Quasi-Scientific Methods of Justification in Set Theory

Wesley Wrigley*

Draft of May 2020

Introduction

Gödel's approach to the justification of the axioms of set theory was two-pronged. On the one hand, he thought that the standard axioms of second-order set theory, as well as extensions thereof by certain reflection principles (which assert that the hierarchy of sets cannot be correctly and uniquely characterized by formulae of specified syntactic kinds), could be justified by intuitive reflection on the content of the concept *set* (1964, p.268 and p.260, fn.18 and fn.20). However, there are axiom candidates asserting the existence of extremely large cardinal numbers, which seemingly cannot be justified this way. Indeed, all known consistent reflection principles of the kind considered by Gödel have at most the consistency strength of an axiom asserting the existence of the partition cardinal (a.k.a. 'Erdős cardinal') $\kappa(\omega)$ (Koellner 2009).[1] I'd like to avoid getting bogged down in technical details, but suffice it to say that, by today's standards, $\kappa(\omega)$ is not a particularly large cardinal. According to Gödel, these stronger principles can instead be justified by *extrinsic*, or quasi-scientific means. The justification of set theoretic axioms by such methods will be the concern of this paper. I'll refer to such axioms which cannot, on the Gödelian view, be justified by appeals to intuition as *large large cardinal axioms.*[2]

*Wadham College and Faculty of Philosophy, University of Oxford

[1] Let $[\alpha]^{<\omega}$ be the union, for all n, of the n-element subsets of α. For any limit ordinal β, the Erdős cardinal $\kappa(\beta)$ is the least cardinal λ with the following property: for every $f : [\lambda]^{<\omega} \to \{0, 1\}$, f is constant on $[H]^{<\omega}$ for some β-sized subset H of λ. See (Jech 2003, pp.109 and 302) for full details.

[2] It is important to bear in mind that such talk is loose, in an important sense. Large cardinal axioms *appear* to be linearly ordered by consistency strength, but there is no

According to Gödel, platonism about set theory is the primary foundation for the use of quasi-scientific methods in justifying large large cardinal axioms. In the case of the natural sciences, the real existence of the objects concerned justifies the use of 'probabilistic' or inductive methods to reach decisions about the nature of those objects, which would not make sense if they were regarded as useful fictions or mental constructions. In Gödel's view, the same realistic attitude to the objects of set theory establishes an analogy between mathematics and natural science which is sufficient to employ analogous methods in establishing an over-all picture of the hierarchy.

In §1, I'll introduce a strong analogy that Gödel draws between sets and material bodies, namely that the former are required to make sense of our mathematical experience in the same way that the later are required to make sense of our empirical experience. I'll argue that no such analogy can be used to justify a belief in large large cardinals.

In §2, I'll introduce Russell's regressive method, which accords well with part of Gödel's thinking on the justification of axioms in set theory, whereby axioms are verified by permitting the deduction of elementary mathematical 'data', just as laws of nature in the sciences are justified by facilitating the prediction of data drawn from sense experience.

In §3, I'll examine the various options for what might constitute the mathematical data for the purposes of Gödel's analogy. These include the deliverances of so-called mathematical perception, the theorems of ordinary mathematics, and Π^0_1 arithmetical consequences. I'll argue that of these candidates, some selection of Π^0_1 sentences offers the only plausible option. Not all sentences of this form can act as data, but a reasonable delineation of some privileged such sentences can be isolated (though this delineation is perhaps not sharp).

In §4, I'll then argue that, on this construal of the data, no large large cardinal axiom gains any *strictly regressive support* by accounting for the

theorem to this effect (and there is no agreed definition of 'large cardinal property' which would be required for the formulation of such a theorem). Secondly, although I'll speak of large large cardinals and large large cardinal axioms more-or-less interchangeably, it is important to remember that the ordering of such axioms by consistency strength is not identical to the ordering of the cardinals concerned by size. A cardinal κ is *huge* iff it is the critical point of a non-trivial elementary embedding j from V into a transitive inner model M containing all sequences of length $j(\kappa)$ whose elements are in M (Kanamori 2009, p.331). κ is supercompact iff for all $\lambda \geq \kappa$, there is some elementary embedding j from V to a transitive inner model M with critical point κ containing all sequences of length λ whose elements are in M and $j(\kappa) > \lambda$ (Kanamori 2009, p.298). If **ZFC** plus an axiom for the existence of a huge cardinal is consistent, then so is **ZFC** plus an axiom for the existence of a supercompact cardinal. However, if cardinals of each kind exist, then the least supercompact cardinal is far larger than the least huge cardinal (Jech 2003, p.381).

data. By that, I mean that no large large cardinal axiom is such that it permits the deduction of a datum that cannot be deduced with help only from weaker assumptions. Hence we can't regard such posits as analogous to laws of nature with strictly regressive support.

So the only plausible respect in which large large cardinal axioms could possibly be justified by quasi-scientific means is by being regarded as principles which seek to maximise the theoretical virtues of set theories to which they might be added. Though I don't have a means of weighing and evaluating the contribution of various such virtues, I'll make the case in §5 that under no scheme for evaluating theoretical virtues should we expect large large cardinal axioms to perform well, if the virtues in question are broadly scientific as Gödel suggests. Indeed, the closer the analogy between mathematics and science, the less well-supported by the analogy are large cardinal axioms, and hence the prospects for justification of these principles by analogical reasoning are bleak.

1 The Material Bodies Analogy

The use of quasi-scientific methods for justifying axioms of set theory is now commonplace in the philosophy of mathematics. Most famously, the indispensability arguments of Quine (implicit in his (1951a)) and Putnam (explicit in his (1975)) justify the truth of set-theoretic axioms by examining the role they play in formulating adequate theories in natural science. Maddy's later work (e.g. (1997)) seeks to legitimise large large cardinal axioms via the empirical study of the behaviour of actual set theorists. In contrast with the Quine–Putnam approach, which attempts to found mathematical platonism on an empirical basis, Gödel's use of quasi-scientific methods is largely internal to mathematics. The role of large cardinals is examined in terms of their contribution to a wider *mathematical* theory; little more than lip service is paid by Gödel to the applications of such theories in the sciences. And in contrast to Maddy's approach, which sees the methods of set theorists as essentially autonomous, Gödel attempts to justify set-theoretic modes of theory choice by showing them to be analogous to sound methods found in the natural sciences. So Gödel's approach to the problem has not survived the decades in its original form, despite the fact that this aspect of his philosophical thought has undoubtedly had the greatest impact on later analytic philosophy, far eclipsing the reception of his anti-mechanism, rationalistic optimism, and conceptual platonism.

Gödel's quasi-scientific approach does not annex mathematics to the sciences, and nor does it insulate the former from the latter. Rather, it *imports* some elements of scientific methodology into mathematics, by finding a structural analogy between the two. There is not, in Gödel's remarks, a unique analogy to this effect, so first I'd like to disambiguate two distinct analogical arguments presented by Gödel.

The analogical argument that is the primary concern of this chapter takes it that large cardinal *axioms* play the role in a mathematical theory that laws of nature play in a scientific theory. It is common enough to conceive of natural-scientific propositions as being divided into two broad kinds (whether or not we take those kinds to be disjoint or sharply delimited): the data and the laws. On this standard conception, the data are empirical propositions that we take to be the facts, and the laws are those propositions which are formulated in order to predict the facts. Indeed, the prediction of the data is the primary means of verifying these laws; whether or not they are intrinsically plausible, we take them to be true if they predict all the data and don't predict anything false. By analogy, certain large cardinal axioms are supposed to be verified by 'predicting' (which is to say, deductively implying) mathematical propositions of some privileged kind identified as the data. This is broadly the Russellian view of the matter, and we shall return to it in due course.

Distinctly, Gödel sometimes speaks as if sets themselves, that is, the particular objects asserted to exist by the axioms, play the role in our understanding of a mathematical theory that physical objects play in understanding our phenomenal experience (1944, p.128). It may seem that these views are not substantially different; perhaps it matters little to natural science, for instance, whether we posit the existence of physical bodies or assent to the truth of sentences asserting them to exist. I'll argue that there is, however, a substantial difference between the two cases when we consider large large cardinals, and that an analogy between mathematical and physical objects cannot justify any large large cardinal axioms.

The analogy between sets and material bodies first appears when Gödel discusses his platonism about sets (though not large large cardinals in particular) and properties of sets. He writes:

> It seems to me that the assumption of such objects is quite as legitimate as the assumption of physical bodies, and there is quite as much reason to believe in their existence. They are in the same sense necessary to a satisfactory system of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions (1944, p.128).

Although Gödel does not elaborate here on the sense in which he thinks the assumption of physical bodies is necessary for a satisfactory theory of perception, I think it is safe to assume that it is something along the lines of the following now-standard explanation from Russell:

> [A]lthough this is not logically impossible [that there are no physical bodies], there is no reason whatsoever to suppose that it is true; and it is, in fact, a less simple hypothesis, viewed as a means of accounting for the facts of our own life, than the common-sense hypothesis that there really are objects independent of us, whose action on us causes our sensations (1912, p.10).

Russell describes the 'simplicity' as stemming from the fact that it would be a 'miracle' (1912, p.9) if objects came and went from existence as we started and finished perceiving them. Obviating the need to believe in this miracle by positing physical bodies is described as a 'natural' theoretical move, rather than an account of how we acquired our belief that there are physical objects (1912, p.11).

Can a similar account be given of the posit that there are sets in general, and large large cardinals in particular? In an early presentation of the regressive method (which will shortly be examined in more detail), Russell (1907, p.573), takes it that 'accounting for' or 'predicting' the relevant data amounts, in the case of mathematics, to proving some given privileged propositions. In the case of large cardinals, we have it that the addition to **ZFC** of an axiom stating that there is a cardinal of some particular kind allows for the deduction of certain sentences which are not provable from **ZFC** alone. Some of those sentences might plausibly count as data, while others should not be considered as such. In particular large cardinal axioms have set-theoretic consequences (which may or may not count as data depending on the case in hand), but also arithmetical consequences which are much more plausible candidates for data. Consider, for example, the theory $\mathbf{ZFC} + \exists x\ x$ is measurable.[3] This proves that there is an inaccessible cardinal, which can hardly count as an elementary datum, but it also proves Π^0_1 arithmetical sentences not provable in **ZFC**, for example $Con_{\mathbf{ZFC}}$, which are much more plausible candidates for mathematical data.

Suppose I posit the existence of a measurable cardinal in order to prove some Π^0_1 arithmetical sentence which I believe to be true. There is an important respect in which positing material bodies in order to systematize our sense data differs radically from this. Consider, for example, the case of positing a table to account for the coherence and continuity of my table-ish experiences with respect to leaving and re-entering a particular room. In

[3] κ is measurable iff it is the critical point of some elementary embedding from V to a transitive class M (Martin and Steel 1989, p.73).

the case of the table-posit, the particular material body being posited, *that* table, plays a crucial role in the systematizing of my table-ish experiences. If 'accounting for the facts of our own life' (as Russell puts it) is to be made any simpler by this posit, it is because *that* particular table is there. The mere truth of the sentence 'there are tables' is insufficient for such purposes. Accounting for the facts of my experience is no simpler, for instance, if there is a table somewhere else, but that *this* is merely a series of sense data. Indeed that seems to rather complicate the story if some (non-hallucinatory) table-ish experiences are of actual tables, and some are merely of sense data. The existential generalisation over tables does not on its own systematize our experience, it is the particular posits of particular tables that perform such a function on a case-by-case basis.

In the case of the measurable cardinal, however, things are not so. It is merely the increase in the strength of our set theory that accounts for the elementary arithmetical consequences. Although it might seem natural to think that it is the least measurable cardinal which accounts for the arithmetical data in this scenario, in truth no *particular* measurable cardinal explains the arithmetical consequences. Unlike in the case of tables, the existence of *any* witness to the existential generalisation will do the job. Worse still, the role played by even the posit of a measurable cardinal is dispensable with respect to the elementary consequences, because it is only the consistency strength of the assertion which matters. For example, the addition of a measurable cardinal allows us to prove $Con_{\mathbf{ZFC}}$. But positing any stronger axiom of infinity, such as the existence of a Woodin cardinal, would do the job just as well.[4] Arguably, it would do the job *better* since it would prove further Π^0_1 arithmetical sentences which are not accounted for by the weaker theory (e.g. it proves $Con_{\mathbf{ZFC}+\exists x\ x \text{ is measurable}}$).[5]

Something quite substantial is at stake here, because *if* large cardinals really were required to make sense of mathematics in the same way that material bodies are required to make sense of our ordinary sense experience, that would afford to them a *massive* degree of quasi-scientific justification, because the existence of material bodies is a far more certain proposition than any particular natural law in the sciences. Even well-established scientific

[4]A cardinal κ is Woodin iff for all $f : \kappa \rightarrow \kappa$ there is some $\alpha < \kappa$ such that $\{f(\beta) : \beta < \alpha\} \subseteq \alpha$ and an elementary embedding j from V into a transitive inner model M such that α is the critical point of j and $V_{j(f(\alpha))} \subseteq M$ (Kanamori 2009, p.360).

