for all of the interest-relative uses of expressions.<sup>152</sup> For we have seen that expressions like 'book', 'city' and 'diamond' whose meanings are, intuitively, *not* interest-relative, can nevertheless be used in a special way in accordance with our interests, instead of as expressing their contextually invariant dictionary or technical meanings. This is because one of the things that can be achieved with hedging locutions is the creation of conversational contexts —perhaps even registers— in which the dictates of expression semantics are temporarily ignored in accordance with our interests.

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<sup>152</sup> See Salmon (2004) for a different route to this conclusion.

I will now apply this moral to the semantics of attributions of knowledge-wh. For reasons of space, I leave the topic of how attributions of knowing-who interact with hedging terms (as well as the topic of how *de re* attributions do so) for further work.

### 8. Interest-relativity and hedging

First I want to discuss a complication that is introduced by the fact that the degree of exactness that we mean by ‘exactly’ varies with the interests and purposes we have when inquiring about a length. Perhaps then we can bite the bullet and say that while Ralph knows how long S is, he does not know *exactly* how long S is, with the result that no one knows *exactly* how long anything is. To make this strange result more palatable, one might claim that quantities are independently existing abstract entities, and furthermore, so is the continuum of real numbers. So, the response continues, it is perhaps not surprising that quantities cannot be exactly correlated with the real continuum, and that consequently we do not apprehend quantities exactly by measuring them. Then, according to this response:

(23) *Technically*, no one knows *exactly* how long anything is.

Unfortunately, this response does not solve the paradox. The reason is that we still speak of the NFL knowing exactly how long a given football field is. This is because the meaning of ‘exactly’ is interest-relative, and relative to the NFL’s interests and purposes, knowing the length of a given football field to the nearest inch is more than sufficient for knowing exactly how long the field is. Further, knowing this requires Ralph, who sets up the first measurement system, to know exactly how long the standard object is, to a degree of exactness suitable for setting up a system, although not to the rather technical degree of exactness described above. But this is just to say that Ralph must know how long the standard object is.

With that said, consider (5) again, and compare it with (24) and (25):



(5) Ralph knows how long S is.

(24) Ralph doesn't *really* know how long S is.

(25) *Technically speaking*, Ralph doesn't know how long S is.

The first and most obvious point is that 'really' in (24) is not used to correct a toy duck case, since nobody will correct (5) with (24), or say 'Of course we never *really* knew how long anything is after all.' Rather, according to my theory of hedging, 'really' in (24) is contributing to a context in which speakers can interpret and evaluate (24) in a special way that is more restrictive than what is required by literal expression meaning, and that is determined by our interests, or by a technical standard as in (25). On this basis, the advocate of interest-relativity can claim that in a context in which the interests are philosophical, or we are speaking technically-philosophically, and the standards for attributing knowledge-wh have been correspondingly restricted, the result is that (24) and (25) are true. Furthermore, in order to solve the paradox, the advocate of interest-relativity can argue that in this same context (5) remains true. This is because its univocal literal expression-meaning and truth conditions remain the same, and because all they require is what Salmon claims they require: that one produce the name of the object's length, while understanding its meaning. Recall that in the case of reference fixing under discussion, Salmon says this requirement is satisfied by Ralph (a') knowing his own intention in introducing the standard name 'meter' for the length of S, while (b') looking at the stick, which gives him the requisite *a posteriori* understanding of the name.

Analogously, it could be argued that we are correctly described as knowing what number  $\pi$  is, in virtue of knowing that  $\pi$  is the ratio of the circumference of a circle to its diameter, despite the fact that we are also correctly described as not *really* or *technically* knowing what number  $\pi$  is.

But now consider the following. There is a debate as to whether anyone can know how long S is. Smith says that no one knows how long S is. And then Jones responds with (5). In that context, (5) can be heard as false. The reason is that the preceding discourse has restricted the standards in just the way that ‘really’ and ‘technically’ do. In which case, it seems, my proposal does not solve the paradox after all.

This objection assumes that the literal truth-conditional contribution of ‘knows how long’ is supplanted by the technical standard for applying that notion. But this assumption is questionable. Firstly, because this does not appear to be a case like ‘mammal’, ‘diamond’ or ‘cardinal number’, in which that the standards for applying those terms literally are the same as the standards for doing so technically.<sup>153</sup> Secondly, because in my view the literal expression meaning of certain non-indexical, non-demonstrative expressions remains stable across contexts, even after we create special contexts in which we can choose to ignore their literal meaning in favor of something more technical. To see this, consider that it must be possible to assert the Moorean proposition expressed by ‘I know that I have hands’, sincerely and literally, during a philosophy seminar in which the standards for knowing p have been restricted; further, for this to be possible requires that ‘knows’ expresses something univocal, that is stable across contexts, including those in which ‘knows’ can be used to convey a more restrictive standard. Since hard cases make a bad law, we should also consider the case of ‘flat’. In this case too, it must be possible for me to assert, sincerely and literally, that the screen of my iMac is flat, even though I am aware that a strict standard has been introduced in a special conversational context via the use of ‘technically’. Again, this requires that ‘flat’ expresses something univocal, a notion of flatness that is stable across contexts. The same is true in the case of (5). That is, it is possible to make a

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<sup>153</sup> See chapter 3 section 2 for a discussion of ‘cardinal number’.

sincere and literal assertion of the proposition expressed by (5), even after a special context has been created in which we are debating whether anyone can know exactly how long S is. For this reason, I find it somewhat plausible that (5) can remain true in a conversational context in which (24) and (25) are true. In any case, this is the best I can do in terms of a contextualist solution to the paradox.<sup>154</sup>

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<sup>154</sup> As this chapter was nearing completion, Gary Ostertag directed me to some forthcoming work by Peter Ludlow, which bears *some* similarity to the view in the text. I have not had time or space to give Ludlow's view the treatment it deserves, but look forward to engaging with it in future work.

## Chapter 6: A Proposal

### 1. Introduction

In this chapter I begin by discussing the topic of count nouns, and attempting to solve a puzzle about them due to Nathan Salmon. This leads me to the proposal about numbers I that favor, which is that numbers are properties of sets. Next I show how my proposal can be spelled out as a proper system of definitions, using the logical background of the simple theory of types (STT). I also show the extent to which the proposal can avoid the problems that were shown to plague Frege's analysis in chapter 2. Finally, I discuss whether the axioms of STT are general primitive truths.

### 2. Count and mass nouns

There is a grammatical distinction between count and mass nouns (e.g. 'dog' vs. 'sand'), and between count and mass occurrences of nouns like 'tomato'. Beginning with count nouns, these sometimes occur with the plural suffix (as in 'dogs') and always occur within the scope of another expression ('Many dogs...', 'John's dogs...'). On the other hand, mass nouns never occur with the plural suffix, and sometimes occur without another expression having scope over them ('Water is wet'). As for nouns like 'tomato' that can have count or mass occurrences, when these occur with the plural suffix or within the scope of quantifiers such as 'many', 'few' or '27', they have a count-occurrence. When they occur in the singular without a determiner or within the scope of quantifiers like 'much', 'little', and 'half a pound of', they have a mass-occurrence.

It will be recalled that in Frege's view arithmetic shares some of the generality of logic, because numbers are applicable to sortal-kind-concepts and almost anything can be bought under a suitable sortal-kind-concept (see chapter 2 section 2). As for what makes such concepts

suitable, it will also be recalled that in addition to the requirement that they be precise rather than vague, Frege adds that:

Only a concept which isolates what falls under it in a definite manner, and which does not permit any arbitrary division of it into parts, can be a unit relative to finite Number (1884: §54).

Here we have a Fregean semantic distinction corresponding to the grammatical one between mass and count nouns. Putting all this together, the concept  $F$  expressed by a count noun must permit of division into  $F$ 's, but permit no arbitrary division of individual  $F$ 's into further  $F$ 's. For example, the concept *dog* expressed by the noun 'dog' does not permit division of the members of its extension into more dogs. Moreover, while 'sandwich' does permit division of the members of its extension into more sandwiches, not everything that results from dividing a sandwich is itself a sandwich, and so this concept permits no arbitrary division. In contrast, 'sand' does permit arbitrary division into more sand, assuming that everything that results from dividing sand is also sand.

This last assumption is questionable, since some things that result from dividing a quantity of sand are not more sand; for example, at the point just before the molecular level is reached, what will result from division are molecules, not more sand. Moreover, a similar moral applies to 'furniture', since a part of a chair that is obtained by dividing furniture need not be furniture. Thus the problem is that there are mass nouns as well as count nouns that do not permit arbitrary division, and so this is not a sufficient condition for being a count noun, given my assumption about what Frege means by 'arbitrary'.

