Mathematical Truth without Reference

By
Colin McCullough-Benner

Submitted to
Central European University
Department of Philosophy

In partial fulfilment of the requirements for the degree of Master of Arts

Supervisor: Hanoch Ben-Yami

Budapest, Hungary
2014

Abstract

A near-universal assumption in the philosophy of mathematics is that mathematical language, taken at face value, at least purports to refer to mathematical objects. However, this assumption, I argue, is at the heart of some of the most difficult problems in the field—not just for platonist views, but for nominalist views as well. In this thesis, I argue that we should not make this assumption and propose an alternative, non-referential account of mathematical language. According to the view that I favor, mathematical truth is to be identified with provability in a system, and mathematical language is contentful in a way that a mere game is not insofar as the concepts that it employs are used outside of mathematics (or are in some sense parasitic on extra-mathematical concepts). Not only does this account avoid the traditional problems with existing non-referential accounts, but, I argue, it also allows us to deal with more sophisticated arguments concerning phenomena related to Gödel's incompleteness theorems and the semantics of sentences in which mathematics is applied.

Table of Contents

1. The Referential Picture of Mathematical Language

1.1. The referential picture: Pride and prejudice

Stop almost any philosopher of mathematics on the street, and they will tell you: It is a truth universally acknowledged that a mathematical term should refer—at least if we take it at face value. Mathematical terms purport to refer to mathematical objects, and mathematical statements purport to describe the relations between such objects.

Interestingly, this phenomenon cuts across the full range of ontological stances toward mathematics, from the most robust realism to the barest nominalism. We would expect such a conception of the face-value semantics of mathematics to best complement various forms of realism, and indeed, realists often cite an ability to take mathematical language at face value as one of the merits of their accounts. However, many of the most influential nominalist accounts involve strategies to paraphrase mathematical language in order to dissolve mathematical language's putative commitment to the existence of abstract mathematical objects. Mathematical fictionalists, for instance, take face-value mathematical statements to be like face-value statements about fictional characters and worlds: any statements apparently committed to the existence of mathematical objects (particularly sentences in which an existential quantifier is dominant) are false, and statements which express the negation of the former kind of statement (as well as bare, universally quantified sentences) true, simply because such objects do not exist. In order to make mathematical discourse comprehensible, the fictionalist provides a way to paraphrase such statements: we introduce a fictional operator of the form "according to X," where X is some mathematical theory. So, while the statement that, say, there are at least three prime numbers is literally false (since there are no numbers at all), this is more charitably interpreted as the true statement that, according to arithmetic, there are at least three prime numbers. This and similar strategies will be considered in greater detail later in this chapter.

Why should this be the case? Well, such a picture has quite a lot of initial, intuitive plausibility.

In mathematics, we use singular terms which seem to refer to mathematical objects like numbers, sets, and so on. We use variables that appear to range over such objects, and we seem to quantify over domains of such objects. "$1 + 2 = 3$" refers to the numbers one, two and three by means of the singular terms "1," "2," and "3" and ascribes to them a particular relation: the sum of the first and the second equals the third. In "$\exists n(n < 3)$," n ranges over the natural numbers, and the statement says that there exists some natural number less than three. If we understand the use of singular terms and quantification in mathematics as at least analogous to their uses in (large expanses) of our ordinary, non-mathematical discourse, then the referential picture seems to follow naturally.

Moreover, even if we did not want to treat, say, arithmetic as purportedly referring to the natural numbers, surely the language of metamathematics must refer at the bare minimum to symbols, strings of symbols, proofs, and other constructions of the system whose properties it is meant to explore. But once we treat metamathematical language as referential, why should we expect the rest of mathematics to be devoid of referential language? After all, in both cases one ultimately proceeds by constructing proofs; the only difference seems to be that ordinary mathematical proofs use mathematical terms while metamathematical proofs use metamathematical terms.

Accompanying this conception of mathematical language is a conception of mathematical truth. This is something along the lines of truth by correspondence. A mathematical statement is true if and only if the mathematical objects to which its terms refer have the properties—including, of course, relational properties—which it ascribes to them.

Of course, there are variations on this theme. A supporter of a standard variety of structuralism might hold that all such properties are relational, as mathematical objects are just places within structures. But this still is essentially a correspondence theory, and I will at times gloss over such nuances.

This conception of truth in mathematics even seems to be supported by mathematics itself. When we want to formalize the semantics of mathematics, we turn to model theory, which, even though it only embeds our base theory in an additional, set theoretical apparatus, still models something like truth-by-correspondence. To put it roughly, in model theory, a model includes a domain of individuals, an assignment of such individuals to non-logical constants of a language, an assignment of extensions to predicates of that language, and so on, and this can be used to evaluate the truth (in that model) of sentences of the language which the model interprets. And so, for example, the sentence $\exists x(F(x) \,\&\, R(x, a))$ is true in a given model just in case there is some individual in the domain which is in the extension of $F(x)$ and such that the ordered pair of that individual and the individual assigned to a is in the extension of $R(x, y)$. The picture that emerges is of terms in the language referring to objects in the domain provided by the model and of the truth of sentences of the language being determined by the referents of those terms having the appropriate properties or standing in the appropriate relations.

The referential conception also has the apparent benefit of easy compatibility with the semantics of the rest of our language. If we accept the referential conception of mathematical language and truth, our semantics should be simpler, as we can treat singular terms, quantifiers and the like in the same way as we treat them in ordinary, non-mathematical language. Moreover, truth in mathematics, as a sort of truth by correspondence, would be easily covered by a modest extension of our account of truth in other domains. This also seems to make it easier to understand the semantics of "mixed sentences," sentences which use mathematical language to describe the (non-mathematical) world (and so include terms referring to mathematical objects as well as terms referring to physical objects).

To argue against such a widely accepted and intuitively plausible picture of the workings of mathematical language is no small task, but—alas—it will be the project of this thesis. I suspect that this referential conception of mathematical language lies behind a number of difficulties facing both realist and nominalist accounts in the philosophy of mathematics. And, ultimately, I suspect that this conception of mathematical language rests upon a flawed picture of how language works more generally. In this thesis, I will argue against this general picture of language and the conception of mathematical language which rests upon it. I will then propose what I think is a more satisfying non-referential account of mathematical language and defend it against some standard objections to such accounts and some objections particular to my own account.

In the rest of this chapter, I provide some of the groundwork for this argument. First, I trace out some of the reasons that might motivate the rejection of the referential picture of mathematical language: problems with both realist and nominalist views which are conditioned, I argue, on the acceptance of such a picture (§1.2). Second, I argue directly against the view that, taken at face value, mathematical language is referential and thereby commits us to the existence of mathematical objects (§1.3).