[5]As long as we are considering the facts of common experience here, the argument can even be re-run with respect to theoretical physical entities. For example, some *particular* arrangement of elementary particles at a particular spatial location is required to explain why I see a table every time I go into the room. The mere existence of some such particles somewhere is insufficient.

laws are at times overthrown or precisified in the face of new experimental data, as occurred with Newtonian mechanics. Moreover, even the most well-established scientific laws can rest uneasily with one another, as is the case with general relativity and quantum mechanics. By contrast, our belief in material bodies is practically certain. Most of us are inclined to agree with Russell that we can't *prove* that we are not dreaming; and yet I am more certain, for instance, that the experience of writing this paper is veridical than I am of any philosophical conclusion reached in it. Since elementary mathematics is no less evident than the experiences we have in the ordinary course of life, if large cardinals were like material bodies in this sense, then we could be overwhelmingly confident that they exist.

This is not to say that Gödel ever seriously entertained the justification of large large cardinal axioms in this sense; where he makes these kinds of assertions he is speaking of sets *generally*. Although this of course includes such cardinals if they exist, a more charitable reading of the passage would interpret these remarks as directed toward the elementary parts of set theory. It is an interesting question which parts of set theory, if any, can be justified by appeal to an analogy with material bodies, though for my purposes it is redundant, since Gödel takes intuitive justifications to be available for these elementary propositions anyway.[6] In any case, the material bodies analogy, though not offered as a specific defence of large large cardinal axioms, promises a huge degree of regressive support for the existence of certain sets, so it is significant that it cannot be used to justify large large cardinals in particular.[7] Rather, a more modest quasi-scientific justification for large large cardinal axioms must be sought, in which axioms are taken to be analogous to scientific laws.

[6]Chihara (1982) takes very seriously Gödel's claim that we have *as much* reason to believe in sets as in material bodies, and rejects it wholesale. I think this conclusion is probably correct, but Chihara gives little consideration to the possibility that we have a good justification for the belief in sets of a similar kind to the justification our belief in material bodies enjoys, even if the quality of the justification is not equal in both cases. Given what Gödel says elsewhere about quasi-scientific justification, it seems probable to me that he suffers from an uncharacteristic lapse of caution in the passage quoted by Chihara, and that the slightly weaker position is Gödel's own.

[7]Maddy (1990, p.31) takes it that the *primary* function of intuition in Gödel's epistemology is to provide intuitive data that is accounted for by mathematical theories by way of analogy to physical bodies. Needless to say, this assessment does not accord with the reading of Gödel offered here.

2 Gödel and the Regressive Method

The version of the science–mathematics analogy that Gödel draws on most heavily has its origins in Russell's regressive method of finding justification for axioms. The similarity between Gödel and Russell runs deep here. Russell had in 1907 drawn a 'close analogy between the methods of pure mathematics and the methods of the sciences of observation' (1907, p.572). Here Russell claims that mathematics, like every science, has a body of commonly accepted propositions for which broader theory is supposed to account. These are known as 'data' or 'facts'. In the empirical sciences, the facts are accounted for by proposing laws of nature which collectively predict them. Analogously, in mathematics the most elementary facts are accounted for by proposing axioms which deductively imply them.

Gödel cites firm approval of this method, and predicts that it will be even more successful in the future (1944, p.121), so a more thorough analysis of Russell will assist in our evaluation of Gödel here. Of course in the 1944 article, Gödel is discussing sets generally, not large large cardinals in particular (as noted above in connection with his first analogy). Nonetheless, we'll see whether the considerations at work in the regressive method can be put to use in justifying these axioms.

Russell sharply separates the epistemological problem, which is also the problem of the present paper, from the psychological and historical (in some cases pre-historical) problem of how we come to believe the propositions identified as data. The *empirical* premises of a belief are those propositions which cause us to believe the data, whereas the *logical* premises are logically less complex propositions from which the data is to be deduced. Take, for example, the proposition that $2 + 2 = 4$, which ought to count as common fact if anything does. Russell conjectures that the empirical premises of this belief will be various beliefs acquired from everyday life, such as ancient shepherds repeatedly noticing that two pairs of sheep always make four sheep, and similar. By contrast, the logical premises of this data will be formulae in a system of mathematical logic or axiomatic arithmetic from which '$2+2 = 4$' can be derived.

The point of interest for Russell is that, for the greater part of mathematics, the simple picture on which the empirical premises and logical premises coincide holds good. In other words, we believe a mathematical proposition precisely because we have a proof of it from simpler propositions which we already accept. With respect to elementary propositions, including $2+2 = 4$, however, this is plainly a misleading picture, since the truths of elementary arithmetic are far more evident than the axioms of any system from which they could be derived. This leads Russell to conclude that the method of discovering and justifying foundational principles in mathematics is 'substantially the same as the method of discovering general laws in any other science' (1907, p.573).

Given the similarity of methods of justification, it is unsurprising that for Russell the degree of verification obtained by axioms in mathematics is alike to the degree which may be claimed for the laws of physics. As he puts it 'when the general laws are neither obvious, nor demonstrably the only possible hypotheses to account for the [data] then the general laws remain merely probable' (1907, p.573).

There is some lack of clarity as to *which* general laws can be, or should be, justified regressively, and hence potentially without complete certainty. In the original paper, Russell seems to think that general logical laws like $\phi \rightarrow \phi$ can be justified in this way (1907, p.576). This strikes me as somewhat bizarre, since such a law seems as evident as a proposition about one's present sense data. Later on, however, Russell appears to shift into thinking that the laws of logic are self-evident upon reflection, and do not require regressive justification (Russell 1914, pp.70–71). This idea is much more appealing, since the most obvious logical laws can then be themselves considered as data on a par with elementary mathematical propositions. But it is still unclear where exactly to draw the boundary between generalities like $\phi \rightarrow \phi$ which may be considered part of the data, and generalities which are designed to account for the data, like the Peano axioms. Despite this, the proposal constitutes a radically non-traditional epistemology of axiomatic systems, in that axioms may be afforded fallible justification in the absence of *any* intuitive evidence.

Gödel's view is in many respects similar to Russell's. An element of Gödel's approach to the issue that differs from Russell's is that he is more concerned with the verification of axioms by the enhancement of what today would be called 'theoretical virtues'. In discussions of the regressive method, Russell focuses on confirmation which flows from two sources: the obvious truths which axioms or laws entail, and the obvious falsehoods which they do not (1907, p.578). Where an axiom candidate is justified because it accounts for some data which have no proof in the unsupplemented theory, I'll call such justification *strictly regressive*. But there is another respect in which posits in science can contribute to verification of a theory beyond its observational consequences, by discriminating between the virtues of competing empirically adequate theories.

To take a famous example, Einstein's theory of general relativity describes spacetime as curved. Logically speaking, we could maintain that spacetime is actually flat, and posit compensating fields. The result is a theory which has the same observational consequences as Einstein's, but which preserves our pre-theoretic Euclidean conception of the geometry of physical space. However, corresponding to each relativistic model there are infinitely many distinct but empirically indistinguishable alternative Euclidean worlds. So despite their empirical equivalence, the Euclidean alternative is not seen as a genuine competitor to general relativity, because of the latter's tremendous advantage in terms of simplicity, naturalness, and other theoretical virtues (Sklar 1992, pp.62–63).

Russell does give consideration to theoretical virtue in cases like this, where there are multiple candidate hypotheses which can account for certain data. For instance, in (1908, pp.242–243), he adopts the axiom of reducibility on the grounds that it does the work required of a theory of classes, but is considerably more convenient than a theory of classes suitably modified to avoid the paradoxes. More generally, he emphasises that axiomatic theories which predict the data serve to organize our knowledge and make it more manageable (1907, p.580). It is not apparent to me, however, that Russell regarded it as possible to justify mathematical axioms *solely* on the grounds that they substantially enhance virtue. He never, to my knowledge, offers an explicit justification for adding an axiom to a theory which has *no* strictly regressive support (i.e. one that is not sufficient to take account of any data not accounted for by the unsupplemented theory), but which does substantially enhance the the theoretical virtues of the unsupplemented theory. On the other hand, Gödel's remarks strongly suggest that he does believe such justification to be possible (see §5). In general, Gödel places much more emphasis on this element of the analogy between mathematics and science than does Russell.

We'll postpone for now the discussion of what such theoretical virtues might be in the mathematical case. The important point is that Gödel is quite alive to the degree of revisionism in this epistemological picture. At the time of writing (1964), Gödel was sceptical about mathematicians' present ability to verify large cardinal axioms by quasi-scientific methods, though he does claim that in principle they could be verified 'at least in the same sense as any well-established physical theory', even in cases where the axioms entirely lack intuitive justification (1964, p.261). Gödel, much like Russell, is clear that certain axioms may possess both intuitive and quasi-scientific justification (1944, p.121), but the main focus is on axioms without *any* intuitive force, such as large large cardinal principles. The verification of such axioms is described as being 'only probable' (1964, p.269).

This all stands in sharp contrast to the epistemology of intuition discussed by many commentators on Gödel; although he did not take intuition to be infallible, it seems he thought that the existence of certain large cardinals could be established definitively by such methods. If however, our mathematics were to make use of axioms possessing *only* quasi-scientific justification, 'mathematics may lose a good deal of its "absolute certainty"' (1944, p.121). Gödel's confidence in his revisionist epistemology is such that in the Gibbs lecture he even claims that the monopoly of deriving 'everything by cogent proofs from the definitions' may turn out to be 'as mistaken in mathematics as it was in physics' (1951, p.313).

In summary, Gödel's introduction of quasi-scientific methods into the theory of large cardinals constitutes a stark departure from his more traditional epistemology of arithmetic and the more basic elements of set theory. He hopes that some justification of set theory can be offered based on two analogies. The first is that sets help us systematize mathematical experience just as material bodies do our sensory experience. We saw that this analogy was ill-founded, at least in the case of large large cardinals. The second analogy is between large large cardinal axioms and scientific laws, derived from the work of Russell. The central elements of the analogy are as follows:

1. Certain mathematical truths stand to set theory as elementary data stand to physical theories.

2. Positing large cardinals can account for this data, similarly to how laws of nature can account for the data of scientific theories.

3. Such posits can be justified either by being necessary for the deduction of elementary data, or by enhancing the theoretical virtues of theory to which they are added.

4. Consideration of such theoretical virtues can be so significant as to admit into mathematics axioms (and hence theorems) which only have a probable justification.

in the next three sections, I'll clarify the central elements of this analogy: what mathematical truths count as data? In what way can large cardinals account for this data? How do large cardinal axioms enhance theoretical virtue?

3 Mathematical Data

If large large cardinal axioms are to find their justification in accounting for the mathematical data in a certain way, then some delineation of which mathematical propositions constitute that data is plainly required. Though such a delineation need not be made entirely precise in order to see the force of Gödel's analogy, we clearly need some account of what the large cardinal axioms are supposed to be accounting for if the analogy with the natural sciences is to be informative at all. At the very least, some delineation of the data is required for the account to be non-trivial; if *any* mathematical truth qualified as data, then any true large cardinal axiom would be self-certifying in a way that could not be considered scientifically respectable. In this section, I'll examine various possible accounts of mathematical data that are suggested in Gödel's writings and elsewhere, and argue that only one of these has any hope of being plausibly seen as analogous to data in natural science, if Gödel's argument is to be of any use in justifying large large cardinal axioms.

3.1 Perception and Objectual Intuition

Given the attention that has been given to Gödel's remarks on 'a kind of perception' in mathematics by later readers, we might expect that the data to be accounted for are mathematical perceptions, and set theory is verified to the extent that it 'predicts', i.e. proves, the propositions which we can perceive to be true. On such a view, there would indeed be an overwhelming analogy between mathematical and scientific theory, namely that both of them are a means of systematizing and streamlining the data we experience into a cohesive theory.[8]

I've argued elsewhere that we should understand Gödel's remarks about perception in mathematics as referring to Kantian or Hilbertian intuition, and not to perception in a literal sense. Moreover, I've argued that *intuition of* (i.e. singular objectual intuition) doesn't play a significant role in Gödel's platonistic epistemology. That said, some view whereby this faculty provides data to be accounted for regressively may still be worth considering, given the enormous degree to which it renders science and mathematics analogous. A clear statement of the view is given by Maddy (1990, pp.44–45). She claims that if we are persuaded of some kind of platonism or realism, then we should expect scientific and mathematical epistemology to be analogous. Since some scientific beliefs are pre-theoretical and non-inferential, so too should we expect this in mathematics. In science, these beliefs are formed by perception, and so in mathematics they should also be formed by perception, or something perception-like. The real problem with this view, at least with

[8]This is not to be confused with Quine's view (Quine 1951a, p.45), according to which mathematics is also an attempt to systematize and streamline the data of experience. On Quine's view, there is a single kind of data, which is accounted for by scientific (including mathematical) theorizing as a whole. The view presently being considered however, posits *two* kinds of data, which are accounted for by the natural sciences on the one hand, and mathematics on the other.

respect to the justification of large large cardinal axioms, is that it is unstable between the two main ways of thinking about mathematical perception: on one account it is far too weak, and on the other it is so strong as to be trivial.