Katherine Koslicki proposes the following solution. For a division to be arbitrary does not require that *everything* that results from dividing the extension of  $F$  is an  $F$ , as I assumed

above. Rather, for a division to be arbitrary requires that the extension of  $F$  can be divided “in a myriad of unprincipled ways” (1997: 420), where the extension of  $F$  can be divided in a myriad on unprincipled ways

just in case *many* (though perhaps not all) proper parts of what falls under [ $F$ ] themselves fall under the concept *and* we can pick these many proper parts *randomly* without any particular care (ibid: 421).

Koslicki’s proposal is that the concepts expressed by count nouns permit no arbitrary division in this sense. This proposal gets the right results in easy cases like ‘sand’ and ‘dog’, and even in some hard cases; for example, sandwiches permit no arbitrary division by this criterion. But it gets the wrong result for ‘furniture,’ since furniture also permits no arbitrary division by this criterion, and yet ‘furniture’ is not a ‘count noun.’ Further, this analysis of ‘arbitrary division’ does not seem to capture Frege’s intent. What on earth is the notion of picking (or choosing) parts at random doing in a semantic analysis of count nouns, especially if the analysis proposed is in the service of logicism? (Compare the discussion of Linnebo’s analysis of the notion of an arbitrary subcollection, in chapter 3, section 7.)

### 3. Salmon’s puzzle about count nouns

Frege’s analysis of count nouns raises an interesting puzzle, due to Salmon, concerning the following scenario: I place three oranges on the table, before cutting off and eating one half of an orange. Exactly how many oranges are on the table? We can all calculate the obvious answer:

There are exactly  $2\frac{1}{2}$  oranges on the table.

However, there is a compelling argument that the obvious answer is incorrect. Either the half of an orange on the table is an orange on the table, or it is not. On the one hand, if it is an orange on

the table, then there are 3 oranges on the table. And on the other hand, if it is not an orange on the table, which it is not according to Frege's doctrine that count nouns permit no arbitrary division, then there are 2 oranges on the table. Either way, the obvious answer is incorrect. Further, if we modify the example slightly, so that I cut off and eat  $\frac{3}{4}$  of an orange, then, since one quarter of an orange is not an orange (again, by Frege's doctrine), there are exactly 2 oranges on the table. So, instead of giving the obvious answer we must give the less obvious answer:

There are exactly 2 oranges on the table.

The problem is that our intuitions (or at least Salmon's and mine) balk at the less obvious answer. Rather, it is the obvious answer that seems correct.

The less obvious answer is required by the logicist doctrine that numbers apply to extensions, classes or sets, together with Frege's doctrine that the members of extensions of count nouns permit no arbitrary division. To see this, consider that according to later Frege's analysis, the number 2 is the extension containing all and only those extensions with 2 members; further, the members of extensions do not permit of division into fractions, because they are the extensions of count nouns, which admit no arbitrary division. Thus there is no extension containing all and only two-and-a-half-membered extensions, and so, if we are to follow Frege, we have to say that there are not  $2\frac{1}{2}$  nor 3 but exactly 2 oranges on the table.

One possible answer Salmon considers is the conjunctive answer, which is as follows:

There are exactly 2 oranges on the table, and there is exactly 1 orange-half on the table.

However, as Salmon points out, according to the conjunctive answer fractions are partly non-mathematical, since while the numerator of a fraction is a numerical quantifier ('1 orange-half'), the denominator is a non-mathematical operator on count nouns ('1 orange-half'). But it is

implausible that fractions are partly non-mathematical; rather, ‘ $\frac{1}{2}$ ’ goes with ‘2’ not with ‘orange’. Another problem is that for the conjunctive answer to be true, both of its conjuncts must be true. So the conjunctive answer entails the less obvious answer that there are 2 oranges on the table, and is thus incompatible with the obvious answer that there are  $2\frac{1}{2}$  oranges on the table.

The solution that Salmon favors is to abandon the logicist doctrine that numbers apply to extensions, classes or sets, and adopt a proposal that justifies giving the obvious answer to the question of exactly how many oranges on the table. According to this proposal, numerals are non-extensional numerical quantifiers. They are quantifiers because the basic form of numerical statements like ‘there are 2 oranges on the table’ is assumed to be:

There are  $n$  things  $x$  such that  $Fx$ .

Further, they are non-extensional because they say

something quantitative not about the class of oranges on the table, nor anything similar (like the characteristic function of that class), but about...well,... *the oranges on the table*—the property, if you will, of being such an orange, or better, the *plurality* (group, collective), i.e. the oranges themselves (1997: 237).

Here the idea is that

some properties are exemplified or possessed by individuals taken collectively, in concert, rather than taken individually, and rather than by the corresponding class (ibid).

An example of such a property is the property of hoisting the groom into the air during the hora, which is usually possessed by individuals taken collectively rather than taken individually, and rather than by the corresponding class or set. Returning to non-extensional numerical quantifiers,



what they designate are properties of pluralities relative to a sortal-kind, where a sortal-kind is not the extension of a count noun but what is semantically expressed by it, and where the former semantic value does not determine the latter. In particular, in Salmon's scenario the half of an orange is *not* one of the things that are in the extension of 'orange on the table', although it *is* among the plurality of things that are collectively of the sortal-kind *orange on the table*. '2½' designates the numerical property that this plurality has relative to this sortal-kind. This is why the proposal justifies giving the obvious answer to the question of exactly how many oranges are on the table: there are indeed 2½ things on the table that are collectively of the kind *orange*, even though there are only 2 things on the table that are members of the extension of 'orange'.

It will be recalled that a serious objection to the logicist definition of number, is that it has the consequence that different entities are identical with numbers in different possible worlds (see chapter 2, section 7). However, since, in Salmon's view, numerals are non-extensional operators, his view promises to solve this problem. For according to the proposal that Salmon favors, numbers are numerical properties. Assuming that these properties, like all properties, exist in all possible worlds, then there can be no problem about identifying a given number with different entities in different possible worlds. Of course, one could try to solve this problem while adhering to the logicist definition more closely than Salmon does, by claiming that numbers are numerical properties of sets. I will return to this view in the following sections.

More recently, Salmon has floated the idea that numerals might plausibly be regarded as non-extensional operators, rather than as non-extensional quantifiers. In his view, they can plausibly be regarded as operators, because the basic form of numerical statements like 'there are 2 oranges on the table' should be glossed as '2 oranges are on the table', and is arguably that of a determiner phrase, where the basic form is:

*n k's are* \_\_\_\_\_

Further, they are non-extensional because a given numeral ‘*n*’ designates the function that assigns to any sortal-kind *k* the (characteristic function of the) class of all and only those pluralities that include exactly *n k's*.<sup>155</sup> As before, *k* is not the extension of a count noun but the sortal-kind semantically expressed by it, where the former semantic value does not determine the latter. As a result, in Salmon’s scenario, this function assigns to the sortal-kind *orange on the table* the class of all and only those pluralities that include the 2 whole oranges and the half of an orange on the table.

As Salmon himself notes, his proposals have an odd consequence. If, in Salmon’s scenario, I place another half of an orange on the table, then, since  $2\frac{1}{2} + \frac{1}{2} = 3$ , by parity with the reasoning in support of the obvious answer given above, there are now exactly 3 oranges on the table. Or suppose that there are two halves of an orange on the table. Then, since  $\frac{1}{2} + \frac{1}{2} = 1$ , there is exactly 1 orange on the table, and so an orange on the table. Salmon claims that his intuitions balk at this consequence much less strongly than they do at the less obvious answer in the original scenario, but admits that a solution which blocks this consequence, while respecting the obvious answer above, “is obviously preferable” (ibid: 240). Personally, my intuitions balk strongly at this consequence. So, while I don’t think that this is a conclusive objection to either of Salmon’s proposals, I do think it worthwhile to investigate whether Salmon’s puzzle has another solution. This will be taken up in the next section.

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<sup>155</sup> Salmon communicated this to me in personal correspondence. This view, taken together with the Frege-Church theory of sense and designatum, would have it that a numeral also semantically expresses the corresponding concept of the designated function.

#### 4. A contextualist response to Salmon's puzzle

Another possible solution to the puzzle, that Salmon rejects, is to claim that the question 'Exactly how many oranges are on the table?' is concerned not with how many but how much. I will now make the case for such a solution.

I have already noted that nouns like 'orange' and 'tomato' can have count and mass occurrences. A further point is that occurrences which by their grammatical status are count occurrences demanding a count reading —because they occur with the plural suffix and within the scope of numerals— can nevertheless require a measure reading. To see this, contrast (1) with (2) and (3):

(1) The 2 tomatoes/oranges on the table cost a dollar each.

(2) \*The 2 tomatoes/oranges in the sauce cost a dollar each.