In the second chapter, I present an alternative, non-referential account of mathematical language. I begin by considering existing non-referential accounts of mathematical language with an eye to both their strengths and their weaknesses and consider three canonical problems: the problem of the application of mathematics, the problem of the metatheory, and the problem of systems' being on a par (§2.1). I then propose my alternative account. A mathematical sentence is true (in a system) iff it has a proof (formal or ordinary) in that system and false iff it has a refutation in that system. Insofar as mathematics is contentful, its concepts are related to extra-mathematical concepts, and mathematics can serve to define and regulate our use of those concepts. Otherwise, it is only contentful in the sense that a chess position is contentful in the context of a game of chess (§2.2). I finish by returning to the three canonical problems I set out at early in the chapter (§2.3).

In the third chapter, I turn to some objections to the sort of I account that I propose in Chapter 2. In particular, I consider objections on the basis of Gödel's incompleteness theorems and how they are dealt with when mathematicians extend formal systems (§3.1) and objections related to the application of mathematics and the semantics of mixed sentences (§3.2), arguing that both sorts of objection can be met.

But before we get there, we should turn back to referential approaches to mathematics and the problems that they face.

1.2. Motivation for rejecting the referential picture

Before I address the referential picture directly, I should provide some independent motivation for my critical stance toward it. In this section, I will consider some very significant problems facing the most prominent accounts associated with the referential picture of mathematical language and argue that they can be traced back to this particular conception of language.

One salient way to divide the philosophy of mathematics is into the competing camps of platonism and nominalism. On a very broad construal, platonism is the position that the subject matter of mathematics is mathematical objects, which are characterized as abstract (existing outside of space and time) and therefore acausal. On the other side of the divide are nominalists, anti-platonists of various stripes. At the very least, they must hold that abstract objects are not the subject matter of mathematics. But there is a great deal of space for a number of different positions. Nominalists may treat mathematical objects as physical objects (e.g., Maddy 1980[1]), treat mathematical language as a useful fiction (Field 1980; 1989) or as hypothetical talk about what such objects or structures would be like if they existed (e.g., Friedman 2005, Hellman 1989, Hellman 1996), or treat mathematics as an empty formal game (e.g., Curry 1951).

Structuralism is a position that does not fit nicely into this dichotomy. Structuralists hold that the subject matter of mathematics is mathematical structures, which are taken to be primitive. Mathematical objects are essentially just positions in these structures. For example, the number four is just the fourth (or fifth) place in the natural number structure. While the majority of structuralists treat structures as abstract objects (or "*ante rem*" structures) and therefore fall into the platonist camp (e.g., Shapiro 1997), modal structuralists treat mathematics as making hypothetical or counterfactual claims about what certain structures would be like if they existed, thereby falling into the nominalist camp (e.g., Hellman 1989, Hellman 1996).

With these preliminaries out of the way, I turn first to the relation between some of platonism's deepest problems and the referential picture. I will then do the same with certain, prominent varieties of nominalism, namely fictionalism and modal structuralism.

1.2.1. The problem with platonism

The canonical objection to platonism in its various guises is the access problem. It was most famously put forth by Paul Benacerraf (1973) in the form of what is now called Benacerraf's dilemma: if we have an account of mathematical truth which is at least parallel with the semantics of non-mathematical language, then this commits us to the existence of abstract objects. This is inconsistent, however, with an adequate epistemological account of

[1] Though Maddy considers her view a kind of platonism.

mathematics (which Benacerraf thinks must be consistent with a causal theory of knowledge). Platonism achieves semantic adequacy at the expense of epistemic adequacy.[2] Of course, the platonist may simply respond by rejecting causal theories of knowledge, citing the many problems unrelated to mathematics plaguing such theories and the fact that they are generally dismissed in contemporary epistemology. No causal theory, no problem, one might say.

However, the platonist should not be quite so sanguine. Benacerraf's dilemma generalizes: surely, if the subject matter of mathematics is mathematical objects, then we should rightly expect those objects to play some role in our coming to know mathematical truths. If such a role is not causal, then the platonist owes us a story (and a well-motivated one at that) about what such a role would be. This is exacerbated by the fact that the way we typically gain mathematical knowledge is by means of proofs, which can be explained without reference to any sort of mathematical object.

Note also that this is not just an epistemic problem. It is also a problem with the motivation of the platonist's ontology and even a linguistic problem. For if mathematical objects have no clear role to play in the practice of working mathematicians, then this leaves us at a loss to see what motivates claims that such a special class of objects exists and how, in such a practice, we manage to refer to them.[3]

We may follow Gödel (see, e.g., Gödel 1947, pp. 267-269) and posit a sense of intuition, which allows us access to mathematical objects. Then intuition may justify certain statements of mathematics as axioms, or a certain set of axioms may be justified because it

[2] Of course, Benacerraf meant for his dilemma to be a problem not just for the platonist but for the nominalist as well. After all, if semantic adequacy entails the existence of abstract objects, then the nominalist achieves epistemic adequacy at the expense of semantic adequacy. However, I will be arguing that an adequate semantic account of mathematical language does not require invoking abstract objects.
[3] For an exposition of the problem of reference, see Hodes 1984.

allows us to prove theorems which we know by intuition. Intuition therefore has a place in mathematical practice. However, the introduction of intuition appears to have no independent motivation, and it is a very strange claim, to say the least.

However, others have provided more subtle answers to these questions. Here I will focus on the account proposed by Stewart Shapiro. Shapiro is a structuralist and holds that mathematics studies structures which exist independently of their instantiations (*ante rem* structures). He proposes that there are several means by which we can both successfully refer to structures and have epistemic access to them. For very simple mathematical structures, simple abstraction or pattern recognition will suffice. We recognize that certain objects instantiate a pattern and describe the properties of the pattern by means of its instances. (This is what some call *perceptual* intuition, which is to be distinguished from the Gödelian sort of intuition considered above.) This is meant to show how we can unproblematically refer to and describe abstract structures on the basis of their instantiations, but this cannot cover the greater part of mathematics. Anything more complicated can be achieved by linguistic means. First, we have linguistic abstraction. For instance, we may notice that the patterns of small natural numbers form a pattern themselves: |, ||, |||, … And we recognize that such a pattern can be extended indefinitely. We build this into a linguistic system whose rules mirror, as it were, the structural features of the whole natural number structure. Further than this, we can reach as high as we want into the set-theoretical hierarchy by means of a set of axioms which implicitly defines a structure—and again, these mirror in a way the structural features of the structure in question. In such cases the means by which we refer to a particular structure is also the means by which we describe it. (Shapiro 1997)[4]

[4] For a similar account, see Resnik 1997.