Firstly, we might imagine that deliverances of singular objectual intuition, something like perception (but not perception itself), must be accounted for by a mathematical theory. That is, we need to provide a formal theory $\mathbf{T}$ such that $\phi \in \mathbf{T}$ if the truth of ϕ is apparent given intuition of the objects concerned (much as the truth of colour-ascriptions are made apparent by looking at the relevant objects in favourable visual conditions). There are of course questions to be raised about how such a faculty of intuition might function, but on anything analogous to Hilbert's view of intuition, what is given by this faculty will be such a tiny fraction of mathematics that no large cardinal axioms will have a role to play in accounting for it. If for example, deliverances of intuition concerning number are captured by primitive recursive arithmetic (i.e. quantifier-free arithmetic), then no objects other than the natural numbers are required to explain the data.[9] Of course, set theory with a large cardinal axiom would *also* explain this, and would also solve lots of open set-theoretic problems besides. But such an explanation would surely fall foul of considerations of simplicity and economy of both ontology and ideology.

For example, Newtonian mechanics is a simpler theory than Newtonian mechanics plus evolution by natural selection. In a perfectly Newtonian world with no living creatures, the supplemented theory would do all the explaining of the base theory, and would additionally answer lots of questions about the heritable traits of living things. But that would not make the theory any *better* because, by our assumption, there is no data for the complicated theory to account for that the basic theory could not. And similarly for large cardinal axioms (indeed, set theory in general), if we take the data to be limited in advance to what is given in singular objectual intuition.[10]

[9] The qualification 'concerning number' is required to avoid questions that could be raised about *geometric* intuition, which plausibly requires resources going beyond those available in primitive recursive arithmetic. Though there are interesting questions about such cases, here is not the place to discuss them. Complications immediately arise concerning geometrical intuition when one considers the modern conception of geometry as lacking an intended interpretation, or the possibility that geometric intuition could be explained as a spatio-perceptual faculty, rather than a genuinely mathematical one.

[10] I don't want to commit myself to the view that singular objectual intuition *is* captured by primitive recursive arithmetic. But it does seem to be a reasonable approximation (the classic presentation of this view is (Tait 1981), though Tait rejects the Hilbertian claim that the *security* of finitary arithmetic is grounded on our ability to represent its objects in intuition). Furthermore, I expect any account of objectual mathematical intuition in the vicinity would equally support a slightly modified argument to the same conclusion.

Alternatively, we might follow Maddy and think that sets themselves can be literally perceived, with no need for the surrogate faculty of intuition. This gives a much richer relation between us and the objects of mathematics than traditional Kantian or Hilbertian intuition, but indeed the relation is much *too* rich. Since perception requires a causal connection between the perceiver and the object of perception, the only sets we can see, according to Maddy, are those with physical objects in their transitive closure. Moreover, *any* sets with the same physical objects in their transitive closure are co-located. A consequence of this is that for any ordinal α, there is a set of rank α where any physical object is (Maddy 1990, p.59).

When it comes to data then, there are two options. If, for whatever reason, we can only see sets of low rank, then it is hard to see how perception would fare any better than objectual intuition did. On the other hand, if we can see any set in our visual field, then there is no need for large cardinal axioms to *account for* the data, since, for any true large cardinal axiom, we'd just be able to see sets of any rank necessary to validate the axiom. I take it that the existence of large cardinals shouldn't be considered part of the data that large cardinal axioms account for, so it seems that the science–mathematics analogy cannot support large large cardinal axioms if we take the data to be given by either singular intuition or perception of sets.

Although a proper discussion would take us too far afield, I want to make it clear that I think objectual intuition is insufficient as a source of data for which large large cardinal axioms *specifically* are required to account. Perhaps objectual intuition is well-suited to providing regressive support for much weaker axioms; but according to the Gödelian account, such weaker theories can be validated by propositional intuition, hence there is no need for such a discussion here. We'll also discuss below the possibility that objectual intuition can still contribute to the data, even if it is insufficient to provide all of it.

3.2 Ordinary Mathematics

If the analogy between mathematics and natural science is to validate a large large cardinal axiom, we need a collection of data more expansive than what is given in intuition that does not include statements about sets of arbitrary rank. An initially promising class that appears to fall between these two extremes would be 'ordinary' mathematics. In emphasising the foundational role of set theory, we might examine the theorems accepted by mathematicians in other areas, and see how strong set theory is required to be to account for these theorems.[11]

However, as a source of data, ordinary mathematics suffers the same defect as objectual intuition, because very little set theory is strictly required to account for ordinary mathematics. Gödel's own view in 1964 was that the lack of observable consequences in other fields was such that 'it is not possible to make the truth of any set-theoretical axiom reasonably probable in this manner' (1964, p.269). We find similar views decades later in the work of Quine (1990, pp.94–95), asserting that the higher reaches of set theory should indeed be *pruned* on account of their irrelevance (although Quine was of course concerned with their relevance only to *applied* mathematics). And decades after Quine, it is still difficult to find an example of a mathematical result from outside set theory which requires a large cardinal axiom for its verification. As Potter puts it 'the overwhelming majority of 20th century mathematics is straightforwardly representable by sets of fairly low infinite rank, certainly less than $\omega + 20$' (2004, p.220). This assessment may be unduly bleak regarding large cardinals as a whole; certainly the central importance of Grothendieck universes in algebraic geometry casts some doubt on the assessment. But even here, the cardinals involved are small by the standards of current set theory.[12] So the 21st century, at least in respect of the application of *large* large cardinals, shows no sign of being any different from its predecessor.

This is not to say that large cardinal axioms don't have *any* consequences that are of significant interest to mathematicians working outside set theory; large cardinal axioms all have number-theoretic consequences, and can at times be used to solve open mathematical problems (see §5 below). The point to note for now, however, is that such consequences are not regarded as true *in advance* of positing a large cardinal axiom, so cannot be seen as *data* for which such an axiom might account. They could, perhaps, be seen as analogous to the additional consequences that scientific theories have which are not themselves data. That is, perhaps such propositions are analogous to scientific *discoveries*. But such a proposition is not to be believed unless we believe the theory of which it is a consequence, and hence it cannot play the same role as the data do in establishing the truth of the theory. Outside of the context of large cardinals, Russell writes that 'the logical premises have, as a rule, many more consequences than the empirical premises, and thus

[11] In proposing this conception of data, is it convenient also to relegate category theory to the realm of extra-ordinary mathematics along with set theory, for the same kinds of reason in each case.

[12] In particular, any Grothendieck universe is either $\emptyset$, V_ω, or a strongly inaccessible cardinal. Many thanks to Daniel Isaacson for highlighting the relevance of algebraic geometry in this context.

lead to the discovery of many things which could not otherwise be known' (1907, p.574). Large cardinal axioms certainly have many such consequences, but the point is that they cannot properly be considered logical premises for mathematics at large, since ordinary mathematics can do perfectly well without them.

As with the case of objectual intuition, it may well be that a study of ordinary mathematics would provide strong regressive support for certain set-theoretic axioms. However, those axioms would be substantially weaker than the large large cardinal axioms which are our present concern.[13]

3.3 Arithmetical Data: Primary and Secondary

Gödel's own suggestion is that the data should come from arithmetic, 'the domain of the kind of elementary indisputable evidence that may be most fittingly compared with sense perception' (1944, p.121).[14] This is a distinct proposal from the one just discussed. After all, not all verified propositions in ordinary mathematics are arithmetical; conversely not all verified arithmetical propositions are found in ordinary mathematics, since many of them are distinctly meta-mathematical.

On the face of it, arithmetic is a much more promising source of data than singular intuition: if the data are sufficiently rich that an incomplete theory is required to account for them, then this opens up the possibility of formulating a sequence of increasingly powerful theories accounting for more and more of the data, with no limit to the strengthening process. This is exactly what the large cardinal hierarchy promises to provide.[15]

[13]It's perhaps worth emphasising that here I'm only discussing the *strictly regressive* justification of large large cardinal axioms, not the quasi-scientific justification of them overall. At this stage, I do not consider myself to have said anything against the view that large large cardinal axioms can be verified by evaluating their theoretical virtues, which might include the ability of these axioms to solve open problems. This issue will be taken up below in §5.

[14]It is worth clarifying that Gödel is here using the term 'sense perception' is a specialized way, in the context of discussing Russell's regressive method. Hence 'sense perception' should here be taken to mean a proposition functioning as data that should be deducible from the wider explanatory theory.

[15]It may at first sight appear that the growth of the large cardinal hierarchy *does* have a limit. We know, for example, that Reinhardt cardinals are too large to exist if the axiom of choice is true. This is an easy consequence of Kunen's theorem that there is no non-trivial elementary embedding from the universe into itself, since a Reinhardt cardinal is the critical point of just such an embedding (Kanamori 2009, pp.318–319). While such cardinals *are* too large, I can't see how this differs from the requirement that the large cardinal axioms must be consistent. The formulation of cardinal axioms too strong for **ZFC** does not imply that *within* the class of large cardinal axioms consistent with **ZFC**,

Since there are a great many arithmetical truths yet to be formulated, let alone believed, not all arithmetical truths can function as data. In identifying a select few of these truths as data, a promising suggestion would be those arithmetical truths expressed by a Π^0_1 sentence. On the one hand, such sentences form a natural class of arithmetical sentences which can be considered suitably elementary, given that they are of the form of universal generalizations over the numbers. Secondly, Gödel's theorems imply that any recursively axiomatized consistent set theory will be Π^0_1-incomplete, guaranteeing that the data are of a suitably inexhaustible kind. Finally, all large cardinal axioms have Π^0_1 arithmetical consequences, meaning that the positing of increasingly strong axioms is guaranteed to have relevance to the data. Hence identifying the data with this class seems most likely to justify the kind of maximalism about the height of the hierarchy that Gödel and others hope to found in terms of the science–mathematics analogy.

Of course, Δ^0_0 and Σ^0_1 arithmetical sentences are just as elementary, and may well merit consideration as data. But for the purposes of large large cardinal axioms, such sentences won't matter much. The Δ^0_0 arithmetical sentences are all equivalent to Σ^0_1 arithmetical sentences (by prefixing redundant quantifiers), and **PA** is complete with respect to this latter class, so we know in advance that no consistent large cardinal axioms will settle any of these sentences that we could not have settled without their help.

An identification of the data along these lines is made by Koellner (2009a, p.98). He distinguishes the 'primary' data, which are previously verified Δ^0_1 sentences, and the 'secondary' data, which are the Π^0_1 universal generalisations of these. Koellner's motivation for this selection is that verified Δ^0_1 sentences are analogous to observation sentences in the sciences, and hence their Π^0_1 generalisations are analogous to observational generalisations in the sciences. He claims further that 'in mathematics the secondary data can be definitely refuted but never definitely verified' (2009a, p.98). This claim is made on the basis of the analogy with physics, yet it is deeply implausible in the case of mathematics. After all, any Δ^0_0 sentence is also Δ^0_1 (since it is logically equivalent to itself prefixed with redundant quantifiers), and we seem to able to verify all sorts of Π^0_1 sentences which are universal generalisations of Δ^0_0 formulae (e.g. that every prime is odd or is identical to 2). Given this implausibility, it is perhaps tempting to think that the data are meant to be some restricted class of Π^0_1 sentences which generalise verified Δ^0_1 sentences. But Koellner's remarks tell against this. For example, he claims that '[t]wo theories are mutually interpretable if and only if they prove the same Π^0_1-sentences, that is, if and only if they agree on the secondary data'

there is a limit to the process of strengthening **ZFC** by successive large cardinal posits.

(2009a, p.98). This means that there is no room for theories to agree on the secondary data and disagree on the full class of Π^0_1 sentences.

A further problem with Koellner's suggestion, and indeed with the more general identification of the data with the Π^0_1 arithmetical data, is that we are no more persuaded of the truth of every true Π^0_1 arithmetical sentence in advance than we are of the truth of every true arithmetical sentence. Take, for example, the even perfect number conjecture (EPN). This states that all numbers which are perfect (i.e. are the sum of their proper positive divisors) are even. This conjecture is clearly Π^0_1, however it is well-documented that mathematicians are (at least collectively) ambivalent regarding the truth of EPN (Baker 2007, p.63). So, if the conjecture is true and a theory proves it, we should regard the proof as analogous to a surprising scientific discovery, and *not* as an account of any data.