(3) \*The 2½ tomatoes/oranges in the sauce cost a dollar each.

Since there are not 2 tomatoes in the sauce, we need the measure reading instead, according to which there are not 2 tomatoes in the sauce, but

[2 tomato's worth] of [tomato] in the sauce.

Further, since it is hard to believe that count occurrences of 'tomato' are ambiguous, what seems plausible is that '2 tomatoes' semantically expresses its count meaning, but can be used to convey the measure reading in certain conversational contexts.

Following the theory presented in chapter 5, my next claim is that in the scenario in which I place three oranges on the table, before cutting off and eating one half of an orange, the question 'Exactly how many oranges are on the table?' contributes to a conversational context in which we up the ante to a more precise but non-literal standard for knowing how many, according to which there are 2½ oranges on table. However, it remains the case that *literally* or

*strictly speaking* the obvious answer is false and the less obvious answer is true: there are 2 oranges on the table. Thus one can try to save the logicist doctrine that numbers apply to extensions, classes or sets from Salmon's objection. But what does the invoked more-precise-but-non-literal standard for knowing how many require? On one view, it requires a measure of quantity, where this measure is given not in terms of a unit from some measurement system like the metric system, but in terms of a unit that is introduced in reference to an instance of a sortalkind, the quantity of which is being measured. On this view, 'Exactly how many oranges are on the table?' is *somewhat* like 'Exactly how many blocks is the distance to Central Park?', or 'Exactly how many weeks long is June?'. I say "somewhat" because instead of asking for a measure of distance in New York City blocks, or a measure of time in weeks, the first question asks for a measure of quantity in terms of the unit *an orange's worth of orange*. This proposal can explain why, in Salmon's scenario, if I place another half of an orange on the table, then, since  $2\frac{1}{2} + \frac{1}{2} = 3$ , there must be exactly 3 of something —albeit not oranges— on the table. It is because the equation applies to measurements of quantity, and on a measure reading there are now *exactly 3 oranges worth of orange on the table*.

However, there is a disanalogy between the case of oranges and that of New York City blocks. For while there is plausibly a convention that a block is a somewhat variable unit of measurement for the dimension of distance, no such thing holds of 'orange' or 'tomato'. Rather, in my view, what a measure reading of a count noun gives us is a somewhat variable dimensionless unit that is introduced in context, in accordance with the following rule:

Let any count noun 'F' designate an F's worth of F, where the context determines how much an F's worth is.

The remaining problem, which Salmon takes to be the main objection to the sort of view that I am pushing, is that my proposal will not work for all count nouns. This is because things like pumpkins and cakes can vary a great deal in size. So, in a context in which there many pumpkins in the yard of various sizes, as well as one half of a pumpkin, a speaker will have no idea how much pumpkin is a pumpkin's worth. In which case, answering 'How many pumpkins are in the yard' with for example '27½' is not answering with a measure of quantity.

The case of 'cake' is easier to handle, because 'cake' has a broad literal meaning, which can be temporarily ignored in favor of something narrower, in accordance with the speaker's interests. (In this respect it is like the cases of 'book' and 'city' discussed in section 5). Thus in a scenario in which I have eaten a petite fours and a chocolate sponge cake, both of which are, strictly speaking, cakes, we have:

(4) *Strictly speaking*, I ate two cakes, I *really* only had one and a bit.

In this example, the interests in the context determine what counts as a cake, and so can determine how much cake is a cake's worth. However, this does not take care of the case of pumpkins, since there is no wiggle room in the literal meaning of 'pumpkin.' Rather, the scenario in which there many pumpkins in the yard of various sizes, as well as one half of a pumpkin, seems to be a degenerate case in which there is nothing stable to determine how much a pumpkins worth is. So the only way to size up the pumpkins in the yard accurately is by counting. In this case 'pumpkin' cannot get a measure reading, and so must get a count reading. In answer to the question 'How many pumpkins are in the yard?', '27½' is strictly speaking false, but is heard as true because in most other cases there is a measure reading to make it true.

This concludes my defense of the logicist doctrine that numbers apply to entities such as extensions, classes or sets against Salmon's objection. I now return to the question of exactly what numbers are.

### 5. Equinumerosity properties

In chapter 2 we saw that Frege defines ' $\#x: Fx$ ' so that it designates the extension of the second-level concept containing exactly those first-level concepts that are equinumerous with  $F$ . However, we also saw that this proposal is undermined by the fact that counting concepts and collecting them into extensions forces one to treat concepts as objects, and so to count and collect the corresponding extensions. Further, we saw that later Frege's proposal, that ' $\#x: Fx$ ' refers to the extension of the first-level concept containing exactly those extensions that are equinumerous with  $F$ , is undermined by the fact that such extensions are non-well founded. Furthermore, we saw that Frege's view has the consequence that different entities are identical with numbers in different possible worlds.

This brings me to another proposal about numbers, that Kripke mentions in the Whitehead Lectures but does not explore in any detail:

I think there's some case for a more intensional view than Frege and Russell took, in any case, of what [a cardinal] number is. It might be better: a *property* of being a set that has elements  $x_1 \dots x_n$  that are distinct and so on, rather than the set.

This view, which is also endorsed by Giaquinto (see chapter 1, sections 7-9), appears to have been the view of Georg Cantor, who proposed that the number of a set is not itself a set of equinumerous sets (or a class of equinumerous classes), but the property of being equinumerous

with that set.<sup>156</sup> This view is expressed in his review of Frege's *Grundlagen*, where Cantor writes:

I call 'cardinality of a collection [*Inbegriff*] or of a set [*Menge*] of elements' (where the latter can be homogenous or heterogeneous, simple or composite) that general concept under which fall all and only those sets that are equivalent to the given set. Here, two sets are to be called 'equivalent', if they can be correlated one-to-one with each other, element for element (1885).

I will add the qualification that the number of a set is the property of sets *of a given kind*, of being equinumerous with that set, and will call such properties "equinumerosity properties of sets." In the next two sections I will suggest one way in which this proposal can be spelled out into a proper system of definitions that can solve the Caesar problem, with the *caveat* that this way of spelling it out does not accord with Kripke's intent. After that I will continue to argue for the proposal, by showing the extent to which it can solve the other problems that plague Frege's theory.

## 6. Church's type-theory

It will be recalled from chapter 3 that second-order logic is an augmentation of first-order logic that quantifies over properties and relations as well as objects. In order to spell out the proposal mentioned in the previous section, I will now proceed beyond second-order logic, into higher-order logic. This is done by quantifying over not only properties, relations and functions, but also over higher-level entities (that take properties, relations and functions as their arguments), higher-higher-level entities (that take higher-level entities as their arguments), and so on. I will work within a version of higher-order logic known as 'the simple theory of types'

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<sup>156</sup> See also Weber (1906).

(or ‘STT’), originally due to Russell but subsequently formulated by Alonzo Church and his students Leon Henkin and Peter Andrews.<sup>157</sup> For the most part, my presentation follows that of Andrews.

There are two basic insights behind STT, one of which accords very well with common sense, and the other ordinary mathematical practice. The first is that numbers, sets of numbers, sets of sets of numbers, functions, sets of functions, etc., are entities of different types, where types themselves are reflected in the language. This insight accurately reflects the practice of mathematicians, who already know the types of the entities that they are thinking about, when working in one or another particular subject area, and who will draw type distinctions when working in a more general framework like set theory, by using different kinds of symbols to designate numbers (e.g. ‘1’, ‘2’, ‘3’..., ), sets of numbers (e.g. ‘ $\mathbf{N}$ ’ or ‘ $\mathbf{\bar{N}}$ ’), and sets of sets of numbers (e.g. ‘ $\mathcal{P}(\mathbf{N})$ ’). The second insight is that entities of a given type apply to entities of the highest type below their type, and so do not apply to entities of the same type. That this is common sense can be seen from the fact that while ‘Someone is over 5 feet tall’ is intelligible, ‘Everyone Kripke’ and ‘Everyone someone’ are nonsense. Likewise, while ‘Kripke is over 5 feet tall’ is intelligible, ‘is over 5 feet tall is a commonplace quality to have’ is nonsense. Once these insights are appreciated, STT can be seen to accord well with our intuitive judgments, with the result that it is *as least as* justified by the method of reflective equilibrium as the iterative conception of set (see chapter 4, section 2).

In accordance with the aforementioned insights, all expressions of STT are assigned types. ‘ $e$ ’ designates the type of individuals, ‘ $v$ ’ the type of truth-values, and if  $\alpha$  and  $\beta$  are type-symbols, then  $\lceil(\alpha \rightarrow \beta)\rceil$  is the type-symbol designating the type of functions from entities of

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<sup>157</sup> Russell (1903), Church (1940), Henkin (1963), Andrews (1986).



type  $\alpha$  to those of type  $\beta$ . (Church writes this  $\lceil(\beta\alpha)\rceil$ , but I find the arrow notation considerably more visually revelatory; see chapter 4, section 13.) For example,  $\langle e \rightarrow v \rangle$  designates the type of functions from individuals to truth-values, and  $\langle v \rightarrow v \rangle$  designates the type of functions from truth-values to truth-values.