This is certainly more plausible than Gödelian intuition, but does it do the work that it is supposed to do? The short answer is: only with an additional, controversial assumption. We must assume that abstract structures must underlie perceptible patterns and linguistic rules and that such patterns and rules cannot be explained without invoking such structures. Only then does Shapiro provide an epistemological account of how we come to grasp truths about abstract structures; otherwise, all of the phenomena that he describes are consistent with there being no abstract structures at all. This is a bullet that the platonist or structuralist may bite,[5] but this assumption does not follow from our ordinary use of language, and it need not be imposed on rival accounts in the philosophy of mathematics. So the question is why, given that these linguistic tools do the job, we need to additionally invoke abstract structures lying behind them.

In both the standard platonist case and in the *ante rem* structuralist case, I think the problem ultimately boils down to having a certain conception of how mathematical language works and what an adequate semantic account of mathematical language in general and mathematical truth in particular ought to look like. This arises even in Benacerraf's own initial formulation of his dilemma. The platonist's mathematical objects are there to give mathematical language a referent and to be the ultimate, objective arbiter of mathematical truth, but since such objects cannot be identified with ordinary objects, their being posited results in the epistemic, ontological and even linguistic problems sketched above. Even the structuralist, who has subtle, nuanced answers to such problems ultimately presents them in terms of phenomena that need not involve abstract objects. Abstract objects need not enter

[5] See Brown 1999 for a platonist account which bites this bullet. See, for example, chapter 9, in which he proposes that the answer to "rule-following skepticism," which Kripke attributes to Wittgenstein, is platonism about rules. Rules as abstract objects determine what is meant by "going on in the same way." But this should look strange, not to mention *ad hoc*, not just to the nominalist, but even to a friend of abstract objects.

the picture unless we assume that linguistic rules and perceptible patterns cannot exist as such without abstract structures existing as well, and this assumption is certainly less plausible than the initial assumption that mathematical language is meant to work referentially.

I will not pretend that these considerations are a knock-down argument against platonism, but they do give us independent reason to question the referential conception of mathematical language. In the next section, I will argue that the referential conception leads to a different sort of problem for those nominalists who accept it.

1.2.2. The problem with nominalist paraphrases

Nominalists have their own set of problems. Here I will focus on those who accept the referential conception as a good picture of the face-value semantics of mathematical language but provide paraphrases in order to make sense of mathematics in the absence of abstract objects. To this end, I will consider two of the most prominent nominalist accounts: mathematical fictionalism and modal structuralism. While many of the same problems will arise in each case, for ease of exposition I will consider each in turn.

In proposing his mathematical fictionalism, Hartry Field is attempting to derail what he takes to be the strongest argument for platonism, Quine's indispensability argument (Quine 1960). According to Quine, our ontology should consist of those things which are indispensable to our best scientific theories. Since mathematics is indispensable to our best scientific theories, mathematical objects should be included in our ontology. Field's response is that mathematical theories as sets of true statements are not indispensable to science. The aim of mathematics is not truth as such, but *conservativeness,* consistency with any nominalistically interpreted, internally consistent physical theory. In something of an echo of David Hilbert, who thought that the role of infinitistic mathematics was to allow simpler proofs of finitistic theorems, Field thinks that the role of non-nominalized mathematics is to permit something like shorter derivations of consequences of physical laws, which can be derived—albeit less simply—by nominalistically acceptable means. (Field 1980)

According to Field, assuming that mathematical objects do not exist, existential claims like “there are at least three prime numbers” are false and universal claims like “all numbers are greater than or equal to seventeen” are vacuously true, since there are no numbers. Because this is unintuitive—after all, 2 is prime, 3 is prime and 5 is prime, and sixteen is less than seventeen—he provides paraphrases which allow agreement in truth value with mathematical practice. For instance, the sentence “according to arithmetic, there are at least three prime numbers” is true, and the sentence “according to arithmetic, all numbers are greater than or equal to seventeen” is false. So, on Field’s account, if our semantics for mathematics is continuous with our semantics for the rest of our language, a significant revision of the truth values of mathematical sentences is in order, but we can reach agreement in truth value by providing such nominalist paraphrases. (Field 1989)

This is the first major problem with mathematical fictionalism. If it takes mathematical language literally (at least as the literal meaning is understood according to the referential conception), then it requires a massive revision of the truth values of mathematical statements, which makes the practice of mathematics incomprehensible. If we paraphrase mathematical language with a fictional operator in order to avoid this consequence, then we still end up revising mathematical language and still cannot capture what mathematicians really mean when they use this language. For, surely, if a mathematician says that there are infinitely many prime numbers, this does not *mean* “according to number theory, there are infinitely many prime numbers,” even if mathematical truth is theory-relative. As a result, it hardly looks like the fictionalist can make sense of mathematics as such, and the dilemma that brought this on is a result of accepting the referential conception but denying that any such reference is successful.

But this is not where the problems end. Field's take on the literal meaning of mathematical language and his project of showing that only a suitably nominalized (i.e. revised) version of mathematics is indispensable to science ultimately lead him into further problems. Field must use certain results in metalogic in order to formulate a nominalist version of conservativeness as well as to prove that set theory (and hence all mathematics that can be modeled in set theory) has this property (see Field 1991, Field 1992). But these results are spelled out in set-theoretical terms, and Field's other metatheoretical resources are equally suspect. As Bueno (2013, §3.2) observes, if we require a metamathematical *proof* that mathematics is conservative, then we end up with a vicious circle or regress: we then must use mathematical tools that we have not proved to be nominalistically acceptable to prove that mathematics (including, perhaps, those very tools) is nominalistically acceptable. And why, in the first place, should *fictional* set theoretical talk persuade us that the application of mathematics is licit in any case? Again, Field ends up with this problem because he assumes that mathematical language, as it is, is ultimately referential and that such reference fails.[6]

Modal structuralism, most prominently developed by Geoffrey Hellman, is roughly the position that mathematics is the study of structures, but that such structures need not actually exist.[7] Rather, the structures in question need only be possible, and the relevant modalities are taken to be primitive. An ordinary statement of number theory, say that there are at least three primes, is thus translated as the conjunction of two claims:

(1) Necessarily, that there are at least three primes holds in any structure which satisfies the Peano axioms, and
(2) It is possible for a structure satisfying the Peano axioms to exist,

[6] My exposition of Field's very complicated views is heavily indebted to the treatment of them in Bueno 2013 §3.
[7] For a version of the view spelled out in terms of objects, rather than structures, see Friedman 2005.

where necessity and possibility are primitive (as opposed to, say, set-theoretical) logical modalities. (Hellman 1996) Like Field's fictional operator, Hellman's modal reconstrual of ordinary mathematical language to avoid ontological commitment ultimately forces a significant departure from the ordinary use of mathematical language by working mathematicians, and the problems with Field's paraphrases apply equally to Hellman's. But Hellman's paraphrases have problems of their own. Hellman treats the relevant modalities as primitive in order to avoid circularity or ontological commitment (to the existence of possible worlds), but then it is difficult to understand the sense of the relevant modalities. In what sense is it logically possible for a structure to satisfy the Peano axioms, or for a structure to have a cardinality of, say, $\aleph_{57}$, and so on, independently of our mathematical characterizations of such things? And in what sense could such a statement be implicit in the (non-literal) meaning of a mathematical statement?