Given the faults in Koellner's primary/secondary classification, the important question now is how to delimit in advance which true arithmetical sentences at most as complex as Π^0_1 are to be considered data. It's entirely possible that no sharp delimitation is possible; indeed the closer the analogy between mathematics and the sciences, the less we should expect a sharp delimitation to be possible. Nonetheless, we can give a preliminary taxonomy of the kinds of arithmetical proposition which should pass muster.

3.4 Arithmetical Data: Hard and Soft

Firstly, there are those sentences originally discussed by Russell, which gain their status as data for broadly Millian reasons, the original example being that two sheep and two sheep are always observed by shepherds to yield four sheep. Since '$2 + 2 = 4$' is Δ^0_0, it is also Π^0_1, and hence is of the right shape for our data class. Moreover, we believe it to be true pre-theoretically, and indeed with more certainty than the axioms themselves. Hence these *Russellian data* should be admissible for Gödel's analogy too.

We saw above that Gödel takes a perception-like relation to hold between us and mathematical objects, most plausibly construed along Kantian or Hilbertian lines as the singular representation of an object to a thinking subject. If this has any significant role to play in Gödel's philosophy, it is in providing *some* of the mathematical data (though for reasons discussed above, it cannot provide all the data). Such intuitive data, like the basic Russellian data, will no doubt be restricted to propositions of a fairly simple sort. Since intuitive representation is supposed to be singular, even without a thorough account of how such intuition is supposed to work, we can tentatively say that arithmetical propositions which we can verify on the basis of this faculty should be equivalent to to a Π^0_1 or Σ^0_1 sentence, as required here. I won't dwell on this issue, since it's fairly clear that Gödel's scant remarks on singular intuition radically underdetermine the theory required to account for them. I mention the issue only because Kantian intuition is a plausible source of data going slightly beyond the Russellian. There might, for example, be simple additions such as $51,000,000,000,000 + 1 = 51,000,000,000,001$ which could plausibly be verified in intuition, but of which no plausible Millian account can be offered. It would be a wealthy shepherd indeed whose work necessitated repeated exposure to concrete instances of the addition above!

In the non-mathematical context, Russell distinguishes between *hard* and *soft* data (1914, lecture III). The distinction is broadly psychological in nature, and is not supposed to be exhaustive or exclusive. But as a heuristic it is still helpful for our purposes to consider the degree to which data can be classified as hard or soft. The paradigmatic hard data for Russell are the laws of logic, Russellian mathematical data in the above sense, and facts about one's own sense data. We can also, for the sake of thoroughness, include propositions verifiable in intuition here. The common characteristic is that Cartesian reflection on propositions of this kind do not induce doubt in us as regards their truth. Soft data are, by contrast, those which are open to at least philosophical doubt, such as the existence of material objects or other minds. Though he does not put it in quite these terms, in the Gibbs Lecture Gödel suggests that soft data in mathematics are admissible for quasi-scientific purposes.

In particular, he argues that a platonist should feel comfortable with the verification of number-theoretic claims by enumerative induction (i.e. verification of universal number-theoretic claims by verification of instances up to large integer values). He writes:

> I admit that every mathematician has an inborn abhorrence to giving more than heuristic significance to such inductive arguments. I think, however, that this is due to the very prejudice that mathematical objects somehow have no real existence. If mathematics describes an objective world just like physics, there is no reason why inductive methods should not be applied in mathematics just the same as in physics (Gödel 1951, p.313).

In trying to reconstruct a Gödelian conception of mathematical data then, it is reasonable to suppose that Π^0_1 arithmetical sentences verified in a sufficiently large number of instances should be considered as soft data. Hence if, for example, Goldbach's conjecture were derivable from a large cardinal axiom, we should count that as regressive support for the axiom candidate.[16] This is because most mathematicians believe the conjecture to be true despite lack of a proof (Echeverria 1996, p.42). The obvious explanation as to why mathematicians typically hold this belief is that the conjecture has been verified in an enormous number of instances.[17]

Of course, there are a number of philosophical issues with soft data of this kind. For one thing, it isn't even known whether Goldbach's conjecture is independent of **PA** (and indeed, if it is false, its negation is provable in **PA**). If it *is* provable in **PA**, then it's possible that the proof is so complex that nobody could feasibly carry it out. If this is the case, then a simpler proof from a large cardinal axiom would provide justification for that axiom merely in terms enhancement of theoretical virtue, rather than a more compelling strictly regressive justification (we'll return to issues around the 'speed-up' of proofs in §5).

Secondly, Gödel's statement that 'there is is no reason why inductive methods should not be applied in mathematics' is false. A very good such reason was offered by Frege, namely that induction in physics is lent plausibility by the fact that *ceteris paribus* any region of space and time can be supposed similar in the relevant physical respects. However the same is not true of the numbers, since the position they occupy in the number series makes a great deal of difference to their arithmetical properties, such as their divisors, primality, and so on (Frege 1884, pp.14–15).

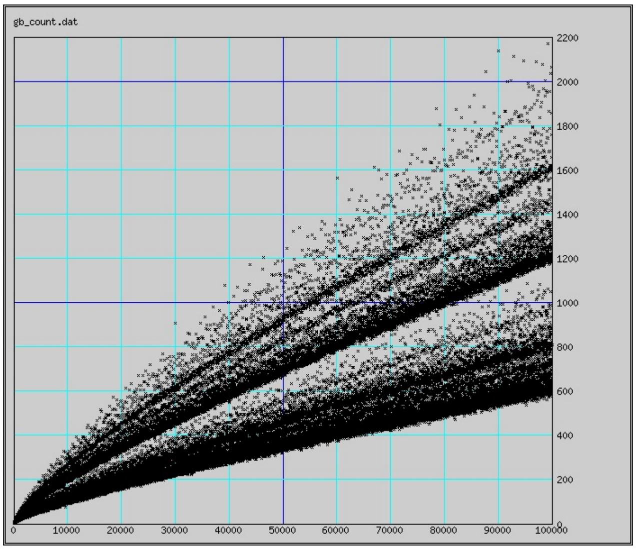
I think that as far as reconstruction of Gödel goes, the results of sufficiently extensive enumerative induction should be admitted as soft mathematical data. Philosophically, however, I think this is mistaken, a view which appears to accord with mathematical practice. Although much work *has* gone into verifying large numbers of its instances, Baker (2007, pp.69–70) makes a compelling case that enumerative induction is not the source of widespread belief in Goldbach's conjecture.

Baker argues instead that the belief has its origins in Cantor's partition function. With a given even number ≥ 4 as its argument, the partition function takes as its value the number of ways it can be decomposed into the sum of two primes. Though this function does not increase monotonically, its graph, displayed below for even arguments from 4 to 100,000, is certainly

[16]In modern form, Goldbach's conjecture states that any even number greater than 2 is the sum of two primes.

[17]At least 2×10^{17} instances have been checked. Up to date information is available at Tomás Oliveira e Silva's website at 'http://sweet.ua.pt/tos/goldbach.html'. Accessed 05/04/2019.

suggestive.[18]



Baker argues that the increase in mathematicians' confidence of the truth of Goldbach's conjecture coincides with investigation of the partition function, and hence that this confidence is not based on enumerative induction alone (which Baker takes to be illegitimate for the Fregean reason above). Rather, the confidence comes from the apparently increasing cone-like pattern exhibited by the graph of the partition function.

At this point, one might be tempted to think that enumerative induction is after all the source of the mathematicians' beliefs here, with a small change in perspective: the induction is that for many even arguments from 4 onward, the value of Cantor's function isn't 0, therefore Goldbach's conjecture is true. But this would be too quick: as Baker argues, there is more than enumerative induction going on here. Given the apparently-increasing pattern of the graph, the 'hard' cases for Goldbach's conjecture should be amongst very small numbers already tested manually. In other words, the sample cases observed are biased *against* Goldbach's conjecture, and if it were false, we should have found the counterexample amongst the previously studied instances. So mathematicians don't need to be seen as accepting the result of simple enumerative induction here, but rather as accepting the result of enumerative induction over a sample biased against the conjecture. Baker takes this to be a distinct kind of non-enumerative inductive evidence for the

[18]The graph is taken from Mark Herkommer's Goldbach research site at 'http://www.herkommer.org/goldbach/goldbach.htm'. Accessed 07/09/2018. © Copyright, 1998-2014 Mark Herkommer. Permission to reproduce this graph has been kindly granted by the copyright holder.

conjecture (2007, p.71). Therefore, even if we do not wish to countenance soft data of the kind envisaged by Gödel, we may be inclined to think that some Π^0_1 arithmetical sentences should be admitted into our class of data on the basis of such non-deductive plausibility considerations.

Lastly, there is a kind of data with much Gödel scholarship has been preoccupied, namely the Π^0_1 arithmetical sentences constructed in the proof of Gödel's theorems, like Gödel sentences, canonical consistency sentences, and Diophantine sentences. When sentences of these kinds are constructed effectively from an axiomatic system which we recognize to be sound, it follows immediately that they are true, *and* that they are not 'accounted for' by the corresponding axiom system in the relevant sense. However, such sentences constructed from the axioms of a sound system which we do not believe in advance to be sound will not pass muster. Since the justification of such propositions is parasitic on the axiomatic system from which they are obtained, data of this kind will be harder the higher our degree of confidence in the soundness of the relevant axiom system. A proposition such as $Con_{\mathbf{PA}}$ should be regarded as data of the hardest kind, with $Con_{\mathbf{ZFC}}$ as perhaps somewhat softer. Something like $Con_{\mathbf{ZF}+\exists x\ x \text{ is Reinhardt}}$ should not be considered data at all.[19]

In summary, "ordinary" mathematics, mathematical perception, and singular intuition cannot supply a collection of data by which the analogy between mathematics and natural science could justify the positing of large large cardinal axioms. The elementary part of mathematics which most plausibly can behave as data is number theory. A restriction on which number-theoretic sentences can be considered data is nonetheless required. Although we cannot determine which statements precisely are data, a natural class consists of Russellian data, together with the Π^0_1 sentences generated by Gödelian incompleteness that we have reason to believe are true. Gödel's writings suggest that he thought the results of certain enumerative inductions should be considered as well; although I've expressed scepticism on the matter, it is plausible that some Π^0_1 arithmetical sentences should be considered data even in the absence of (formal or informal) proof, namely where there are strong heuristic reasons to suspect they are true. In the next sections, we'll examine the sense in which large large cardinals might be thought to account for such data.

[19] Although the existence of Reinhardt cardinals is known to be inconsistent with **ZFC**, it is unknown whether they are consistent with **ZF**. A recent attempt by Rupert M^{c}Callum to prove Kunen's inconsistency theorem without the axiom of choice (which would settle the question negatively) almost succeeded, though not quite. The events are documented by Joel Hamkins on his website at 'http://jdh.hamkins.org/tag/rupert-mccallum/'. Accessed 05/04/2019.

4 The Laws of Nature Analogy

Since large cardinals themselves are not analogous to material bodies as Gödel initially suggested, the alternative is that the axioms which assert the existence of such sets are analogous to 'laws of nature' or other theoretical posits. One respect in which an axiom playing the role of a law can be successful is *strictly regressive*, if it allows for the deduction of data which could not be obtained by weaker principles. The other possibility is that large cardinal axioms function as laws of nature the positing of which enhances the theoretical virtues of set theory. This section will be concerned with the strictly regressive justification of large large cardinal axioms, by analogy to laws of nature. Our key question is whether large cardinal axioms can account for any data that cannot be obtained without them, according to the delimitation of the data given in the last section. If so, that would give strong regressive support to the large cardinals project, since in the sciences we certainly do accept natural laws which are posited for such reasons. However, I'll argue that Gödel's analogy cannot be sustained in this case.