Expressions are subscripted with type-symbols that indicate their type, which is to say the type of the entities that they designate. For example, the usual symbols for negation and disjunction are subscripted to indicate, respectively, the type of functions from truth-values to truth-values, and functions from pairs of truth-values to truth-values:

$$\sim_{\langle v \rightarrow v \rangle}, \vee_{\langle (v, v) \rightarrow v \rangle}$$

According to STT, statements such as ‘P’ and ‘Q’ designate truth-values, so ‘ $\sim P$ ’ and ‘ $P \vee Q$ ’ are written, respectively as:

$$\sim_{\langle v \rightarrow v \rangle} P_v$$

$$P_v \vee_{\langle (v, v) \rightarrow v \rangle} Q_v$$

In what follows I will omit the types of connectives.

Before I give the criteria for being a well-formed expression of STT, another crucial ingredient must be introduced. This is predicate abstraction, which allows us to build complex predicates and functors from formulae and open sentences. I will walk through how this is done slowly, first explaining things rather concretely before doing so with greater generality.

It will be helpful to recall from chapter 3 that the expressive power of second-order logic is obtained by laying down comprehension axioms, which are axioms stating that a formula  $\Phi$  defines a second-order entity in the domain of the higher-order variables, such as a property or class. It will also help to recall that these axioms have the following form:

$$\exists P \forall x_1 \dots x_n [P x_1 \dots x_n \leftrightarrow \Phi(x_1 \dots x_n)]$$

Now suppose that we want to define *complex* properties and functions of individuals (so we are staying at the second-order). To do this we introduce the variable binding  $\lambda$ -abstraction operator. Then, by the above axiom, in the case in which  $\Phi$  is the formula ‘ $x$  is a Polish diplomat and  $x$  is a great pianist’, we can abstract the corresponding complex predicate, as follows:

$$\lambda x[x \text{ is a Polish diplomat and } x \text{ is a great pianist}] \leftrightarrow x \text{ is a Polish diplomat and } x \text{ is a great pianist}$$

The open sentence on the right hand side expresses a conjunctive proposition (with respect to an assignment). In contrast, the  $\lambda$ -abstract on the left hand side expresses the complex monadic property of being a thing that is both a Polish diplomat and a great pianist. As a result, these expressions, although logically equivalent, are arguably not synonymous, since someone such as Kripke’s Peter, who is confused about the identity of Paderewski, could believe that Padereski is a Polish diplomat and Padereski is a great pianist, without believing that Padereski is a thing that is both a Polish diplomat and a great pianist.<sup>158</sup> In addition to expressing this complex monadic property, the  $\lambda$ -abstract designates the characteristic function that maps any potential value of  $x$  to truth iff it is a Polish diplomat and a great pianist. Since this  $\lambda$ -abstract is a complex predicate of individuals, in STT it is written

$$\lambda x_e[x_e \text{ is a Polish diplomat}_{(e \rightarrow v)} \text{ and } x_e \text{ is a great pianist}_{(e \rightarrow v)}]$$

In the context of STT we need to proceed beyond second-order logic, so we need a more general comprehension axiom scheme stating that an expression  $\Psi_\beta$  of any type  $\beta$  defines a complex function  $u$  of type  $(\alpha \rightarrow \beta)$ , which does not occur free in  $\Psi_\beta$ :

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<sup>158</sup> See Kripke (1979).

$$\exists u_{(\alpha \rightarrow \beta)} \forall y_{\alpha_1} \dots y_{\alpha_n} [u_{(\alpha \rightarrow \beta)} y_{\alpha_1} \dots y_{\alpha_n} = \Psi_{\beta}]^{159}$$

We designate the function whose existence is asserted directly above as

$$\lambda x_{\alpha_1} \dots x_{\alpha_n} [\Psi_{\beta} x_{\alpha_1} \dots x_{\alpha_n}]$$

So we have the comprehension scheme

$$\forall y_{\alpha_1} \dots y_{\alpha_n} [\lambda x_{\alpha_1} \dots x_{\alpha_n} [\Psi_{\beta} x_{\alpha_1} \dots x_{\alpha_n}] y_{\alpha_1} \dots y_{\alpha_n} = \Psi_{\beta}]$$

More perspicuously, we can designate the aforementioned function

$$\lambda x_{\alpha} [\Psi_{\beta}]$$

So the comprehension scheme becomes

$$\forall y_{\alpha} [\lambda x_{\alpha} [\Psi_{\beta}] = \Psi_{\beta}]$$

Corresponding to this axiom scheme, the  $\lambda$ -conversion rule of  $\lambda$ -expansion licenses the replacement, within a formula, of any occurrence of  $\Psi_{\beta}$  by the  $\lambda$ -abstract  $\lceil \lambda x_{\alpha} [\Psi_{\beta}] y_{\alpha} \rceil$ . The rule of  $\lambda$ -contraction licenses the reverse replacement. So, for example, from ‘ $x$  is a Polish diplomat and  $x$  is a great pianist’ we can infer ‘ $\lambda x[x$  is a Polish diplomat and  $x$  is a great pianist] $y$ ’, and vice versa.

To give a more pertinent example, suppose that we allow  $\lambda$ -conversion in an un-typed language. Then we can write a formula that purports to designate the Russellian function that does not apply to itself: ‘ $\lambda F[\sim F(F)]$ ’, which we shall call ‘ $R$ ’. Now consider the formula ‘ $RR$ ’, which expresses that  $R$  applies to itself. By the definition of ‘ $R$ ’ and  $\lambda$ -conversion:

$$RR \leftrightarrow \lambda F[\sim F(F)]R$$

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<sup>159</sup> Since  $\Psi_{\beta}$  is of any type  $\beta$ , we use boldface ‘=’, which reduces to ‘ $\leftrightarrow((\forall v) \rightarrow v)$ ’ in case  $\Psi$  is a formula, and to identity, ‘ $=((\alpha\alpha) \rightarrow v)$ ’, in case  $\Psi$  is a singular term.

But then by an application of  $\lambda$ -contraction to the right hand side,  $RR \leftrightarrow \sim RR$ , which is a contradiction. However, if the language is typed, then this problem cannot arise. For according to STT, well-formed expressions are limited to:

- (i) Variables and constants of type  $\alpha$ , for example ' $x_e$ ', ' $\sim_{(v \rightarrow v)}$ ' and ' $P_v$ '
- (ii) Complex expressions of type  $\beta$  formed from expressions of type  $\alpha$  and of type  $(\alpha \rightarrow \beta)$ , for example ' $\sim_{(v \rightarrow v)} P_v$ '
- (iii)  $\lambda$ -abstracts of type  $(\alpha \rightarrow \beta)$  formed from variables of type  $\alpha$  and expressions of type  $\beta$

By (i) – (iii), ' $F(F)$ ' is not well-formed, since there is no type of functors that take themselves as arguments. For there is no type of expressions  $\delta$  formed only from expressions of type  $\delta$ . As a result, ' $\sim F(F)$ ' is not well-formed, and neither is  $R$ . So the contradiction cannot arise.

Next I will show how the proposal that numbers are properties of sets can be developed into a proper system of definitions. I begin by explaining the basic idea behind my definitions, before stating the definitions properly within STT.

### 7. Equinumerosity properties again

The basic idea in what follows is to amend Frege's definitions, so as to reflect the proposal that numbers are equinumerosity properties of sets of entities of a certain kind (see section 5). This can be done by defining the relation of equinumerosity between such sets in terms of a one-to-one function, and then defining the notion of the cardinal number of a set in terms of the property of being equinumerous with a given set  $Z$ :<sup>160</sup>

The number of  $Z =_{\text{df}} \lambda X [\text{Equinumerous}(X, Z)]$

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<sup>160</sup> Subsequently I discovered that Carnap defines 'the number of  $Z$ ' as I do in the text. See Carnap (1947: 116).

If ‘ $\lambda$ ’ is the standard function abstraction operator, then the  $\lambda$ -abstract in the definiens designates the characteristic function of the set of sets that are equinumerous with  $Z$ , a function that maps  $X$  to truth if  $X$  is equinumerous with  $Z$ , and maps  $X$  to falsity otherwise. This  $\lambda$ -abstract also expresses the property of being equinumerous with  $Z$ . Armed with the above definition of ‘the number of  $Z$ ’, one can then continue to amend Frege’s definitions, first by defining the empty set:

the empty set =<sub>df</sub> the set of all  $x$ :  $x \neq x$

Then by defining ‘0’ as:

0 =<sub>df</sub> the number of the empty set  
 =<sub>df</sub>  $\lambda X[\text{Equinumerous}(X, W)]$ , where  $W$  = the empty set

This abstract expresses the property of being equinumerous with the empty set, and designates the characteristic function of the set of sets that are equinumerous with the empty set.