The modal structuralist, like the mathematical fictionalist, faces these problems in large part due to accepting the referential conception as an adequate picture of the face-value meaning of mathematical language. Since face-value mathematical language is taken to engender a commitment to abstract objects, problematic paraphrases must be introduced.

1.3. A faulty picture of language and the problem of ontological commitment

At the beginning of this chapter I considered some reasons why we might want to accept the referential conception. In light of these problems, I think it is time to reconsider them. To briefly summarize what I said there, one might think that a referential conception of mathematical language and the associated correspondence theory of truth in mathematics is required of us if we are to have a uniform semantic account of both mathematical and non-mathematical language. The idea is that in ordinary, non-mathematical language we use singular terms to denote objects, and predicates to ascribe properties to such objects. An atomic sentence "*a* is *F*" is true just in case the object denoted by *a* has the property denoted by *F* and *just means* that the object denoted by *a* has the property denoted by *F*. Likewise with ordinary quantifiers: "there is an *F*" just means that some object which has the property denoted by *F exists*, and it is true just in case such an object exists. If we take mathematical sentences of the same form literally, then they too involve reference to objects and their properties.[8]

For now, the question is whether this is how singular terms and quantifiers in ordinary, non-mathematical language work. And the right answer seems to me to be a resounding "yes and no." Certainly, we often use sentences of the form "*a* is *F*" to say of an object that it has a certain property. For instance, when I say, "Whiskers is black," I pick out a certain object,[9] a cat named Whiskers, and ascribe to it the property of being black. Likewise with sentences of the form "some *x* is *F*." When I say, "there is an ink mark on this page," I mean in some sense that some object is an ink mark and is on this page. But this does not seem to generalize to all uses of such language. For instance, "pride is a sin" is also of the form "*a* is *F*," but it would be quite a stretch to say that pride is an object in the same sense as an ink mark or Whiskers the cat. Likewise with "the fall of the Soviet Union was historically significant" or "1963 was a good year." The same could be said about "there is a solution to this problem," "there are words in the English language that polite people do not use," "there is a distinct concept of freedom common to thinkers of the Enlightenment," and so on.

Of course, there are ways the friend of the referential conception can explain these things away. One might think that 1963 refers to a certain temporal part or that "pride is a

[8] Further arguments can be given to the effect that the use of model theoretic semantics for formal languages reflects the fact that this is how mathematical language works and that such an understanding better allows us to understand statements in which both mathematical and non-mathematical terms occur, such as statements of laws of physics. I think a better response can be given to these arguments once a non-referential account has been spelled out. I will return to them in Chapter 3.
[9] Though, I admit, I am disinclined to call animals "objects."

sin" is best formalized in second-order logic, in which case it is not of the form "*a* is *F*."[10] That is, it is certainly *open* to someone to say so, but then it begins to look like we are forcing upon our language *a preconception of how language ought to work*. If it is even possible, one would have to jump through an awful lot of hoops to find a sense of "object" which covers all and only those things expressed by singular terms other than "things expressed by singular terms"—after all, it must include not just cats and ink marks, but also character traits, events, years, courses of action, words, concepts, and so on.

In diagnosing this tendency, Agustín Rayo attributes it to three preconceptions: first, that there is some carving of the world into objects that is most metaphysically apt; second, that to each (legitimate) singular term in our language there corresponds one of those objects; and third, that atomic sentences' truth conditions require that the object picked out by the singular term have the property expressed by the predicate (Rayo 2009, p. 242). I think that this is more or less the right way to look at the matter. What we find is a tendency to take a form of language with disparate uses and to assume that it reflects a common structure of things in the world. In the case of the referential conception, this takes the form of the presupposition that the structure of an atomic sentence of the form $F(a)$ must reflect a common structure in the world, a relation between object and property. But when we look at how atomic sentences like this are used, their purpose is only rarely to describe what lies between nature's joints, as it were. Of course, singular terms do express *something*, and the composite structure of atomic sentences built out of a singular term and a predicate is not idle. But to insist that there is something in the world (as opposed to something grammatical) which all such sentences reflect is to go far beyond what the *use* of such language justifies.

---

[10] Of course, with regard to the latter example, one could be pedantic and insist that this itself might be a departure from the literal meaning of a sentence using a singular term. But I will not bother.

Of course, one might still insist that if we deny that mathematical language (at face value) commits us to an ontology of abstract objects, then this means that we cannot treat, say, singular terms uniformly—for we must treat singular terms like "Whiskers" as serving to refer to objects, but cannot treat singular terms like "57" that way—and so we cannot have a uniform semantics for mathematical and non-mathematical language. But if the problem really is that we cannot treat mathematical terms as serving to refer to objects, then, as I have argued above, this "problem" afflicts much of non-mathematical language as well. If we continued to follow the line of reasoning in the objection, we would have to conclude that we cannot have a uniform semantics even for ordinary, non-mathematical language.

This is, of course, nonsense. Just as "this is an ink mark on the page" suffices for the literal truth of "there is an ink mark on the page," so "11 is a prime number greater than 10" suffices for the literal truth of "there is a prime number greater than 10." With respect to existential quantification, one will find no more uniformity than this in the semantics of non-mathematical language.[11]

Moreover, the literal truth of "2 is a prime number" does not imply that there is an abstract object designated by "2" any more than "pride is a sin" or "the fall of the Soviet Union is historically important" imply that there are *objects* designated by "pride" or "the fall of the Soviet Union." Of course, the proponent of the referential conception is free to claim that there are such objects, but then they have no grounds to claim that it is *nominalists* who

[11] See Ben-Yami (unpublished) for an argument for this sort of substitutional understanding of quantification in formal logic (and against an objectual, model theoretical understanding of quantification) on the basis of the fact that sentences in ordinary language may be of the same logical form and yet be made true in very different ways. Understanding all such sentences in terms of domains of objects imposes an apparent uniformity where there is actually considerable diversity. A substitutional approach allows one to take into account the common logical form of such sentences and the logical inferences that they license without treating them as made true in the same way. This, I contend, is ultimately all the uniformity we can have in our semantics for quantifiers, even in ordinary, non-mathematical language. The argument of this section as a whole is in a sense a variation on this argument.

must introduce awkwardness into their semantics. The real awkwardness arises when it comes time to explain, without equivocation, how the semantic value of each and every legitimate singular term is an object.