Whether the adoption of large large cardinal axioms can account for data not accountable for without them (or alternative axioms of similar strength) is of course tremendously sensitive to what we take the data to consist of. The outline of the data just given is neither sharply delimited, nor precisely defined. But critically, the sentences expressing such propositions are all provable in **PA**, or else are of a restricted kind of Π^0_1 arithmetical sentences independent of **PA**. So we can say something relatively precise about the sentences expressing the data (though admittedly not as precise as in Koellner's account), and hence can say something quite definite about the role large cardinals might play in accounting for them. As one would expect in advance, the data are (at least in one respect) not very complicated sentences, and their simple form might give us good reason to suppose that large cardinal axioms do allow us to account for data which cannot be accounted for in their absence. This is because the addition of any large cardinal axiom to **ZFC** will reduce the degree to which it is Π^0_1-incomplete, as can be seen from the arrangement of large cardinal axioms in a hierarchy of consistency strength.[20]

Indeed, we may even think that the inclusion of sentences constructed by Gödelian methods in our selection of data *guarantees* that positing a large cardinal axiom will relevantly account for some of the data, in the following way: suppose you are persuaded that intuitive considerations justify the be-

[20] As in fn.1 (above), it simply *appears* that the large cardinal axioms are so arranged. There is no theorem to this effect.

lief that the theory $\mathbf{ZFC} + \exists x\ x = \kappa(\omega)$ is sound, as mentioned above. You'll then certainly believe that the theory is consistent, but by Gödel's theorem $\mathbf{ZFC} + \exists x\ x = \kappa(\omega) \nvdash Con_{\mathbf{ZFC}+\exists x\ x=\kappa(\omega)}$. So $Con_{\mathbf{ZFC}+\exists x\ x=\kappa(\omega)}$ is a relevant piece of data, namely a Π^0_1 arithmetical sentence that we take to be true for Gödelian reasons. The adoption of a large cardinal axiom stronger than one asserting the existence of $\kappa(\omega)$ will allow you to prove the consistency sentence, and hence account for more data than the unsupplemented theory.

Since our newly supplemented theory accounts for more of the data, the analogy between science and set theory justifies (albeit not with certainty) a belief that it is sound, and hence that it is consistent. And the whole process starts again, justifying a set theory of ever increasing strength by extension with stronger and stronger large cardinal axioms.[21] The fact that a large cardinal axiom 'accounts for the data' gives us only probable reason to believe that it's true, so we have the expected gradual loss of certainty as we move up the hierarchy of large cardinal axioms as well.

In my view, this is the most persuasive quasi-scientific argument for large cardinal axioms in which those axioms are afforded strictly regressive support (as opposed to being merely virtue-enhancing), and as a point of interpretation it fits well with Gödel's general remarks on the issue. Firstly, the initial step of the argument requires the use of intuition to found the truth of the axioms of some strong set theory.[22] Secondly, the incompleteness theorems play a crucial role in the argument, since they are required to establish the need for a *series* of extensions via large cardinals, as outlined at (1964, pp.260–261). Thirdly, there is a clear sense in which the large cardinals 'account' for the data, since they do so directly via increasing the deductive strength of the base theory. Finally, the picture accords well with Gödel's twin claims that such axioms need no intuitive justification, and thereby introduce axioms and theorems into mathematics the truth of which can only be maintained as probable.

Compelling though it may be, this argument suffers from a serious philosophical flaw. According to the picture sketched above, it looks as if we might come close to a hierarchy of regressively justified large cardinal axioms constrained only by consistency. A potential problem is that the progressive decrease in the certainty of our axioms could perhaps lead to a decrease in regressive support such that we stop being justified in positing new cardinals

[21]The argument here is inspired by Gödel's remarks (1964, p.269), though the idea there is actually about intuitive justification, and does not mention large cardinals in particular.

[22]Even in papers like (1964), where the quasi-scientific programme is well underway, Gödel maintains that intuition has an important role in founding the general platonist interpretation of the axioms, suggesting that he does not think quasi-scientific justification is alone sufficient for developing such a picture.

rather early in this process. But that is merely a possibility. In reality, there are more substantial issues in the vicinity.

A first point to note is that, as we've seen, much of the mathematical data will be consistency sentences, or sentences which are equivalent to consistency sentences. An immediate problem that raises doubts about the need for large cardinal hypotheses with respect to such data is related to ordinal analysis. If the consistency sentences we can take to be data are those of sound recursive theories, then the consistency sentence should be provable via Gentzen's method of transfinite induction up to the theory's proof-theoretic ordinal. Since the proof-theoretic ordinal of a theory $\mathbf{T}$ is the supremum of ordinals for which there is a notation in $\mathcal{O}$ which $\mathbf{T}$ verifies is a notation, it follows that any proof-theoretic ordinal is $< \omega_1^{CK}$. The initial worry then, is that for the purposes of verifying elementary data, large cardinals are *excessive*; much of the work could be done using much more conservative resources in the large countable ordinals. That said, identifying the proof-theoretic ordinal of a theory is often far from straightforward. And moreover, we admitted that other Π^0_1 arithmetical sentences besides consistency sentences might pass muster as data, so the picture outlined above remains intact. There is, however, a much more severe problem with the proposal, to the effect that large cardinal axioms cannot be *required* to account for the data as construed.

In particular, the problem is that when a large large cardinal axiom 'accounts' for some otherwise unaccounted for piece of data, that gives us no reason to believe that the axiom is *true*. The key reasons are that **PA** is sound, and is complete with respect to Σ^0_1 arithmetical sentences, although this requires a little explanation. Suppose that δ is some large cardinal axiom consistent with **ZFC**, and that ϕ is a Π^0_1 datum such that $\mathbf{ZFC} \nvdash \phi$ and $\mathbf{ZFC} + \delta \vdash \phi$. Suppose ϕ is false; in that case $\neg\phi$ is equivalent to a true Σ^0_1 arithmetical sentence. Since **PA** proves all true Σ^0_1 arithmetical sentences, $\mathbf{PA} \vdash \neg\phi$. However, since **ZFC** extends **PA**, $\mathbf{ZFC} \vdash \neg\phi$. This contradicts our assumption that δ is consistent relative to **ZFC**, since $\mathbf{ZFC}+\delta \vdash (\phi\wedge\neg\phi)$. So (assuming **ZFC** is consistent), ϕ is true. Hence, we have accounted for a new piece of data, namely ϕ, by proving that it is true. Crucially, however, *at no point* was the truth of δ required. All that was used in the argument was the assumption that δ was consistent with **ZFC**.

To see this, suppose that γ is some axiom candidate consistent with **ZFC** such that $\mathbf{ZFC} + \gamma + \delta \vdash 0 = 1$. Suppose further that ψ is a Π^0_1 datum such that $\mathbf{ZFC} \nvdash \psi$ and $\mathbf{ZFC} + \gamma \vdash \psi$. The same argument as before suffices to show that ψ is true: if it is false, $\neg\psi$ is equivalent to a true Σ^0_1 arithmetical sentence. Hence, $\mathbf{PA} \vdash \neg\psi$, so $\mathbf{ZFC} \vdash \neg\psi$. This contradicts our assumptions, hence ψ is indeed true. Now γ and δ, by construction, are not *both* true. Yet the data for which these axioms were supposed to account are both true regardless. So the deduction of data which are not derivable in **ZFC** by large cardinals axioms provides no regressive support for the truth of these axioms; rather it at best supports the assumption of their consistency relative to **ZFC**.[23]

It seems that this observation should be of some concern to the platonist with a substantial notion of mathematical truth going beyond mere consistency. If, as I have argued we must, we restrict mathematical data to a special class of Π^0_1 arithmetical sentences, then as far as these data are concerned, we seem to be in a Hilbertian scenario with respect to large cardinal axioms, in so far as their consistency is as good as their truth. If we want to put the large cardinals programme on solid philosophical ground, it won't do to think that the open series of extensions of **ZFC** that we ought to believe consists simply of **ZFC** extended by propositions asserting the consistency of **ZFC** with certain large cardinal statements. But that picture is all that is regressively supported by the data, so the idea that the methods of theory choice in science can be applied in set theory is placed under considerable strain.

To put it the other way, recall that the analogy between mathematics and natural science was founded on the view that the subject matter of mathematics was analogous to the subject matter of natural science, such that some version of the methods of the latter was thereby admissible in the former. Given that the theories of natural science are certainly not confirmed by mere consistency, the platonist's case for large large cardinals is substantially undermined. The science–mathematics analogy prevents the Gödelian from taking up a Hilbertian conception of axioms, according to which consistency and truth coincide. The argument that I've presented shows that elementary mathematical data will *at best* support the view that large cardinal axioms are consistent, and not that they are true. So either the laws of nature analogy must be abandoned, or else the platonist must admit that it fails to justify a belief in large large cardinals.

None of this is to say that the Gödelian *cannot* justify the truth of large large cardinal axioms by other quasi-scientific methods; after all they can still argue that the truth of such an axiom can enhance the theoretical virtues of set theory to a greater extent than can the corresponding consistency sentence. But this is a much weaker kind of support far more open to doubt. Indeed, the situation for quasi-scientific justification keeps getting worse: the strongest form of regressive support that has been offered for large cardinal axioms was that the existence of large cardinals was only as open to doubt

[23]Technical details relevant to this point are explained further in (Potter 2004, pp.217–218).

as the existence of medium-sized dry goods. But as we saw, that analogy could not be sustained. Now we have seen that large cardinal axioms do not even contribute to the adequacy of set theory with respect to its data, since the statement that they are consistent relative to **ZFC** will do that just as well. Making a case that the truth of large cardinal axioms is substantially more virtue-enhancing than their mere consistency relative to **ZFC** is the only option left for the platonist who takes Gödel's analogy seriously.

On the other hand, the argument above is unlikely to trouble platonists who don't share Gödel's view that theory confirmation within mathematics is analogous to theory confirmation in physics. You could of course be both a platonist *and* a maximalist of a less naturalistic persuasion; for instance, if you thought that the concept *set* mandated the adoption of any consistent maximising principle, large cardinal axioms included, the argument above would be of little consequence.[24] It's clear however, that such a position can make no room for a substantial analogy between mathematics and natural science, since in the natural sciences there is no sense in which the consistency of a theory amounts to its truth. In summary, the platonist who takes the analogy between mathematics and natural science seriously cannot maintain that large cardinal axioms function analogously to laws of nature which are necessary to account for the data.

It is worth noting that the argument offered here is of some relevance beyond the narrow confines of the large cardinals debate. The only plausible candidates for mathematical data are arithmetical sentences of at most Π^0_1 complexity that we have prior reason to believe are true. If I'm correct about this, then the completeness of **PA** with respect to Σ^0_1 arithmetical sentences places severe constraints on the regressive justification of any axioms which are stronger with respect to arithmetical sentences than the axioms of **PA** themselves. Therefore any regressive epistemology in the vicinity should have at most modest aspirations. The problem then, is that weaker axioms are more likely to be persuasive candidates for self-evidence, and therefore the significance of the regressive project as a whole is put into question by the arguments of this section.

To sum up: if indeed we should adopt any large large cardinal axioms, it will not be because they must be adopted to account for any of the data,

[24]Certain remarks of Gödel's do at times suggest that he is tempted by such a position. For example, footnote 23 in the 1964 version of the continuum paper cautiously suggests that the concept *set* dictates a maximality principle inconsistent with $V = L$. If this is correct, then mathematical intuition would verify a principle considerably stronger than previously supposed; this is because the existence of $\kappa(\omega)$ is consistent with $V = L$ (Jech 2003, p.304). The corresponding footnote in the 1947 version is number 22, which contains no such suggestion, possibly indicating a shift in Gödel's view over the intervening years.

since that data can be equally accounted for by much weaker consistency sentences. *Prima facie*, the ability of a theory to account for the relevant data in the sciences offers a strong reason for accepting it, but no such justification is available to large large cardinal axioms if we have a conception of the data similar to which such strong axiom candidates might plausibly be thought relevant. This not only weakens the case for adopting such axiom candidates, but also places a good deal of strain on the overall analogy that Gödel wishes to draw between mathematics and the natural sciences.

5 Theoretical Virtues

Things are not looking promising for the analogy between mathematics and natural science as a means of justifying the large cardinals programme in full generality. We've seen that, although a plausible delineation of the data is possible, there is no sense in which accounting for this data can give large large cardinal hypotheses strong regressive support. On the one hand, such cardinals themselves are not required for the prevention of a philosophical miracle, as material bodies plausibly are. This is because no particular cardinal plays the right explanatory role; all that is required is the truth of some existential generalisation of a certain consistency strength or greater. On the other hand, large large cardinal *axioms* do not receive strictly regressive support by accounting for the data in the relevant way, since demonstrably only their consistency is required for this.

There is another respect in which large large cardinal axioms can be quasi-scientifically successful, namely by enhancing the *theoretical virtues* of set theories to which they are added. This aspect of the analogy is by far the most often discussed in the literature, and appears to be the central justification for large large cardinal axioms, as far as several mathematicians and philosophers are concerned. The classic exposition of the view, unsurprisingly, comes from Gödel:

> Success here means fruitfulness in consequences, in particular in "verifiable" consequences, i.e., consequences demonstrable without the new axiom, whose proofs with the help of the new axiom, however, are considerably simpler and easier to discover, and make it possible to contract into one proof many different proofs... A much higher degree of verification than that, however, is conceivable. There might exist axioms so abundant in their verifiable consequences, shedding so much light on a whole field, and yielding such powerful methods for solving problems (and even solving them constructively, as far as that is possible)

> that, no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory (1964, p.261).