Although I will continue to develop my proposal in terms of characteristic functions, I want to emphasize that these entities are not the only way of implementing the basic idea that numbers are equinumerosity properties. For example, one could also adopt an apparatus in which the number of  $Z$  is the propositional function that assigns to any class  $X$  the singular proposition that  $X$  is equinumerous with  $Z$ . However, I also want to emphasize that there is good reason to work with functions of one kind or another, rather than with higher-order logic under its classical extensional interpretation, for reasons I will give in the last few paragraphs of this chapter.

Within the framework of STT, properties or kinds, and the sets corresponding to those kinds, can be represented as characteristic functions, which characterize sets by mapping entities to truth iff they are elements of the set in question (entities of the kind in question), and to falsity otherwise. For this reason, while one can talk about the set of elements of type  $\alpha$ , one might just

as well talk about the characteristic function of type  $(\alpha \rightarrow \nu)$ . In particular, while one can talk about a set of individuals of type  $e$ , one might just as well talk about the characteristic function of type  $(e \rightarrow \nu)$ . I will now drop the arrow notation for functions, writing  $\ulcorner(\alpha\beta)\urcorner$  instead of  $\ulcorner(\alpha \rightarrow \beta)\urcorner$ , and so in the case of sets ‘ $(e\nu)$ ’ instead of ‘ $(e \rightarrow \nu)$ ’. I will also drop the outer parentheses of type-symbols, for example writing ‘ $(e\nu)\nu$ ’ instead of ‘ $((e \rightarrow \nu) \rightarrow \nu)$ ’.

The first order of business is to define the relation of equinumerosity between any two sets  $Z$  and  $Y$ . Since sets are functions of type  $(\alpha\nu)$ , ‘Equinumerous( $X, Z$ )’ becomes

$$\text{Equinumerous}_{((\alpha\nu)(\beta\nu))\nu}(X_{(\alpha\nu)}, Z_{(\beta\nu)})$$

This designates a function  $s$  from pairs of functions to truth-values, that correlates one-to-one the arguments of  $X$  with the arguments of  $Z$ . That is to say: (1)  $s$  is one-to-one on  $X$ : it maps every argument of  $X$  to an argument of  $Z$ . (2)  $s$  maps  $X$  onto  $Z$ : it maps all of the arguments of  $X$  to all of those of  $Z$  (rather than embedding the arguments of  $X$  in those of  $Z$ ). So we have:

$$\text{Equinumerous}_{((\alpha\nu)(\beta\nu))\nu}(X_{(\alpha\nu)}, Z_{(\beta\nu)}) \equiv_{\text{df}}$$

$$\lambda X_{(\alpha\nu)} \lambda Z_{(\beta\nu)} \exists s_{(\alpha\beta)} [\forall x_{\alpha} (X_{(\alpha\nu)}x_{\alpha} \rightarrow Z_{(\beta\nu)}(s_{(\alpha\beta)}x_{\alpha})) \wedge \forall y_{\beta} [Z_{(\beta\nu)}y_{\beta} \rightarrow \exists x_{\alpha} (X_{(\alpha\nu)}x_{\alpha} \wedge (y_{\beta} = s_{(\alpha\beta)}x_{\alpha}))]]$$

Now the above definition of ‘the number of  $Z$ ’ can be modified accordingly:

$$\text{The number of } Z_{(\beta\nu)} =_{\text{df}} \lambda X_{(\alpha\nu)} [\text{Equinumerous}_{((\alpha\nu)(\beta\nu))\nu}(X_{(\alpha\nu)}, Z_{(\beta\nu)})]$$

This  $\lambda$ -abstract expresses the property of sets of the same type as  $Z_{(\beta\nu)}$ , of being equinumerous with  $Z_{(\beta\nu)}$ , and designates the corresponding characteristic function of the set of sets that are equinumerous with  $Z_{(\beta\nu)}$ .

The natural numbers 0, 1, 2 etc. are properties of sets of type  $(ev)$ , and thus of type  $(ev)v$ .

One can amend Frege's definitions accordingly, first by defining the empty set:

the empty set $_{(ev)}$  = $_{df}$   $\lambda x_e: x_e \neq x_e$

Then by defining '0' as:

$0_{((ev)v)}$  = $_{df}$  the number of the empty set

= $_{df}$   $\lambda X_{(ev)}$ [Equinumerous $_{((ev)(ev)v)}$ ( $X_{(ev)}$ ,  $W_{(ev)}$ )], where  $W_{(ev)}$  = the empty set

Since '0' is of type  $(ev)v$ , and the successor function maps each number to its successor, it will be a function of type  $((ev)v)((ev)v)$  from functions to functions. The following definition ensures that the successor of  $m$  is the number of a set  $X$  iff  $X$  contains exactly *one more individual* than the set of which  $m$  is the number:<sup>161</sup>

$S_{((ev)v)((ev)v)}m_{(ev)v}$  = $_{df}$   $\lambda m_{(ev)v} \lambda X_{(ev)} \exists x_e [X_{(ev)}x_e \wedge m_{(ev)v}(\lambda a_e [X_{(ev)}a_e \wedge a_e \neq x_e])]$

Glossing the above definitions, I now have:

0 = the number of the empty set,

S0 = the number of a set that contains exactly one more individual than the empty set,

SS0 = the number of a set that contains exactly one more individual than a set that contains exactly one more individual than the empty set....

The semantic values respectively expressed and designated by the above  $\lambda$ -abstracts form a progression satisfying the axioms of arithmetic, so long as it is assumed that there exist infinitely many individuals. Of course one must say exactly how these individuals are ordered. So, following Andrews, I will accept the following axiom of infinity:

Infinity $_{order}$ : There is a strict partial ordering  $r$  of the individuals with respect to which there is no maximal element.

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<sup>161</sup> See Andrews (ibid: 203).

In the context of STT, an ordering is regarded as a function  $r$  from pairs of individuals to truth-values, and is thus of type  $(ee)v$ . A strict partial ordering is irreflexive ( $\sim rxx$ ), transitive ( $(rxy \wedge ryz) \rightarrow rxz$ ) and so asymmetric ( $\sim (rxy \wedge ryx)$ ). So we have:

$$\text{Infinity}_{\text{order}}: \exists r_{(ee)v} \forall x_e y_e z_e \exists w_e [r_{(ee)v} x_e w_e \wedge \sim r_{(ee)v} x_e x_e \wedge \\ ((r_{(ee)v} x_e y_e \wedge r_{(ee)v} y_e z_e) \rightarrow r_{(ee)v} x_e z_e)]$$

There is no requirement of trichotomy: *for all individuals  $x$  and  $y$  either  $rx y$ , or  $x = y$ , or  $ry x$ .*

This is to say that there may be individuals for which none of the requirements of trichotomy hold, just as there may be humans who are not related by ancestry. Call such individuals

“incomparable,” and call individuals  $u$  and  $v$  “comparable” iff  $ruv$ , or  $rvu$ . In the light of this,

$\text{Infinity}_{\text{order}}$  can be rephrased slightly more intuitively, as follows: there is a strict partial ordering of the individuals, with respect to which there is no individual that comes after everything with which it is comparable, where there is no assumption that every individual is comparable. (That there is no such assumption is important, so I will return to it in section 9.)

The axiom of infinity is needed in the context of STT to prove that numbers with the same successors are the same:<sup>162</sup>

$$\forall m_{(ev)v} n_{(ev)v} [S_{((ev)v)(ev)v} m_{(ev)v} = S_{((ev)v)(ev)v} n_{(ev)v} \rightarrow m_{(ev)v} = n_{(ev)v}]$$

Proving this axiom requires proving that for every number, if a set  $X$  has that number, then there is an individual not in  $X$ :

$$\forall m_{(ev)v} [m_{(ev)v} X_{(ev)} \rightarrow (\exists w_e \sim X_{(ev)} w_e)]$$

This in turn is proven from the claim that every number has the following property: if  $X$  is a set of that number, then there is an individual that does not come before anything in  $X$ :

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<sup>162</sup>Andrews (ibid: 207-9).



$$\lambda m_{(ev)v} \forall X_{(ev)} [m_{(ev)v} X_{(ev)} \rightarrow \exists z_e \forall w_e (X_{(ev)} w_e \rightarrow \sim r_{(ee)v} z_e w_e)]$$

The axiom of infinity ensures that there is always such an individual, since it ensures that there is no individual that comes after everything with which it is comparable.