Now, even if we reject this picture of language, it is still open to us to conceive of mathematical terms as referring to mathematical objects. Even if an ontology of abstract objects is not a consequence of our mathematical language, our language is still consistent with reference to mathematical objects for all I have said so far. However, given that this leads to the problems discussed in §1.2 and given that our language does not force such a conception upon on, this looks less and less desirable. And if the account I propose in the next chapter is plausible, then there are viable alternatives which do not lead to such problems.

2. Toward a Non-Referential Account

2.1. A brief history of non-referential approaches

Before I begin to tell my own story about how mathematical language might be used non-referentially, it will be instructive to consider the few similar approaches proposed in the existing literature. In this section, I will consider early formalist approaches, Wittgenstein's philosophy of mathematics, and contemporary formalism with an eye to potential pitfalls, problems that require an explanation, and promising directions for a non-referential account.

2.1.1. Early formalism and three canonical problems

The non-referential approach got off to a rocky start. It first came to be relatively well-known in the form of (pre-Hilbertian[12]) formalism, proposed by Johannes Thomae and H. E. Heine. This brand of formalism, in turn, was not known for its virtues but for the devastating critique of the position delivered by Frege (1960). In effect, as Frege presents their views, Thomae and Heine equivocate wildly between two positions, which Weir (2011) calls *term* formalism and *game* formalism, respectively. The term formalist holds that mathematical language is referential, but that terms refer to symbols, understood, it seems, as concrete inscriptions. The game formalist, in contrast, holds that mathematical language is non-referential and that it is a sort of game involving the manipulation of symbols according to certain rules. Any content that the signs have is the result of the way they behave given the

[12] Formalism in mathematics ultimately has two meanings. Hilbertian formalism, more prominent in the history of mathematics and of the philosophy of mathematics, is the position that "ideal," infinitistic mathematics is an empty formal game allowing mathematicians to find simpler derivations of results that are provable in finitistic mathematics, which is contentful. In contrast, the sense of formalism that I will use throughout this thesis, unless otherwise noted, applies to positions which hold that all mathematics—not just the "ideal" portion—is merely formal, in one or the other sense of the word.

rules that govern them. With respect to the non-referential approach, only game formalism is of interest.

While many of Frege's lengthiest criticisms are aimed at term formalism, he provided important and influential criticisms of game formalism as well. First, Frege thinks the game formalist is at a loss to explain the successful application of mathematics. How can arithmetic be applied so successfully both outside of mathematics and to other branches of mathematics if it is just a game of manipulating uninterpreted symbols according to arbitrarily specified rules? In a surprisingly Wittgensteinian moment, Frege even writes, "it is applicability alone which elevates arithmetic from a game to the rank of a science" (Frege 1960, §91, p. 187). According to Frege, a prerequisite of application is expression of a thought, and this is something that an uninterpreted formal system cannot do.

Second, Frege argues that the game formalism that he takes as his target does not adequately distinguish the "game" from the "theory of the game" (Frege 1960, §107, p. 203). According to Frege, the rules of the game are a foundation of sorts for the theory of the game, not a part of the game itself; nothing in the game itself can express a rule, though moves in the game are made in accordance with rules. So the theory of the game seems to be essential even to specifying the rules of the game in question, and this theory appears to be a piece of mathematics itself, allowing us to prove results about what can be derived in the game. Even if the game itself is contentless, the theory of the game must be a contentful piece of mathematics. Moreover, this theory seems to be committed to the existence of abstract objects, which formalism was meant to avoid. After all, if the theory of the game is contentful and takes the workings of the game as its object, then it would seem to be committed to an infinite number of expression types, derivations, and so on, which cannot be identified with concrete objects like ink marks which constitute 'pieces' or 'moves' within the game.

A third canonical objection to formalism in this basic form, related to but distinct from Frege's criticisms, is that since it treats mathematics as a collection of contentless games, it must treat all mathematical systems as fundamentally on a par. Why then, when mathematicians have the option to choose between several systems, is there relative consensus regarding the one to adopt. This applies not just to choosing between standard arithmetic and arithmetic modulo 7, say, but also to choosing extensions of systems which decide previously undecidable sentences. (Horsten 2012, §2.3)

Whatever one thinks of these criticisms applied more generally, they do appear to deal a fatal blow to Heine's and Thomae's versions of formalism, and more generally to have relegated formalism to the dustbin of the history of philosophy.[13]

2.1.2. The later Wittgenstein

In his later work, Wittgenstein presents a related, but significantly different non-referential account of mathematical language. Like the game formalist, Wittgenstein treats mathematical language as non-referential and suggests at times that mathematical practice could go on without the notions of "mathematical proposition" and "mathematical truth." He treats mathematical language not as a means of describing numbers, for instance, but as a practice which operates with them in a purely syntactical way: "To say mathematics is a game is supposed to mean: in proving, we need never appeal to the meaning of the signs […]" (RFM[14] V §4).

This, however, means not that mathematics is devoid of content, per se, but only that it is devoid of descriptive, referential content. What is essential to the *meaning* (or at least the

---

[13] The position does seem to have survived in a somewhat similar form in treatments of mathematics in Wittgenstein's *Tractatus* (Wittgenstein 1922) and Carnap's *Logical Syntax of Language* (Carnap 1937), albeit with a heavier emphasis on the application of mathematics. However, due to limitations of time and space, I cannot go into these positions here.

[14] That is, *Remarks on the Foundations of Mathematics* (Wittgenstein 1978).

meaningfulness) of a mathematical assertion is how it is used. This is at the heart of two of Wittgenstein's more controversial doctrines which involve a significant departure from traditional formalism: first, that a new proof of a proposition effects a change in its meaning (even if we already have a different proof of the same proposition) and, second, that what makes a mere game of manipulating signs into a genuine piece of mathematics is its application outside of mathematics (recall Frege's second criticism of game formalism in §2.1.1.). In the first case, the new proof makes new connections between the proposition and other expressions in the mathematical system in question:

> One would like to say: the proof changes the grammar of our language, changes our concepts. It makes new connexions, and it creates the concept of these connexions. (It does not establish that they are there; they do not exist until it makes them.) (RFM III §31)

This affects how the proposition is used within the calculus and hence its meaning. In the second case, the application of a piece of mathematics outside of mathematics serves in a way to "fix" the meanings of the terms of mathematical language in giving them a certain use. This should also apply to applications of one branch of mathematics to another—for instance, in our use of arithmetical language in specifying the dimensions of a geometrical space or the use of the equation of algebra $x^2 + y^2 = 1$ to define a circle in coordinate geometry—though, to my knowledge, Wittgenstein makes little mention of this. This conception of meaning in mathematics also appears to inform Wittgenstein's rejection of undecidable propositions as meaningless at least within those systems in which they are undecidable, since such propositions have no use in such systems.