Although Gödel thought that the verification of large cardinal axioms by such means could only ever be probable, and that at the time of writing no proposition had been so verified, the core idea of this passage has been remarkably influential in the philosophy of mathematics. Quine (1990, pp.94–95), Maddy (1997, p.233), Koellner (2010, p.190) and others have all adopted the idea that a decision on the truth of at least some axiom candidates can be reached on the basis of analysing the extent to which these axioms enhance the *theoretical virtues* of **ZFC** when they are added to it.

As is the case with respect to the natural sciences, it isn't clear exactly what properties are to count as theoretical virtues, and there are difficult questions in the vicinity about how such virtues are to be weighted, and how virtues collectively should fare against other criteria for theory choice. Nonetheless, there are canonical examples of theoretical virtues in mathematics that should prove sufficient for our discussion. In the passage above, Gödel focuses on the speed-up or contraction of existing proofs, and the solution of open problems. Other virtues discussed include the 'naturalness' of an axiom candidate (Gödel 1938, p.27), the naturalness or expectedness of its deductive consequences (Moschovakis 1980, p.610), maximisation of interpretative power, and the 'effective completeness' of the supplemented theory (Koellner 2010, p.204).[25] Far too many virtues have been proposed in the literature to canvass here, however the most important examples will be explored in some detail. I'll examine the two central theoretical virtues mentioned by Gödel above, and also parsimony, the virtue which takes center-stage in discussions of scientific theories. I'll argue that with respect to these theoretical virtues, large large cardinal axioms should not expect a very positive evaluation.

5.1 Open Problems

The solution of open problems is a virtue of strong set theories that has received an enormous amount of attention, from Gödel onwards. The initial axiom candidate lauded with this virtue was $V = L$. Although the virtues of several large cardinal axioms inconsistent with this principle are now well-regarded in the literature, $V = L$ does indeed have the virtue of solving many

[25]This is perhaps a theoretical virtue that has no strong analogue in the natural sciences. A theory is said to be 'effectively complete' with respect to a given class of statements if it decides every statement in the class *except* the undecidable statements generated by Gödelian incompleteness.

open problems, both within set theory and without.[26] Within set theory, it solves GCH affirmatively, as proved by Gödel. Perhaps slightly less well-known is that $V = L$ implies that every Whitehead group is free, solving a famous open conjecture in algebra (Shelah 1974).

Nowadays, the focus is on the ability of large cardinal hypotheses to solve open problems in descriptive set theory. A proliferation of results exist using large cardinals to prove that sets of reals have various separability and measurability properties, and that particular games on sets of reals are determined. The most famous such result is probably Martin and Steel's proof (Martin and Steel 1989) of the projective determinacy axiom,[27] which follows from the existence of infinitely many Woodin cardinals. There are many other well-known examples of open problems solved by large cardinal hypotheses, and there is no need for my purposes to report them all. It is important to note that even where large cardinals have consequences for more concrete areas of mathematics, the decision of open problems can only be taken to enhance the theoretical virtue of set theory including such an axiom. We cannot take the extension of set theory by such an axiom as having strict regressive support on this basis, since the solved problems are viewed as being genuinely open in advance of positing the large cardinal axioms which facilitate their solution. As remarked above, the solution to open problems should be viewed as analogous to the making of a novel scientific discovery: the discovery is to be trusted only if the theory from which it follows is already believed to be sound.

This is not, of course, to say that the ability of an axiom candidate to provide solutions to open problems should not be considered highly virtuous. But there are a number of considerations which should make us regard this kind of justification with some caution. In the first instance, the strength of this kind of support is sensitive to whether the problem solved is one about which mathematicians have a strong prior view. For example, it may be that a large cardinal axiom which proved Goldbach's conjecture would be very virtuous indeed due to the widespread belief in the truth of that conjecture. But the solutions to open problems that we see in reality are by no means

[26] Large large cardinal axioms become inconsistent with $V = L$ quite rapidly. If the Erdős cardinal $\kappa(\omega_1)$ exists (a rather small large large cardinal), then so does $0^{\#}$, the set coding true statements about indiscernibles in L (Kanamori 2009, p.107). A theorem of Kunen shows that if $0^{\#}$ exists, then there is a non-trivial elementary embedding $j : L \to L$ (Kanamori 2009, p.XX). Another theorem of Kunen shows that there is no such embedding $j : V \to V$ in models of **ZFC** (Kanamori 2009, pp.318–319).

[27] This axiom, PD, states that in every two-player game of length ω with perfect information on a projective set of reals, one of the players has a winning strategy (i.e. the game is determined). See (Moschovakis 1980, ch.6) for the relevant details.

so persuasive.

For example, Maddy favourably cites the result that if there is a measurable cardinal, then there is no projective well-ordering of the reals (Maddy 1990, p.138). She cites Martin as expressing the view that this result, and others like it, are 'pleasing'. But hypotheses in science are not, to my knowledge, accepted on a regular basis for having pleasing consequences. Indeed what we find pleasing is highly contingent of the history on the discipline, not to mention personal taste: perhaps to some, the implication from $V = L$ that there is a relatively simple Δ^1_2 well-ordering of the reals would be pleasing. Indeed $V = L$ was described by Gödel as being a very natural principle at the time of his relative consistency proofs. Of course many open problems are such that the mathematical community is overall undecided with respect to their solution. The continuum hypothesis is a good example of such a problem. The collective ambivalence of mathematicians as regards it partly explains how it is possible that the most popular axiom candidates leave it open, where it is settled positively by the unpopular $V = L$.

Secondly, it is clear that the strength of support lent to an axiom by the solution of an open problem is related to the urgency within the mathematical community of solving the problem in question. And this matter is clearly relative to the interests of the community under consideration. As Potter highlights, (2004, p.221), the open problems solved by large cardinals are typically set-theoretic in nature, and not part of 'ordinary mathematics'. Examples such as $V = L$ solving the Whitehead conjecture are not easy to come by; in most cases, large cardinal axioms are typically used to solve problems and conjectures raised by set theorists themselves, rather than by practitioners in more mainstream areas of mathematics. If a large cardinal axiom could be used to solve a live conjecture posed by a number theorist, that should count as a greater theoretical virtue than the ability to solve a set-theoretical problem. As of yet, no example of such a conjecture has been found. The closest example of a genuinely mathematical open problem solved using large cardinals is that of Borel determinacy, proved by Martin (1970), under the assumption of a measurable cardinal. However, Martin subsequently proved the Borel determinacy axiom in unaugmented **ZFC** (1975), so the example should not inspire us with confidence that large cardinal axioms are useful in the solution of problems outside of set theory. So, for now at least, we should not in general place too great an emphasis on the solution of open problems as a theoretical virtue of large cardinal axioms.

Moreover, there is a much deeper problem for a platonist like Gödel with the idea that the solution of open problems can confirm a large cardinal hypothesis. The reason is that, as mentioned above, this kind of support is interest-relative, and hence the solution of open problems provides us only with interest-relative justification for those axioms. But for a platonist, such interest-relative justification cannot be considered justification proper, since the hierarchy is surely indifferent to the questions that interest us mathematically.

For example, it is quite possible that there be some mathematical community, exactly similar to ours except with respect to their interests, who place overwhelming value on the kinds of determinacy problems that appear in descriptive set theory. Suppose such people regard the solution of determinacy problems as the proper goal of all mathematical enquiry. To such a community, the full Axiom of Determinacy, AD, would have overwhelming theoretical virtue in respect of solving open problems.[28]

Actual mathematicians don't typically consider AD to be viable, since it contradicts the axiom of choice (Kanamori 2009, p.368). And if faced with such a community, we could certainly try to dissuade them by highlighting the merits of the axiom of choice, both intuitive and quasi-scientific. Indeed, for the platonist, this would be the only honest course of action. A pluralist might think that the imagined community have a perfectly good justification for studying set theory with AD, and that the actual community of today is quite right to ignore it. But for a Gödelian platonist, AD is simply a blatant falsehood. It is not *merely* that for the imagined community, AD has many virtues which (according to a choice-favouring platonist) ought to be outweighed by other considerations. It is rather that the imagined community has *misleading* interests, in that the pursuit of such interests is counter-productive to uncovering the truth about sets. So it is hard to see how a platonist could make sense of the idea that mathematicians in another community have *any* reason to believe that AD is true, *merely* in light of their mathematical interests. After all, their position is assumed to be epistemically similar to ours in all ways other than with respect to their interests.

Moreover, when we get past the axiom candidates which possess intuitive support, and consider large large cardinal axioms with *only* quasi-scientific justification, it is hard to know how we could verify whether or not our own interests are misleading in this way. So I think that the platonist in particular should not take the solution of open problems too seriously when it comes to justifying axiom candidates, especially given the narrowly focused achievements of large large cardinal axioms in this regard to date.

---

[28] This axiom is a generalisation of PD, and states that that every two-player game of length ω with perfect information on *any* set of reals is determined.

5.2 Speed-Up Results

One theoretical virtue, mentioned by Gödel above, is the ability of more powerful systems to speed up the proofs of theorems already provable by weaker theories. While this virtue is discussed much less often that either the solution of open problems (above) or parsimony (below), I have chosen to include a discussion of it because the ability of an axiom candidate to speed-up proofs can do much more than simply make a theory more virtuous. In the right circumstances, the effect that speed-up has on a system can close a genuine and pressing explanatory gap.

A classic presentation of the issues at stake can be found in (Boolos 1987). In that paper, Boolos presents an argument which is essentially a Sorites-paradox-style inference appended with a definition of a very fast-growing function. The number of steps of the shortest proof of this result in first-order logic is given by an exponential stack of 64 '2's, far greater than the number of particles in the universe. Yet the proof is not difficult in second-order logic; indeed Boolos provides this in a short appendix to the paper. Moreover, the reasoning is obviously valid, as can be seen from its appropriate arithmetical interpretation. The moral of the story is that, since we *should* be able to prove the conclusion of the argument, given that it obviously follows from the premises, the fact that we *can't* (in the relevant sense) give a first-order proof of it is evidence that second-order logic is logic. After all, second-order resources are required for a feasible proof, which we seem perfectly able to provide.

We might hope to find similar support for a large large cardinal axiom. For this, we would need to find a genuine mathematical example of an agreed-upon theorem, such that the formal proof is unfeasibly long without the axiom, but is completely feasible when the axiom is used. This would offer some powerful support for thinking that the axiom was true. If the informal justification for the theorem is obviously valid, this demands an explanation. In particular, if the formal proof of the theorem in our unsupplemented set theory is so long that it would take more than a human lifetime to complete, then the ability to follow the reasoning of that proof cannot explain our recognition of the theorem's validity. If a simple, feasible proof relies critically on a large cardinal assumption, then the large cardinal assumption gains a good degree of support from the fact that its truth is required to explain why a piece of reasoning which we all recognize to be valid and appear to be able to follow has these properties.

There are, however, several reasons to think that such an example will be extremely difficult to come across for our current purposes. In the first instance, given that we have an intuitive basis for believing in small large cardinals, according to Gödel, at least, the example would have to be one where a natural mathematical theorem from outside set theory was plainly valid, had an unfeasibly long proof without assuming the existence of a large large cardinal, and had a feasible proof *with* the assumption of such a large large cardinal. I certainly know of no example of a theorem meeting such specific constraints.[29]

Another limitation on the use of speed-up results to verify the existence of large large cardinals is that, according to the Gödelian conception of set theory being considered, we have *already* benefited from a huge amount of speed-up by using a second-order theory. Combining results from Gödel (1936) and Buss (1994), we get the following theorem:

> **Speed-Up Theorem:** For any function f, there are infinitely many formulae such that for any one of them, ϕ, $\mathbf{PA} \vdash \phi$ and $\mathbf{PA}_2 \vdash \phi$ and where n is the number of lines of the **PA** proof, and m is the number of lines of the $\mathbf{PA}_2$ proof, $n > f(m)$.

Hence the move to a second-order theory has already vastly increased our proof speed, at least with respect to arithmetical sentences. So, more specifically, verifying a large cardinal axiom θ via speed-up would require a formula ϕ which mathematicians regard as being informally valid, has an unfeasibly long proof in $\mathbf{ZFC}_2$ supplemented by any intuitively verifiable large cardinal axioms, and has a feasibly long proof in $\mathbf{ZFC}_2 + \theta$. This is a tall order indeed, but it is not impossible that such an example could be found. To my mind, finding such an example would offer the strongest quasi-scientific justification available for a large large cardinal axiom. Sadly, most of the available research on speed-up results relates to the order of the logical apparatus of a theory, rather than the large cardinal axioms it includes, so it is difficult to say anything conclusive on the subject of such axioms specifically.[30] For

[29] Tim Button has pointed out to me that in other circumstances, speed-up provided by a large large cardinal axiom could perhaps enhance the virtue of a theory even where the sped-up proof was not of a theorem the truth of which we were convinced of in advance. Namely, if we could show that the large large cardinal axiom was conservative over **ZFC** with respect to some class of statements, a proof of feasible length of some theorem T belonging to this class using the large cardinal axiom should convince us that T is true. If no feasible proof in **ZFC** of T can be found, then the speed-up of the proof of T could then be counted in the axiom's favour, as normal. This strikes me as correct, though I am pessimistic about the prospects of finding a concrete example of the phenomenon; especially since large large cardinal axioms tend to be radically non-conservative even over very simple classes of statements.