Recalling the discussion of Hilbert's hotel from chapter 3, another candidate to be the axiom of infinity is:

Infinity<sub>set</sub>: There is a set of individuals that is equinumerous with a proper subset of itself.

$$\exists X_{(ev)} Y_{(ev)} [\forall x_e [Y_{(ev)} x_e \rightarrow X_{(ev)} x_e] \wedge \exists y_e [X_{(ev)} y_e \wedge \sim Y_{(ev)} y_e] \wedge$$

$$\text{Equinumerous}_{((ev)(ev)v)}(X_{(ev)}, Y_{(ev)})]$$

I will discuss the philosophical ramifications of accepting the axiom of infinity in section 9.

It remains to imitate Frege's strategy of defining 'natural number', without using 'reached from 0 by finitely many iterations of  $S$ ', in such a way that a version of mathematical induction is true of the natural numbers. One option is to begin with properties of type  $((ev)v)v$  of entities of type  $(ev)v$ . Then I can define the property of properties of type  $((ev)v)v$  of *being closed under  $S$* , where for a property of type  $((ev)v)v$  to be closed under  $S$  is for it to apply to the successor of any entity it applies to:

$$\text{Cl}_{((ev)v)v} X_{((ev)v)v} =_{\text{df}} \lambda X_{((ev)v)v} \forall m_{(ev)v} [X_{((ev)v)v} m_{(ev)v} \rightarrow X_{((ev)v)v} S_{((ev)v)((ev)v)} m_{(ev)v}]$$

This maps  $X_{((ev)v)v}$  to truth if  $X_{((ev)v)v}$  is closed under  $S_{((ev)v)((ev)v)}$ , and to falsity otherwise. Then I can define 'natural number' as expressing the property of having every property of type  $((ev)v)v$  that 0 has and that is closed under  $S$ :

$$\text{Natural Number}_{((ev)v)v} m_{(ev)v} =_{\text{df}} \lambda m_{(ev)v} \forall X_{((ev)v)v} [(X_{((ev)v)v} 0_{(ev)v}) \wedge$$

$$\text{Cl}_{((ev)v)v} X_{((ev)v)v}] \rightarrow X_{((ev)v)v} m_{(ev)v}]$$

These last two definitions can be more perspicuously stated by eliding the type ‘ $(ev)v$ ’ of ‘ $m$ ’ and using ‘ $\Psi$ ’ for properties of type  $((ev)v)v$  of entities of type  $(ev)v$ :

$$Cl(\Psi) =_{df} \lambda \Psi \forall m [\Psi(m) \rightarrow \Psi(Sm)]$$

Then ‘natural number’ can be said to express the property of having every third-level property  $\Psi$  that 0 has and that is closed under  $S$ :

$$\text{Natural Number}(m) =_{df} \lambda m \forall \Psi [\Psi(0) \wedge Cl(\Psi) \rightarrow \Psi(m)]$$

Another option is to define a function that maps pairs of entities of type  $(ev)v$  to truth just in case one can be reached from the other by finitely many iterations, by introducing ‘ $\leq_{((ev)v)((ev)v)v}$ ’ and defining it as follows:

$$\begin{aligned} & \leq_{((ev)v)((ev)v)v} l_{(ev)v} m_{(ev)v} \\ & \equiv_{df} \lambda l_{(ev)v} \lambda m_{(ev)v} \forall X_{((ev)v)v} [X_{((ev)v)v} l_{(ev)v} \wedge \forall k_{(ev)v} [X_{((ev)v)v} k_{(ev)v} \rightarrow \\ & \quad X_{((ev)v)v} S_{((ev)v)((ev)v)v} k_{(ev)v}] \rightarrow X_{((ev)v)v} m_{(ev)v}] \\ & \equiv_{df} \lambda l \lambda m \forall \Psi [\Psi(l) \wedge \forall k [\Psi(k) \rightarrow \Psi(Sk)] \rightarrow \Psi(m)] \end{aligned}$$

This reflects Frege’s idea that  $m$  can be reached from  $l$  by finitely many iterations iff  $m$  has every property that  $l$  has and that all of  $l$ ’s successors have. Then ‘natural number’ can be defined as follows:

$$\text{Natural Number}_{((ev)v)v} m_{(ev)v} \equiv_{df} \leq_{((ev)v)((ev)v)v} 0_{(ev)v} m_{(ev)v}$$

Using these definitions, Andrews shows that the Dedekind-Peano axioms can be derived from the axioms of STT and the axiom of infinity.

Next I will argue for the philosophical significance of this derivation, by rehearsing the points in favor of the above definitions, and in favor of the basic proposal which they implement:

that numbers are properties of sets of entities of a certain kind. Then I will return to the axioms of STT and the axiom of infinity.

### 8. Assessing the above definitions

The first point in favor of the above system of definitions is that it arguably solves the Julius Caesar problem. To see this, recall from chapter 2 that in Salmon's view, the Caesar problem is not to provide a criterion of identity and individuation for numbers sufficient to distinguish a given number from Julius Caesar; rather, it is a problem about improper definitions. If Salmon is correct about this, then the problem is now solved, since the above is a proper system of definitions which, together with the facts about Caesar, determine that 'Natural Number(Caesar)' is false.<sup>163</sup> Moreover, it is also worth noting that the present proposal provides me with a sort of insurance policy. For even if the Caesar problem *is* to provide a criterion of identity and individuation for numbers sufficient to distinguish a given number from Julius Caesar, the basic idea behind the definitions can still help. This is because the basic idea is that numbers are properties, which are necessary existents, whereas Julius Caesar is not a necessary existent.<sup>164</sup> However, since I have invoked characteristic functions to turn this basic idea into a proper system of definitions, it is obviously desirable that these functions also be necessary existents. I will say more about this in a moment. (I would add that Salmon could also take out my insurance policy (although I suspect that he feels safe without it), since numbers are also properties on the view he favors (see section 8).)

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<sup>163</sup> Salmon himself proposes a system of definitions on behalf of Frege that solve the Caesar problem so understood. See Salmon (forthcoming). I have appropriated his strategy for developing such a system of definitions, although not the definitions themselves.

<sup>164</sup> This happy consequence of identifying numbers with necessary existents is noted by Parsons (1965).

The next point in favor of the present proposal is that since it is a modification of Frege's, it enjoys many of the features that make his proposal so faithful to ordinary usage (see chapter 2). In particular, it can explain why arithmetic shares some of the generality of logic. It is because numbers are properties of sets, which on the present proposal are functions, and almost anything can be collected into a set or taken as the argument of a function (modulo the considerations about count nouns discussed in the sections 2 - 4). The present proposal also preserves the thought that the successor of  $m$  is the number of a set containing *exactly one more thing* than the set of which  $m$  is the number. Further, it also respects a version of the Hume-Cantor principle (see chapter 2, section 6): the property of being equinumerous with  $X$  is the property of being equinumerous with  $Z$  iff  $X$  and  $Z$  can be put in one-to-one correspondence. Furthermore, according to the above definition, 0 does not have "second-rate" status as a pseudo-number, but is the property of being equinumerous with the empty set. There is of course the worry that, like Frege's proposal, the present one focuses exclusively on the cardinal aspect of numbers while neglecting their ordering. However, this is mitigated by the fact that the semantic values of the above  $\lambda$ -abstracts form a numerical progression.

I now turn to what I consider to be another very attractive feature that the present proposal inherits from Frege's. The proposal provides an account of how we can, in principle, deduce concepts of cardinal numbers, which can then be used in counting, from an understanding of count nouns and of one-to-one correspondence. Further, as we will see, this account dovetails nicely with an attractive view of how we grasp numbers intuitively in practice. As a result of all this, the proposal provides the requisite account of how it is possible to grasp numbers prior to using them in counting, which I argue is missing from Burge's story (see chapter 1, section 13). The proposal also provides an answer to the question of what it is that infants don't understand,

when they count transitively with numerals without understanding the cardinal significance of what they have done (again see chapter 1, section 13, and most recently chapter 4, section 11). I will now elaborate on these claims.