Given his conception of the meaning of a mathematical expression as its use, Wittgenstein is in a position to reject a platonist construal of mathematical language and a Tarskian or correspondence conception of mathematical truth. Reference to mathematical objects—or structures, for that matter—is not involved in the *use* of mathematical language, but is a part of a *picture* of mathematics as a project of discovery. There is nothing wrong with this picture as such, but it ultimately does not support a platonist construal of mathematical language and truth, as it does no work in the *use* of mathematical language and ascriptions of truth to mathematical propositions.

What then are the uses of mathematical language generally and mathematical truth in particular? Let us begin with truth. Wittgenstein takes the criterion of truth to be given by the conditions in which we (are entitled to) make an assertion. In the mathematical case, we make assertions relative to a system, in some sense.[15] For ease of exposition, let us take Peano Arithmetic as our system. We are entitled to assert a sentence in Peano Arithmetic just in case there is a proof of that sentence from the Peano axioms. Mathematical truth is therefore to be identified with mathematical proof. (See RFM I appendix III §§1-7) Of course, this appears to be problematic in light of Gödel's incompleteness theorems, something which Wittgenstein was well aware of. Gödel's first incompleteness theorem, put to a philosophical use, appears to show us how the axioms of any formal system with certain properties (shared by most interesting sets of mathematical axioms) commit us to accepting certain sentences which cannot be derived from the axioms.

While I will consider Gödel phenomena in greater detail in the next chapter, it is perhaps worth pausing to consider the general form of Wittgenstein's response now. There is a long-running debate on what exactly Wittgenstein is doing in his writings on Gödel's theorem, but my contention is that he is arguing that the theorem cannot do this philosophical work. The interpretation of Gödel's first incompleteness theorem according to which it shows that there are true but unprovable propositions is flatly incompatible with Wittgenstein's

[15] For example, one is entitled to make the assertion "the sum of the angles of any triangle is 180°" in Euclidean geometry, and this does not conflict with or contradict the fact that one is not entitled to make this assertion in, say, hyperbolic geometry.

conception of mathematical truth, but the theorem itself is not. The sentence which Gödel constructs to be undecidable in the system of Whitehead and Russell's *Principia Mathematica* is not true relative to the axioms of *Principia Mathematica*, on Wittgenstein's view, since it is undecidable. Given this undecidability, the Gödel sentence does not properly belong to *Principia Mathematica*, and so even if we accept a principle of bivalence we do not have a true but unprovable proposition. Moreover, the way Gödel—like many others since—argues from the theorem to the truth of the Gödel sentence is to give what is essentially a proof sketch in what Wittgenstein (and I) would call another system. Gödel's reasoning can most directly be formalized in a system which augments *Principia Mathematica* with axioms corresponding to Tarski's definition of truth as satisfiability,[16] and we thereby arrive at a proof (and therefore the truth) of the Gödel sentence in *another system*. Since Wittgenstein's conception of truth in mathematics is system-relative, he has no trouble accommodating this. (See RFM I appendix III §§7-20)

With regard to the use of mathematical language more generally, Wittgenstein seems to retain an idea first expressed in the *Tractatus* (Wittgenstein 1922):

> In life it is never a mathematical proposition which we need, but we use mathematical propositions *only* in order to infer from propositions which do not belong to mathematics to others which equally do not belong to mathematics. (§6.211)

This survives in his later work as the requirement that, to be a genuine, meaningful piece of mathematics rather than a mere sign game, a mathematical system must be applicable outside of mathematics:

> "[I]t is essential to mathematics that its signs are also employed in *mufti*. It is the use outside mathematics, and so the *meaning* of the signs, that makes the sign game into mathematics. Just as it is not a logical inference either, for me to make a change from one formation to another (say from one

[16] Although, as Neil Tennant in particular has argued, such reasoning can and perhaps should be formalized by much weaker means. See especially Tennant 2002 and 2005.

> arrangement of chairs to another) if these arrangements have not a linguistic function apart from this transformation" (RFM V §2).

In this sense, meaning in mathematics depends on its applications, and in these applications it allows us to make inferences from one non-mathematical proposition to another—for instance, from "an odd number of guests will attend the dinner event" to "there will be an odd number of guests sitting at at least one table," and knowing the latter proposition may lead us to choose round tables over rectangular ones in order to avoid awkward seating arrangements.[17] Likewise, by means of the law of classical mechanics expressed by $F = ma$ and some simple mathematics, we can infer the force exerted on an object from its mass and its acceleration.

In such cases not only do we use mathematics to make inferences from one non-mathematical proposition to another, but we also use mathematics to define and regulate certain concepts.[18] To take a simple example, in Euclidean geometry, any quadrilateral with three right angles has a fourth right angle. If someone persisted in claiming that they had found a quadrilateral with three right angles and a fourth angle of, say, 85°, then we would conclude that this person was not working with the concept of "right angle" or of "quadrilateral" which is at play in Euclidean geometry, either because they accepted a non-

[17] This example is adapted from Rayo 2009, p. 243.

[18] It should be said that Wittgenstein does not take this to be *the* essential thing about all of mathematics. While it is clear that Wittgenstein thinks that there is a very important relationship between mathematics and concepts applied outside of mathematics, his insistence upon mathematics' being a family resemblance concept prevents him from making this point in full generality. He writes, for instance, that we can recognize concept formation as "the essential thing about a great part of mathematics (of what is called 'mathematics') and yet say that it plays no part in other regions. […] Mathematics is, then, a family; but that is not to say that we shall not mind what is incorporated into it" (RFM VII §33). On the other hand, he does seem to say that the "in mufti" requirement ought to apply to all mathematics in one way or another: "I have asked myself: if mathematics has a purely fanciful application, isn't it still mathematics?—But the question arises: don't we call it 'mathematics' only because e.g. there are transitions, bridges from the fanciful to non-fanciful applications?" (RFM VII §32). And he does seem to say that what distinguishes a mathematical proposition from an empirical prediction is its role in concept formation: "The limit of the empirical—is *concept formation*. […] *In the course* of this proof we formed our way of looking at the trisection of the angle, which excludes a construction with ruler and compass" (RFM IV §§29-30).