[30] In the more general area, Potter (2004, p.235) gives a very nice example of speed-up at work in enhancing the virtues of set-theoretic axioms: for large values of m, that the Goodstein sequence with m as its starting value terminates is provable in **PA**, but the

now, at least, we have good reason to believe that large large cardinal axioms are not substantially supported by the speed-up of proofs they provide, and that such support could be earned only in very exacting circumstances.

5.3 Parsimony

Another central theoretical virtue, more often discussed in connection with the sciences than with mathematics, is parsimony, or simplicity, of both ontology and ideology.[31] As I noted above, there is a difficult question about how to weight the virtues against each other, but I'll argue for a form of *pessimism* about the quasi-scientific justification of large large cardinal axioms, on the basis of parsimony considerations. To be clear, I'm not going to argue against the existence of such cardinals *tout court*; rather I'm going to argue that we have prior reason to believe that axioms positing them will score poorly on the front of theoretical virtues, as construed by the analogy with the natural sciences.[32]

The first ingredient in my argument is merely the observation that considerations of both ontological and ideological parsimony play an important role in the justification of theories in natural science. The two principles under consideration are:

1. **Ockham's Razor:** Entities are not to be multiplied beyond necessity.

2. **Kant's Razor:** Principles are not to be multiplied beyond necessity.

Both of these principles are no doubt familiar, and widely deployed within philosophy and elsewhere.[33] Similar principles have been endorsed by philosophers at least since Aristotle, but much more significantly they have been

proof is unfeasibly long. Since every Goodstein sequence terminates, it is obvious that the sequence which starts with m terminates. Replacement-free first-order set theory gives a feasible proof of Goodstein's theorem, and a proof of it for m by universal quantifier elimination. In this case, speed-up certainly supports the belief that certain first-order set theoretic axioms are true.

[31]Although it is not often discussed in connection with large large cardinals, the idea has been in circulation since at least (Quine 1951a, p.45).

[32]There is a distinct view, proposed by Maddy (1997), that mathematics has its own *autonomous* theoretical virtues, and that at least certain large large cardinal axioms score very well on this front. My argument will have nothing to say for or against such a view; here I am just focusing on the theoretical virtues that drop out of the analogy with the natural sciences.

[33]Of course, Kant didn't invent the principle that the non-ontological aspect of a theory should be a simple as possible. But then again, Ockham didn't invent the corresponding ontological principle. The formulation of Kant's razor here is taken from remarks at A652/B680 of the *Critique of Pure Reason* (1787, p.595).

strongly endorsed within the natural sciences themselves. Galileo, in his critique of the Ptolemaic system, deployed the principle that 'Nature does not multiply things unnecessarily; that she makes use of the easiest and simplest means for producing her effects; that she does nothing in vain, and the like' (Galileo 1632, p.397). A similar principle appears under the heading of 'Rule I' in Newton's *Principia* (1687, p.320). More recently, the sentiment was echoed by Einstein:

> [T]he grand aim of all science... is to cover the greatest possible number of empirical facts by logical deductions from the smallest possible number of hypotheses or axioms (Einstein, in (Nash 1963, p. 173)).

These examples are all taken from physicists, since physics is the science to which Gödel thought mathematics most analogous. There are examples to be found from across the range of the sciences, however.[34] While a full sociological or historical investigation is out of the question here, it is sufficient for my purposes merely that theoretical and ontological simplicity are important virtues in the natural sciences. Since that is a rather unremarkable claim, I'll proceed with the argument that large large cardinal axioms should automatically score poorly when evaluated with respect to parsimony (of both relevant kinds).

Firstly, the adoption of large cardinal axioms will substantially bloat the ontology of mathematics in a fairly straightforward way: such axioms tell us that there are more sets than were previously thought. Indeed, large large cardinal axioms often tell us that there will be *drastically* many more levels in the hierarchy than previously thought, since a relatively common feature of such axioms is that they imply the existence of an unbounded class of cardinals satisfying weaker large cardinal hypotheses.

On a straightforward reading of Ockham's principle, this observation is sufficient to show that large large cardinal axioms will score poorly on the front of ontological parsimony. Adding a large large cardinal axiom to **ZFC** involves massively bloating the size of the ontology of the theory, and these entities will, in a strict sense, have been multiplied 'beyond necessity'. After all, the arithmetical data accounted for by a large cardinal principle will equally be accounted for by a corresponding consistency sentence. In a more general sense, **ZFC**, or perhaps a tentative extension thereof, is already powerful enough to reproduce all of 'ordinary' (i.e. non-foundational) classical mathematics; so even if the multiplication of entities brought about

[34] Baker's article (2016) contains a veritable trove of such examples, from many sub-fields of both philosophy and science.

by a large cardinal axiom is in some way desirable, or virtuous, it is certainly beyond necessity.

An immediate objection would be that Ockham's razor, as a general principle, is not supposed to count against theories which posit *more* entities (all else being equal), rather it is supposed to count against theories which posit *more kinds* of entities (all else being equal).[35] An objector might claim then, that large large cardinal axioms do not at all imply the existence of new kinds of entities; rather they imply the existence of (many, many) more entities of the same kind, namely sets. It would not count against a theory in physics, the objector might say, if it entailed that there are more entities than previously supposed of a kind we already countenance, such as electrons. So why should positing more sets count against large cardinal hypotheses in set theory, since set theory is itself analogous to a natural science?

The problem with this suggestion is that Ockham's razor can be rendered *trivial* by permitting sufficiently wide kinds. There is clearly a good deal of slack in the notion of a kind of entity, at least for the purposes of considering parsimony principles, but the delineation of kinds for such purposes appears constrained, at least in practice. Violation of Ockham's razor played an important role in Lavoisier's critique of phlogiston theory, for instance (Baker 2016, §1). It would have been no defence to claim that phlogiston is of a kind we already accept, since it is a *physical substance* or similar. In scientific cases such as this, Ockham's razor is applied non-trivially, and so if the science–mathematics analogy is appropriate, as Gödel argues, some parallel restriction should also be in place when considering mathematics.

It is clear that *set* is too general a kind for the meaningful use of Ockham's razor as a principle of theory choice within mathematics. Indeed, the ontology of mathematics can be given exclusively in terms of sets (or perhaps sets with numbers as urelements, and classes as the values of higher-order variables). Hence the admissibility of *set* as a kind for the purposes of Ockham's razor would render that principle trivial within the domain of mathematics. That should not be an acceptable conclusion for the advocate of the science–mathematics analogy, since the application of the principle in the former domain is highly non-trivial.

I certainly don't want to claim that Nature Herself divides sets up into certain kinds, which are or are not subject to Ockham's razor. It is more plausible that the appropriate evaluative kinds should be based on *salience* for mathematical purposes, and hence sensitive to the investigative context. But sets come in many mathematically salient kinds. Some are ordinals, some

[35]This qualitative version of Ockham's Razor was famously championed by Lewis (1973, p.87).

are cardinals, some are arithmetical, some analytical, some Borel, some projective, and so on. The fact that they are all sets certainly does not mean that a large cardinal with hitherto uninstantiated properties does not constitute a new kind of entity. Indeed, the significant increase in the strength of set theory that is offered by large cardinal hypotheses render it all but certain that in any investigative context, the adoption of large large cardinal axioms will bloat the ontology of set theory beyond necessity, even if we envisage the evaluation as being about the number of kinds of entities in the ontology.

Similar points can be made about the increase in theoretical complexity incurred by the addition of large large cardinal axioms. As we've seen, such posits are not *necessary* to account for the data, since it is sufficient that they are consistent relative to **ZFC**. So the addition of complexity to the theory is not automatically legitimate. And similarly to the case of Ockham's razor, Kant's razor tells quite strongly against the addition of large large cardinal axioms to set theory. On a straightforward understanding of the virtue of ideological simplicity, **ZFC** will fare better than its extension by any large cardinal principle, since those extra principles go beyond what is necessary to account for the mathematical data.

However, it is not entirely clear when one theory is more ideologically complex than another. Instead of looking just at the number of axioms or schemata in a theory (as on the straightforward understanding of this virtue), Quine (1951, p.14) considers the 'ideology' of a theory to be the range of ideas expressible within the theory. This corresponds roughly to the kinds-based understanding of Ockham's razor, since on this understanding one theory can contain more principles than other without having a more bloated ideology, as long as no further 'ideas' are expressible in the more verbose theory. Quine's notion of ideology is a primarily linguistic matter, the formulation in terms of ideas being (hopefully) eliminable (Quine 1951, p.15). Nonetheless, there is an ambiguity here. Are we to understand parsimony as favouring overall less expressively powerful theories, or merely theories with a smaller number of primitive expressions?

If we understand the 'expressible ideas' of a theory in terms of its primitive vocabulary, large large cardinal axioms will score neutrally with respect to ideological parsimony. This is because the addition of a large cardinal principle to set theory leaves the undefined primitives of the theory (logical vocabulary, '$\in$', and possibly a symbol to distinguish sets from urelements) undisturbed.[36]

[36]If our set theory contains urelements *and* the empty set, the inclusion of a further non-logical primitive is required to distinguish sets from urelements. There are several means by which this can be achieved: a distinguished predicate for sets, a distinguished predicate for urelements, or a singular term for the empty set. This works straightforwardly

Things are very different, however, on the former disambiguation, according to which the extent of the ideology of a theory corresponds to its general expressive power. On this understanding, large large cardinal axioms will score poorly with respect to ideological simplicity. The addition of a large cardinal principle to set theory increases the range of definable sets lower down in the hierarchy, and correspondingly the theory will be able to express, and prove, many more 'ideas' about sets than its unsupplemented counterpart. The very reason that large cardinal axioms have Π^0_1 arithmetical consequences which are independent of **ZFC** is that more and more subsets of ω are definable under stronger and stronger large cardinal assumptions. If Quine's notion of an 'expressible idea' is cashed out in terms of propositions about definabile sets, then large cardinal axioms will be largely uneconomical. It seems likely to me that this disambiguation corresponds closely to Quine's intentions, since he claims that the classical theory of the reals has a denumerable ideology, and claims that investigation of primitive ideology is a 'subdivision' of the overall ideological investigation (1951a, p.14). Both comments would be misleading if his intention had been to refer to the *finite* number of analytical primitives, and considered the investigation of primitive ideology to be an improper subdivision of overall ideological investigation!

Given the lack of clarity in Quine's suggestion, perhaps a broader notion of an ideologically parsimonious theory is required. However, on any reasonable construal of a theory's ideology (other than as consisting of its primitive vocabulary), large cardinals will bloat it: more properties of sets will be instantiated, new embeddings between the universe and transitive classes appear, new sets are definable, and so on. Given that large large cardinals can be used to solve set-theoretic problems not solvable in **ZFC** alone, a large large cardinal axiom will *always* give a richer picture of the hierarchy than is strictly required to explain the data, since the data is accounted for by only positing the consistency of large cardinal axioms relative to **ZFC**, without the need for any further increase in the power of the theory.

However exactly you construe the notions of ontology and ideology, a very basic problem for the large cardinal advocate appears: large cardinals axioms posit new sets (bloating its ontology), with new and remarkable properties (bloating its ideology). So, large large cardinal axioms are always purchased at the expense of two theoretical virtues which have historically played a central role in scientific theory choice. A conception of set theory which places overwhelming value on maximal ontology and richness of the structure of the

---

in the first two cases. In the third case, one can formally define the predicates S ('is a set') and U ('is an urelement') stipulating that $\forall x(Sx \leftrightarrow \exists y\ y \in x \vee x = \emptyset)$ and $\forall x(Ux \leftrightarrow \neg\exists y\ y \in x \wedge x \neq \emptyset)$.

hierarchy needn't be concerned by such issues at all. But on such a picture, the theoretical virtues of set theory become more distant from those of natural science.

I don't want to claim that there is *no* analogy between set theory and natural science; any things are analogous if you squint hard enough. But nor do I want to claim that set theory and natural science are strongly analogous. Rather, I'm claiming that the more analogous you think the two disciplines are, the more difficult the justification of large large cardinal axioms becomes. And *that* point spells doom for Gödel's attempt to find the justification for large large cardinal axioms in any kind of an analogy between set theory and natural science.