As regards how we can grasp cardinal numbers prior to counting, the claim is that because we can understand count nouns ('*F*', '*G*' etc.), understand what it is to one-to-one correspond the members of their corresponding sets (*X*, *Z* etc.), and can reason with higher-order logic, we understand of sentences of the form 'Equinumerous(*X*, *Z*)'. Further, using the resources of higher-order logic, in particular the aforementioned comprehension scheme

$$\forall y_{\alpha}[\lambda x_{\alpha}[\Psi_{\beta}] = \Psi_{\beta}]$$

we can, in principle, deduce the content of 'the number of *Z*' from the content of sentences of the form 'Equinumerous(*X*, *Z*)', by abstracting the complex property expressed by ' $\lambda X[\text{Equinumerous}(X, Z)]$ ' from the sentence 'Equinumerous(*X*, *Z*)'. Furthermore, we can abstract particular cardinal numbers in a similar fashion, because we can abstract the complex properties 0, *S*0 etc., by the method indicated in the above system of definitions. Moreover, even *without* assuming an axiom of infinity (more of which in the next section), these numbers form an initial part of a progression that can be used in counting. In my view, while it is through counting that we are first *taught* about cardinal numbers, some of them are already in principle accessible to us, by the deductive route just described, without counting via some finite intuitive process that terminates, and without an axiom of infinity. Less prosaically, they are already accessible to us through reflection.

However, recalling the distinction between the contexts of discovery and justification (see chapter 2, section 2), I am not claiming that the folk actually come to grasp numbers through this sort of reflection. Rather, I conjecture that numbers are first grasped intuitively, by

visualizing the accumulation of discrete units in a direction (see chapter 1, end of section 14), which is a structurally revelatory representation of the progression described in the previous section (see also Kripke's remarks quoted in chapter 4 section 8):

$0$  = the number of the empty set,

$S0$  = the number of a set that contains exactly one more individual than the empty set,

$SS0$  = the number of a set that contains exactly one more individual than a set that contains exactly one more individual than the empty set....

In sum, according to the present view, numbers are accessible to us in principle by reflection, and in practice by visual intuition, even before we are taught to recite decimal numerals in order and use them in counting.

Now I turn to the question of what it is that infants don't understand, when they count transitively with numerals without understanding the cardinal significance of what they have done. In my view, while these infants can establish a one-to-one correspondence between numerals and objects, their competence understanding does not extend to understanding the conceptual connection between counting, equinumerosity and cardinal numbers, and in particular does not extend to understanding that the last numeral of the transitive count —with which they answer “how many”— expresses an equinumerosity property of sets. This is why, when instructed, after counting, to ‘Give me  $m$  F's’ —where  $m$  is the last numeral used in the transitive count— they give the experimenter a random number of F's. Were they to understand that ‘ $m$ ’ expresses the relevant equinumerosity property, and that the set they have counted thereby has that property, then they would be able to give the experimenter the requisite number of F's. Here I should note that this solution only depends on ‘ $m$ ’ expressing the relevant equinumerosity property, and does not depend on the property being a certain characteristic function.

In any case, I take the availability of this solution to be a significant advantage that proposals in the Frege-Russell tradition enjoy over the set-theoretic proposals due to Zermelo and von Neumann that were discussed in the previous chapter. To take the case of Zermelo, suppose that the child knows both that ‘ $m$ ’ designates the set of all iterations of the unit-set operation she performs, and that the set of F’s is one-to-one correlated with the set of all these iterations. This does not suffice for the child to be able to give the experimenter the requisite number of F’s. What she also needs to know is that ‘ $m$ ’ expresses (or designates) the property of being equinumerous with the set of all iterations of the unit-set operation she performs, and that the set of F’s has this property. But this is to make the Zermelodic proposal a special case of Frege-Russell. (The point can also be stated in terms of von Neumann’s proposal.)

The present proposal also provides a response to Frege’s arguments that numbers are objects rather than properties. These arguments, it will be recalled, are that numbers are designated with definite descriptions like ‘the number 1’, as well as in arithmetical statements like ‘2 is prime’ and ‘ $1 + 1 = 2$ ’ (1884: §57). Further, Frege argues, numbers are objects because they can be counted; for if one can count numbers, then one can also designate them with complete expressions; but in that case one treats numbers as objects, since by Frege’s lights anything that one can designate with a complete expression is an object.

The response is that on the present approach there is a clear account of how properties can have both an attributive role and a role as referent or designatum. To give this account we have to recognize a corresponding grammatical distinction, between unsaturated functional expressions and saturated  $\lambda$ -abstracts. For while unsaturated expressions such as ‘ $x$  is a horse’ and ‘ $\lambda x[x \text{ is a horse}]y$ ’ apply *to* their arguments and express properties that are true of individuals, saturated  $\lambda$ -abstracts such as ‘ $\lambda x[x \text{ is a horse}]$ ’ thereby *designate* functions, without

missing their target by designating the corresponding objects instead.<sup>165</sup>  $\lambda$ -abstraction can thus explain how numbers are designated in arithmetical statements like ‘2 is prime’ and with definite descriptions like ‘the number 1’. For the numerals ‘1’, ‘2’ etc. are synonymous with ‘ $S0$ ’ ‘ $SS0$ ’ etc., and according to my system of definitions these in turn are synonymous with saturated  $\lambda$ -abstracts designating functions. Furthermore, since there is no need to say that these expressions designate objects, there is no need to introduce extensions or courses of values, governed by the inconsistent Basic Law V (see chapter 2, section 8). Rather, one can work within STT with functions that need not be regarded as extensions or sets.

The proposal can also help with one of the most serious objections leveled against Frege: that numbers as Frege defines them are non-well founded (see chapter 2, section 7). Adjusted slightly to fit the present context, the first objection runs as follows. Supposing that 3 is a root of an equation  $E$ , then 3 is a member of the set of roots of  $E$ . Further, since numbers can be counted, we may suppose that the number of roots of equation  $E$  is 3. By this supposition and by definition, the set of roots of  $E$  is a member of 3, since 3 is by definition the characteristic function of the set of sets that are equinumerous with a class that contains exactly 3 things. But then the class corresponding to this characteristic function is non-well-founded: one can have two classes  $x$  and  $y$  such that  $x$  is included in  $y$  and  $y$  is included in  $x$ . Happily however, this objection does not apply in the context of STT, since 3 is a property of sets of type  $(\epsilon v)v$ , whereas *being a root of  $E$*  is a property of properties of sets of type  $((\epsilon v)v)v$ . Clearly then, 3 cannot take the set of roots of  $E$  as argument, since it is of a lower type. Of course the point can also be put set-theoretically, if we choose to speak that way, by saying that the set of roots of  $E$

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<sup>165</sup> See Church (ibid), which receives endorsement in work by his students: see Kaplan (2005), Burge (2005: 21).



cannot be included in 3. But in STT we get this result while sticking more closely to Frege's analysis.

Answering the modal objection is harder. This is because while my basic proposal is that numerals are properties that are invariant from world to world, I have also claimed that numerals designate characteristic functions, in order to turn my basic proposal into a proper system of definitions. Further, while these functions need not be regarded as sets, they still appear to be subject to the modal objection. To see why, it will be helpful to introduce the notion of the graph of a function  $f$ , which Frege calls a "course of values" or "value-range" (1893: vii). To identify a function with its graph is the usual way of identifying a function in mathematics, extensionally speaking. The graph of  $f$  is the set of all and only those ordered pairs whose first members are arguments from  $f$ 's domain, and whose second members are the corresponding values from  $f$ 's range. In the case of the characteristic function that, on the present proposal, is the number 1, its graph will be the set of ordered pairs whose first member is a set and whose second member is one of the two truth-values. This will lead us into modal trouble, because graphs contain sets, which may in turn contain contingently existing objects. With this in mind, consider the following adjusted version of the modal objection:

- (1) Sets are individuated by their members.
- (2) Sets contain the same members in every possible world in which they exist. (By 1.)
- (3) Sets do not contain non-existent objects.
- (4) Actual sets only exist in other possible worlds in which all of their actual members also exist (By, 2, 3.)
- (5) There is a possible world  $w$  in which Richard Carpenter does not exist.
- (6) The actual set of surviving members of the Carpenters does not exist in  $w$ . (By 4, 5.)

- (7) The set (which I will refer to as ‘ $Y$ ’) that actually contains all and only those sets that are equinumerous with a set containing exactly one member, contains the actual set of surviving members of the Carpenters.
- (8) The actual graph of the characteristic function of  $Y$  does not exist in  $w$ . (By 4, 6, 7, identity conditions for graphs.)
- (9) The number 1 exists in  $w$ . (Assumption.)
- (10) In  $w$ , the number 1 is not identical with the actual graph of the characteristic function of  $Y$  (By 8, 9.)

In order to avoid the excessively implausible conclusion that different entities are identical with 1 in different possible worlds, one might be tempted to insist that functions are intensional entities —such as instruction or rules— that exist apart from their graphs. For then one could accept (10) while also saying that in  $w$ , the number 1 is identical with the characteristic function of  $Y$ . However, there is good reason to think that identifying functions with intensional entities would lead to paradox.<sup>166</sup> As things stand then, my system of definitions is consistent in part because it is extensional. So it still remains to develop my proposal into a system of definitions in a way that answers the modal objection.