Euclidean geometry or because they misunderstood at least one of the relevant concepts. Likewise, it is at least very plausible that our techniques for calculating derivatives play a role in fixing the sense of the concept of acceleration in classical mechanics; treating acceleration as the derivative of velocity with respect to time gives a sense to instantaneous, as opposed to average, acceleration.

On the face of it, Wittgenstein's later philosophy of mathematics provides answers to Frege's main criticisms of formalism. Mathematics and its applications go hand in hand—application providing meaning to mathematical signs and mathematics facilitating certain non-mathematical inferences and regulating certain concepts employed in such inferences—though this account could stand to be fleshed out further. Moreover, Wittgenstein appears to have at least an initial answer to the problem of the "theory of the game." In the first place, there is a marked difference between following the rules of the game and describing them. Mathematical practice is a practice which proceeds according to rules, but a description of these rules is not a proposition of mathematics any more than "bishops can only move diagonally" is a move in a game of chess. Further, insofar as we mathematize such things in the form of metamathematics, we create new pieces of mathematics no more contentful than ordinary mathematics.[19]

I think that these are insights which at the very least point to a promising direction for a non-referential account of mathematical language. In this respect, my view contrasts with the typically dismissive stance towards Wittgenstein's philosophy of mathematics taken by contemporary philosophers, including those sympathetic to a non-referential take on

[19] See, e.g., *Philosophical Grammar* (Wittgenstein 1974) Part II Ch. III §12: "There is no metamathematics. […] I said earlier 'calculus is not a mathematical concept'; in other words, the word 'calculus' is not a chesspiece that belongs to mathematics. There is no need for it to occur in mathematics.—If it is used in a calculus nonetheless, that doesn't make the calculus into a metacalculus; in such a case the word is just a chessman like all the others."

mathematical language. For instance, Weir, whose own account I will consider in the next section, finds a number of problems with Wittgenstein's views, especially with respect to the two problems that Frege set out:

> But there is no systematic theory of how this applicability comes about, no proof of a conservative extension theorem, for example, showing how application of mathematical calculi to empirical premisses will never lead us to derive an empirical conclusion which does not follow from those premisses. And there is no resolution of the problem of the metatheory. (Weir 2011, §3)

He concludes, dismissively, "On the other hand, we should observe that these notes of Wittgenstein on the philosophy of mathematics were not published by him, but by others after his death" (Weir 2011, §3).

I do not think that Wittgenstein would have bothered with a 'systematic' theory of the applicability of mathematics, a conservative extension theorem, or anything of the like even if he had prepared the remarks in RFM for publication—not out of laziness or oversight, but because, on his account, such endeavors are unnecessary, perhaps even impossible. Wittgenstein would not attempt a systematic theory of the applicability of mathematics, as such applications are diverse and not particularly amenable to the sort of theory that Weir hopes for. That said, it would be desirable to give a fuller story about the relation between mathematical and non-mathematical language than Wittgenstein provides. However, a conservative extension theorem of the sort proposed by Field (1992) and Weir (2010, pp. 127-135) would not do the job; it would be just another piece of mathematics, and circularity will plague any attempt to support an anti-Platonist philosophy of mathematics with such a piece of mathematics, just as it plagues Field's and Weir's own attempts to do so (see my remarks in §1.2.2. for a short argument to that effect)[20]. Finally, if my interpretation is

[20] Moreover, this does not sit well with Wittgenstein's own remarks on the relationship between philosophy and mathematics. Philosophy "leaves mathematics as it is, and no mathematical discovery can advance it. A 'leading

correct, then Wittgenstein did have an answer to the problem of the metatheory, albeit one that can and ought to be fleshed out further.

In providing my own account, I will be following Wittgenstein's lead in these respects, albeit filling in some of the aforementioned gaps and deviating more or less from the letter—but not, I hope, the spirit—of his treatment of mathematics. Before I get there, though, it will be instructive to consider two of the most prominent contemporary formalist accounts of mathematical language.

2.1.3. Contemporary formalism

To wrap up this brief history, I will consider the two accounts in the contemporary literature which are relatively close (at least in their aims) to the one I will propose, those proposed by Jody Azzouni and Alan Weir.

Azzouni proposes what is ultimately not a non-referential account, but one which is very close in spirit to such an account. Azzouni's view is built around a formalist core. Ordinary mathematical proofs, to be valid, must "indicate" (classes of) formal derivations. This relation of "indication" is not entirely clear, but the central thought is that ordinary proofs are related to formal derivations and formal at their core, though such proofs are neither abbreviations of such derivations, nor reducible to them (Azzouni 2005, p. 142). Like Wittgenstein, Azzouni takes such derivations to be part of a great number of different systems with their own "posits" or axioms (Azzouni 1994, pp. 79ff.).

Nonetheless, Azzouni, like Wittgenstein, thinks that mathematical language allows for interpretation and truth despite this formalist core (See, e.g., Azzouni 1994, pp. 88-105). In giving an account of how such interpretation is possible, Azzouni ultimately ends up

---

problem of mathematical logic' is for us a problem of mathematics like any other" (*Philosophical Investigations* (Wittgenstein 2009), §124).

proposing a referential account—albeit one in which we can refer successfully to non-existent objects.

When we interpret mathematical language, we stipulate that its terms refer to certain posited (but non-existent) "objects." Mathematical singular terms refer to posited objects, and quantifiers range over them. These posits are "ultrathin," in that they carry no ontological commitment and need not be indispensable to the success of our empirical theories (Azzouni 1994, p. 119). Talk of such posited objects engenders only what Azzouni later calls "quantifier commitment," which is to be distinguished from ontological commitment as such. A quantifier commitment is engendered by any theory which entails existentially quantified sentences, but this in itself is not yet a commitment to the existence of certain objects, since, for example, we often quantify over objects that we know not to exist. (Azzouni 2004) When the systems in which mathematical proofs are carried out are related to one another, this requires us to stipulate that certain terms in one system refer to the same posited "objects" as certain terms in the other system. (Azzouni 1994, pp. 118-139) By means of this stipulated coreferentiality, Azzouni's formalism can accommodate the observation that even seemingly distant branches of mathematics are richly interrelated.

There is a problem here. If mathematical terms refer to posited objects, then we still have a picture of mathematical language as serving to describe the properties of such objects. We should then expect mathematics to be answerable to these posits. But, on Azzouni's own account, such objects do no such work. Azzouni admits as much:

> Now, it is true that I explain what is going on here not in terms of our somehow recognizing that the *object* referred to didn't have the properties we thought it had, but, rather, in terms of certain later systems having virtues preferable to those of earlier systems. And, one might say, for the mathematical object to have dropped out like this on one's picture *is* for one to treat mathematical terms as irreferential. (Azzouni 1994, pp. 148f.)