It's important to note, however, that parsimony considerations come with a *ceteris paribus* clause: the bloating of ontology and ideology only tells against a theory if the other virtues of the theory don't compensate. I expect that it is *possible* that the virtues of large cardinal axioms could be so overwhelming as to compensate for a bad score in both kinds of parsimony. An example of the right kind of speed-up result discussed earlier would be a possible example of this. But in actuality there is room for much scepticism of the theoretical virtues which are commonly ascribed to large cardinal axioms. Most importantly, the solution of open problems is not to be valued for its own sake when the solution provided for by an axiom does not itself enjoy extensive support from elsewhere.

I think it's worth distinguishing the argument offered here from that presented by Quine (1990, pp.94–95). The argument there is somewhat ambiguous. On the one hand, Quine suggests that 'higher' set theory is *meaningless*, because, whatever the axioms that constitute higher set theory are supposed to be, they never have any implications for natural science. They are treated by us as meaningful only because to do otherwise would constitute an 'unnatural gerrymandering of grammar'. Another argument offered, however, is that the questions of higher set theory are, at least in part, settled by parsimony considerations. In particular, Quine argues that considerations of simplicity, economy, and naturalness compel us to adopt $V = L$ as a new axiom, since it 'inactivates the more gratuitous flights of higher set theory'. I take it that this is a reference to the inconsistency of $V = L$ with most large large cardinal axioms, though Quine is not specific. I'm unsure how to reconcile these two arguments, since a decision of the kind Quine envisages seemingly involves regarding the relevant axiom candidates as meaningful. After all, it is hard to see how one uninterpreted string of symbols can give a more natural or economical picture of the hierarchy than another. In any case, Quine claims that considerations of theoretical virtue tell decisively against large large cardinal axioms.

My argument, on the contrary, involves no such claim, and approaches the problem from an entirely different perspective. In the first instance, Quine subjects set theory to evaluation in terms of the theoretical virtues of natural science and with respect to scientific applications, since presumably his holistic naturalism implies that this is the only appropriate set of virtues to figure in *any* theory choice, regardless of the subject matter. Gödel's analogy, on the other hand, requires only that the means of theory choice be analogous between mathematics and science, and does not require that the theoretical virtues of a putative axiom of set theory be considered in relation to its application in natural science. Since I'm here in the business of assessing Gödel's position, my argument does not consider the virtues of set theory as they relate to scientific applications.

Secondly, and more significantly, I have not attempted to offer an argument that $V = L$ is true, nor have I even offered an argument that we shouldn't accept large large cardinal axioms in general. Rather, I have argued for the much weaker claim that, however such axioms are justified (if at all), it does not look much like how theoretical posits are justified in natural science. The argument presented here therefore should certainly not be confused with the one offered by Quine, despite the central role played by considerations of economy in each.

Conclusion

I've argued that none of the analogies that Gödel saw between mathematics and the natural sciences can serve as an adequate justification for large large cardinal axioms. Three attempts were offered to provide these axioms with a viable quasi-scientific justification, inspired by remarks made by Gödel. None of them proved to be successful. Although this is not to say that such arguments cannot justify the existence of *any* sets, such an account would be redundant for the Gödelian platonist who thinks that the weaker axioms follow directly from the iterative conception.

Firstly, we saw that large large cardinal axioms cannot have roughly the status of propositions asserting the existence of ordinary material bodies. This would afford us an enormous degree of confidence in the existence of larger cardinals, but the account is not viable. In particular, the large cardinals cannot play the same kind of explanatory role that posited material bodies do.

More promisingly, we investigated the idea that large cardinal axioms could play the role within mathematics played by laws of nature in science, as pioneered by Russell. The statements of scientific laws are strongly supported because they allow us to predict the initial data, regardless of the degree of intuitive appeal such principles may have. I argued, however, that large cardinal axioms cannot enjoy this same kind of regressive support.

Various candidates for the mathematical data were considered. The only viable conception of the data on offer is that propositions acting as data are expressed by Π^0_1 arithmetical sentences and are either hard data in Russell's sense, are generated by the Gödelian incompleteness of a theory we believe to be sound, or perhaps are such that a strong heuristic justification can be offered for their truth, as with Goldbach's conjecture.

The problem for the Gödelian is that accounting for such data affords justification only to the propositions that such large cardinal axioms are consistent relative to **ZFC**, and not to the truth of the axioms themselves. The trouble is not that we can't prove whether such axioms are consistent; the independence results that proliferate in modern set theory should teach us to be less ambitious than that. Rather it is that the consistency of a large cardinal axiom is a strictly weaker proposition than its truth, and is alone sufficient to account for any data in the relevant sense.

Quite aside from considerations of data, I've argued further that the justification of large large cardinal axioms does not look much like the justification of virtue-enhancing principles in science, since adding a large cardinal axiom to a theory always causes significant bloating to the ontology and ideology of a theory, a practice which is anathema to the modes of theory choice in natural science where simplicity and parsimony are highly respected arbitrators between competing empirically equivalent theories.

The problem here can be put in quite simple terms: between empirically equivalent theories, the mode of theory choice in natural science is minimising and conservative with respect to ontology and ideology. Since large large cardinal axioms are *maximising* with respect to ontology and ideology, it follows that either the modes of theory choice in mathematics are not much like their scientific counterparts, or that large large cardinal axioms fail to be justified. Unlike Quine, I don't wish to take a side on this matter; the disjunction is sufficient to make my point, which is that this well-received aspect of Gödel's thought is ultimately not fit for purpose as regards large large cardinal axioms.

Of course, I've not discussed a large number of other attempts to justify the large cardinals programme. But the need to find a compelling justification for it is of some urgency. The general programme of formulating large cardinal axioms and investigating the consequences of their assumption is one of the central research areas in the foundations of mathematics. It would be a philosophical scandal if we could say no more than that this programme involved its practitioners in mere 'if-then-ist' thumb-twiddling. The mathematical significance of the large cardinals enterprise demands philosophical explanation, preferably one which justifies a contentful interpretation of the consistency-constrained maximalism at work in current set-theoretic practice. Unfortunately, this explanation cannot be given by means of Gödel's analogy between mathematics and the sciences.[37]

Bibliography

Baker, A. (2007). "Is there a problem of induction for mathematics". In: Leng, M., A. Paseau, and M. Potter (2007), pp. 59–73.

— (2016). "Simplicity". In: Zalta, E. (ed.) (2016) *The Stanford Encyclopedia of Philosophy* (Winter 2016 Edition). URL: `https://plato.stanford.edu/archives/win2016/entries/simplicity/` (visited on 06/08/2018).

Boolos, G. (1987). "A Curious Inference". In: *Journal of Philosophical Logic* 16, 1–12.

Bueno, O. and Ø. Linnebo, eds. (2009). *New Waves in the Philosophy of Mathematics*. Basingstoke: Palgrave Macmillan.

Buss, S. (1994). "On Gödel's Theorems on Lengths of Proofs I". In: *The Journal of Symbolic Logic* 59, 737–756.

Chihara, C. (1982). "A Gödelian Thesis Regarding Mathematical Objects: Do They Exist? and Can We Percieve Them?" In: *The Philosophical Review* 91, 211–227.

Echeverria, J. (1996). "Empirical methods in mathematics: A case study: Goldbach's Conjecture". In: Munévar, G. (1996), pp. 19–56.

Feferman, S., J. Dawson, W. Goldfarb, C. Parsons and R. Solovay, eds. (1995). *Kurt Gödel: Collected Works, Volume III.* Oxford University Press.

[37]I am very grateful to Michael Potter for invaluable feedback on several versions of this paper, and to Tim Button, Owen Griffiths, Daniel Isaacson, Alex Oliver, and Robert Trueman for extensive comments on subsequent drafts which have greatly improved it. This research was generously funded by an Arts and Humanities Research Council Studentship.

Feferman, S., J. Dawson, S. Kleene, G. Moore, R. Solovay and J. van Heiejenoort, eds. (1986). *Kurt Gödel: Collected Works, Volume I.* Oxford University Press.

— eds. (1990). *Kurt Gödel: Collected Works, Volume II.* Oxford University Press.

Feferman, S., C. Parsons and S. Simpson, eds. (2010). *Kurt Gödel: Essays for his Centennial.* Oxford University Press.

Frege, G. (1884). *The Foundations of Arithmetic,* trans. Austin, J. (2nd ed.) (1959). Evanston, IL: Northwestern University Press.

Galileo, G. (1632). *Dialogue Concening the Two Chief World Systems,* trans. Drake, S. (1962). Berkeley, CA: University of California Press.

Gödel, K. (1936). "On the lengths of proofs". In: Feferman, S. et al. (1986), pp. 397–398.

— (1938). "The consistency of the axiom of choice and of the generalized continuum hypothesis". In: Feferman, S. et al. (1990), pp. 26–27.

— (1944). "Russell's mathematical logic". In: Feferman, S. et al. (1990), pp. 176–187.

— (1951). "Some basic theorems on the foundations of mathematics and their implications". In: Feferman, S. et al. (1995), pp. 304–323.

— (1964). "What is Cantor's continuum problem?" In: Feferman, S. et al. (1990), pp. 254–270.

Jech, T. (2003). *Set Theory: The Third Millenium Edition.* Belin: Springer-Verlag.

Kanamori, A. (2009). *The Higher Infinite* (2nd ed.) Berlin: Springer-Verlag.

Kant, I. (1787). *Critique of Pure Reason* (2nd ed.) Trans. Guyer, P. and A. Wood (1998). Cambridge University Press.

Koellner, P. (2009). "On Reflection Principles". In: *Annals of Pure and Applied Logic* 157, 206–219.

Koellner, P. (2009a). “Truth in Mathematics: The Question of Pluralism”. In: Bueno, O. and Ø. Linnebo (2009), pp. 80–116.

— (2010). “Absolute Undecidability”. In: Feferman, S. et al. (2010), pp. 189–225.

Leng, M., A. Paseau and M. Potter, eds. (2007). *Mathematical Knowledge*. Oxford University Press.

Lewis, D. (1973). *Counterfactuals*. Oxford: Basil Blackwell.

Maddy, P. (1990). *Realism in Mathematics*. Oxford: Clarendon Press.

— (1997). *Naturalism in Mathematics*. Oxford: Clarendon Press.

Martin, D. (1970). “Measurable Cardinals and Analytic Games”. In: *Fundamenta Mathematicae* 66, 287–291.

— (1975). “Borel Determinacy”. In: *Annals of Mathematics* 102, 363–371.

Martin, D. and J. Steel (1989). “A Proof of Projective Determinacy”. In: *Journal of the American Mathematical Society* 2, 71–125.

Moore, G., ed. (2014). *The Collected Papers of Bertrand Russell, Volume 5*. Oxford University Press.

Moschovakis, Y. (1980). *Descriptive Set Theory*. Amsterdam: North Holland.

Munévar, G., ed. (1996). *Spanish Studies in the Philosophy of Science*. Dordrecht: Springer.

Nash, L. (1963). *The Nature of the Natural Sciences*. Boston, MA: Brown, Little.

Newton, I. (1687). *The Principia: Mathematical Principles of Natural Philosophy*. Snowball Publishing.

Potter, M. (2004). *Set Theory and its Philosophy*. Oxford University Press.

Putnam, H. (1975). “What is Mathematical Truth?” In: Putnam, H. (1979), pp. 60–78.

Putnam, H. (1979). *Philosophical Papers Volume 1* (2nd ed.) Cambridge University Press.

Quine, W. (1951). "Ontology and Ideology". In: *Philosophical Studies* 2, 11–15.

— (1980). *From a Logical Point of View* (2nd ed.) Cambridge, MA: Harvard University Press.

— (1990). *Pursuit of Truth*. Cambridge, MA: Harvard University Press.

— (1951a). "Two Dogmas of Empiricism". In: Quine, W. (1980), pp. 20–46.

Russell, B. (1907). "The Regressive Method of Discovering the Premises of Mathematics". In: Moore, G. (2014), pp. 571–580.

— (1908). "Mathematical Logic as based on the Theory of Types". In: *American Journal of Mathematics* 30, 222–262.

— (1912). *The Problems of Philosophy*. Oxford University Press.

— (1914). *Our Knowledge of the External World*. At: Project Gutenberg. URL: `http://www.gutenberg.org/files/37090/37090-h/37090-h.htm` (visited on 07/09/2018).

Shelah, S. (1974). "Infinite Abelian Groups, Whitehead Problem, and Some Constructions". In: *Israel Journal of Mathematics* 18, 243–256.

Sklar, L. (1992). *Philosophy of Physics*. Boulder, CO: Westview Press.

Tait, W. (1981). "Finitism". In: *The Journal of Philosophy* 78, 524–546.