### 9. Are the axioms of STT primitive truths?

If arithmetic were analytic (by Frege’s lights), then its axioms would be derivable from the laws of logic together with what is expressed by the definitions of the arithmetical primitives, without appeal to intuition or any other non-logical source of knowledge. But the axiom of infinity is not a logical truth. This raises the question of what non-logical source of knowledge

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<sup>166</sup> That is, it appears to lead to a version of the intensional paradox described in Appendix B of Russell (1903). This can be avoided using the apparatus of Russell (1910), but this leads to other serious problems.

justifies the assumption that there exist infinitely many individuals. After all, the assumption is not obviously self-evident. Why then should we accept it as a primitive truth? One could of course argue for the axiom based on its fruitfulness i.e. from the theorems that follow from it. But why is this not just wishful thinking?

The first reason is that the axiom is justified by the method of reflective equilibrium. To see this we should recall Frege's observation that any set can be numbered, so long as its members can be put into one-to-one correspondence with a segment of the numbers. By accepting an axiom of infinity we extend Frege's observation, since by Cantor's denumerability results this observation applies not only to discrete collections of objects of a given kind, but also to subsets of real numbers. (See the discussion of HP in chapter 3, section 2.) Crucially, this theory can be bought into accord with our intuitive judgments somewhat, since it is an extension of Frege's intuitively plausible observation, which already shows that in practice we are counting relatively small sets with only an initial segment of a much larger progression of numbers.

Here I have to mention a suggestive but to my mind inconclusive argument due to Thomas Nagel, who offers considerations related to those just given in the above paragraph, as part of an argument that we must somehow rationally grasp the axiom of infinity in order to *explain how we discover and make sense of* the evident fact that every number has a successor:

To get that idea [that every number has a successor], we need to be operating with the concept of numbers as the sizes of sets which can have anything whatever as their elements. What we understand then, is that the numbers we use to count things in everyday life are merely the first part of a series that never ends (1997: 71).

I take Nagel's idea to be that "anything whatever" encompasses infinitely many objects of any kind, for this would ensure that the numbers we use in everyday life are the first part of an

unending series. However, Nagel does not appeal the method of reflective equilibrium to justify the axiom of infinity. Rather, he simply claims that the axiom's truth must be assumed if we are to even *make sense of* the practice of counting:

When we think about the finite activity of counting, we come to realize that it can only be understood as part of something infinite. The idea of reducing the apparently infinite to the finite is therefore ruled out: Instead the apparently finite must be explained in terms of the infinite (ibid).

Perhaps then it is Nagel's view that the axiom of infinity is self-evident to a sufficiently reflective person, albeit not obviously so. In any case, I suspect that I am not alone in finding Nagel's claim that we must realize that the axiom of infinity is true to even make sense of our practice of counting rather dogmatic. So I feel obliged to augment this account with a justification for the axiom by reflective equilibrium.

Another argument for the axiom of infinity is the argument from fruitfulness: that one should accept as primitive axioms that are not self-evident, if doing so allows one to discover the correct analysis of arithmetic (what we would call "the right modeling"). That is, one should accept axioms if doing so allows us, for the first time, to derive correspondents of the axioms of arithmetic *which preserve the thoughts expressed by the latter axioms*, in virtue of fully analyzing terms like 'number' and 'predecessor'. (See chapter 2 section 4, and chapter 3 section 7.) Of course this argument is hostage to the accuracy of the proposed definitions. As such, it draws support from the considerations offered in section 8 of this chapter. In this respect, the proposal can be contrasted favorably with Heck's proposal to derive the axioms of arithmetic from those of Frege Arithmetic (including HP) using predicative comprehension, which, it will be recalled, requires taking 'the number of F's' and ' $P(m, n)$ ' as primitive.

The proposal can also be contrasted favorably with Giaquinto's proposal about our intuitive grasp of the number structure. His proposal is that we grasp the numbers as discrete points on an endless line, taken in their left-to-right order of precedence, and thus grasp a strict linear ordering with no greatest element, which is also a well-ordering. (See chapter 1, section 14). The reason that the contrast is favorable is that although Giaquinto and I both assume an axiom of infinity, he does so in the service of what I argue is an uninformative theory, while I do so in the service of an informative analysis. Further, I only assume that there is a strict partial ordering with no maximal element, and so assume something with significantly less structure than the numbers.

Next I turn to the comprehension axioms of STT (introduced in section 6 of this chapter). Here it will be helpful to recap a little bit. When discussing Frege's Theorem (in chapter 3, section 6) I was prompted to ask after the possible values of the second-order variables, in the impredicative comprehension axioms for second-order logic, which are instances of:

$$\exists P \forall x_1 \dots x_n [Px_1 \dots x_n \leftrightarrow \Phi(x_1 \dots x_n)]$$

I argued that if the second-order variables are thought of under a classical extensional interpretation, as ranging over all subsets of the domain, then one should use plural logic to understand what their values are. I was then lead to ask after the values of the plural variables in axioms of plural comprehension:

$$\exists xx \forall u_1 \dots u_n (u_1 \dots u_n < xx \leftrightarrow \Phi u_1 \dots u_n)$$

My answer was that plural variables range over *all pluralities*, where a plurality is many things, not one collection. This raised the question of what is required to understand the concept of all pluralities, to which I answered: an understanding of the concept of *combinations of individuals*. Further, I claimed, this concept is not logical but *combinatorial*, in the sense of

concerning unordered arrangements of objects. Thus I argued that plural logic, and second-order logic under its classical extensional interpretation, both assume non-logical content in their comprehension axioms.

This argument does not apply to the comprehension axioms for  $\lambda$ -abstraction, in the context of STT. Recall that these are instances of a scheme stating that an expression  $\Psi_\beta$  of any type  $\beta$  defines a complex function of type  $(\alpha\beta)$ :

$$\exists u_{(\alpha\beta)} \forall y_{\alpha 1} \dots y_{\alpha n} [u_{(\alpha\beta)} y_{\alpha 1} \dots y_{\alpha n} = \Psi_\beta]$$

The reason that the above argument does not apply is that in STT one does not think of higher order variables under their classical extensional interpretation, as ranging over all subsets of a domain, but as ranging over functions of a given type. Thus second and higher-order logic have a greater claim to logicity when they are thought of in this way, as opposed to in terms of their classical extensional interpretation.

**Table 1:**

Decimal	Base-26	Conversion
0	z	
1	a	
2	b	
3	c	
4	d	
5	e	
6	f	
7	g	
8	h	
9	i	
10	j	
11	k	
.....		
25	y	
26	az	$1 * (26 \text{ to the 1st power}) + 0 = 26$
27	aa	$1 * (26 \text{ to the 1st power}) + 1 = 27$
28	ab	$1 * (26 \text{ to the 1st power}) + 2 = 28$
29	ac	$1 * (26 \text{ to the 1st power}) + 3 = 29$
30	ad	$1 * (26 \text{ to the 1st power}) + 4 = 30$
.....		
51	ay	$1 * (26 \text{ to the 1st power}) + 25 = 51$
52	bz	$2 * (26 \text{ to the 1st power}) + 0 = 52$
53	ba	$2 * (26 \text{ to the 1st power}) + 1 = 53$
54	bb	$2 * (26 \text{ to the 1st power}) + 2 = 54$
....		
77	by	$2 * (26 \text{ to the 1st power}) + 25 = 77$
78	cz	$3 * (26 \text{ to the 1st power}) + 0 = 78$
79	ca	$3 * (26 \text{ to the 1st power}) + 1 = 79$
....		
103	cy	$3 * (26 \text{ to the 1st power}) + 25 = 103$
104	dz	$4 * (26 \text{ to the 1st power}) + 0 = 104$
105	da	$4 * (26 \text{ to the 1st power}) + 1 = 105$
....		
649	xy	$24 * (26 \text{ to the 1st power}) + 25 = 649$
650	yz	$25 * (26 \text{ to the 1st power}) + 0 = 650$
651	ya	$25 * (26 \text{ to the 1st power}) + 1 = 651$
....		
675	yy	$25 * (26 \text{ to the 1st power}) + 25 = 675$
676	azz	$1 * (26 \text{ to the 2nd power}) + 0 + 0 = 676$
677	aza	$1 * (26 \text{ to the 2nd power}) + 0 + 1 = 677$
....		
701	azy	$1 * (26 \text{ to the 2nd power}) + 0 + 25 = 701$
702	aaz	$1 * (26 \text{ to the 2nd power}) + 1 * (26 \text{ to the 1st power}) + 0 = 702$
703	aaa	$1 * (26 \text{ to the 2nd power}) + 1 * (26 \text{ to the 1st power}) + 1 = 703$
....		
308,915,776	azzzzzz	$1 * (26 \text{ to the 6th power}) = 308915776$

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