Azzouni chalks up this line of criticism to a significant additional constraint on the concept of reference—namely, that one cannot refer to "ultrathin posits." But I do not think that this gets to the heart of the problem.

I take it that the real problem comes to this: reference *per se* and the objects purportedly referred to do not do any real work. The main role that reference plays in Azzouni's account is to connect two different mathematical systems by giving their terms common referents. But one does not need reference to do this, especially given that this common reference, according to Azzouni, is still merely *stipulated*. Stipulating a connection between the terms of two mathematical systems need not involve reference at all. Given that the objects thus referred to do no work themselves (besides fixing the reference), Azzouni appears to be at a loss to explain what work reference does here. I suspect that what attracts Azzouni to a referential account are many of the apparent virtues of the referential picture I sketched in Chapter 1, in particular the idea that the ordinary use of singular terms and existential quantification, even if it does not engender substantial ontological commitments, still requires some kind of reference to objects. However, if my remarks in §1.3 were to the point, then even this much is not required and does not contribute in any positive way to Azzouni's account.

Weir (1991; 1993; 2010) also proposes a broadly formalist account of mathematics in that he holds that the criteria for correct assertions in mathematics are purely formal. Weir distinguishes between several uses of assertoric language, not all of which serve to represent a mind-independent reality (Weir 2010, p. 6). According to Weir, mathematical propositions are presented in a "formal" mode of assertion quite distinct from the representational mode of assertion often employed outside of mathematics (Weir 2010, p. 69). "There are infinitely many primes" is true in formal mode, since it has a concrete proof, but the same sentence is false in representational mode, since numbers do not exist and *a fortiori* neither do primes. In this sense, the statements which mathematicians ordinarily take to be true are literally true, and no revision of mathematical language is required to avoid commitment to the existence of abstract objects.

He further distinguishes between the sense, or informational content, of an utterance and its metaphysical content, the difference being roughly that the informational content is something like the conventional, literal meaning of the utterance (as understood by competent speakers) and the metaphysical content is something like an explanation of what makes the utterance true or false. (Weir 2010, pp. 26ff.) Speakers must be aware of the informational content but not necessarily the metaphysical content of their utterances, though their utterances should at least conform (allowing for some error) to the rules given by the metaphysical content. In the case of mathematics, the metaphysical content is formal: a mathematical statement is true iff there is a concrete proof of it and false iff there is a concrete refutation (Weir 2010, p. 69).

While the metaphysical content of mathematical utterances is fairly clear, Weir says very little about their informational content. What is clear is that the informational content of such utterances is not supposed to be a representation of the underlying formal system, even though the truth conditions of such utterances are inextricably tied to an underlying formal system. Moreover, the informational content is presumably more meaningful than, say, the meaning of a position in a game of chess, insofar as we would call such things meaningful. In this sense, Weir attempts to distance himself from traditional formalists of the sort considered in §2.1.1, in that he takes mathematical utterances to be neither contentless nor representations of formal games.

That is all well and good, but we are still very much in the dark about the informational content of mathematical assertions. To use a fanciful example, suppose that Chris makes arithmetical assertions against the backdrop of a formal game given by the relevant part of the system put forth in Whitehead and Russell's *Principia Mathematica* (which gives his assertions their metaphysical content) and Christine makes them against the backdrop of the formal game given by the Peano axioms (which gives her assertions their metaphysical content), and they agree on arithmetical claims like "5 + 7 = 12." We will want to say that they mean the same thing by such utterances, but it is not clear on what basis. According to Weir, the informational content of Chris' and Christine's respective utterances of "5 + 7 = 12" will be the same just in case there is an "admissible mapping" between their respective linguistic practices, the one on the basis of *Principia Mathematica*, and the other on the basis of Peano Arithmetic (Weir 2010, p. 218). It seems then that we would have to say that Peano Arithmetic and *Principia Mathematica* share a common derivability relation (or something of the sort) if we have a suitable mapping between expressions in the one and expressions in the other, and the common informational content of Chris' and Christine's utterances seems to be fixed on the basis of the common structure of their underlying formal systems.

So, whatever this informational content is, it looks like it is fixed in relation to the formal game. The practices in which we use arithmetic do not contribute to the meaning of "5 + 7 = 12" as a sentence of pure mathematics. In Weir's account of applied mathematics (Weir 2010, pp. 127ff.), only by means of adding "bridge principles" to such systems of pure mathematics are we able to apply mathematics to the world. But, we might think, this is to put the cart in front of the horse. The formal game played according to the rules of Peano Arithmetic, and even an associated language which shares the terms and rules of "pure" arithmetic proper are not even properly arithmetical unless they are part of a wider practice of counting, adding, subtracting, and so on. These practices are an essential part of what is common to Chris' and Christine's use of sentences like "5 + 7 = 12" insofar as they express the same sense. After all, one can do many things that are not arithmetic with a formal system of arithmetic; Gödel used the system of *Principia Mathematica*, for instance, to code and prove metamathematical sentences about the system *Principia Mathematica*, though we would not be inclined to say that such metamathematical sentences have the same sense as the corresponding arithmetical sentences. Moreover, arithmetic has uses in *pure* mathematics which are very similar to its canonical extra-mathematical applications. We use arithmetical concepts, say, when we say that a certain space has *n* dimensions or that a polygon has *n* sides; it is not only physical objects which can be counted, added, and so on, but these procedures apply just as well to other branches of mathematics.

This is also problematic for his broader account of the application of mathematics. Weir takes it that mathematics is conceptually but not ontologically indispensable to science. This much is fine. However, to defend this claim, Weir, like Field, seeks a mathematical proof that a calculus of applied mathematics (including both mathematical and 'empirical,' representational terms) will not allow us to derive any empirical statement which does not follow from the empirical premises alone (Weir 2010, pp. 131ff.). If this is what we need, then Weir's program is hopeless. The proof itself will be a piece of "applied mathematics," based on a practice of manipulating uninterpreted symbols, and if we need convincing that applied mathematics is justified, then appealing to more applied mathematics will not do the job. Weir writes off the circularity as being of a variety common to most philosophers after certain results of Gödel's and Tarski's (Weir 2010, pp. 134f.), but does not address the worry that the circularity comes from the *very idea* that the applicability of mathematics requires results in applied mathematics in order to be justified—not because most interesting formal systems cannot prove their own consistency or express their own truth predicates, for even if that is not the case for a given system, we would still be using applied mathematics to justify the application of mathematics.

2.2. An alternative non-referential account

In light of this historical review, we can list some desiderata for a non-referential account of mathematical language. First, that such an account is non-referential should not mean that mathematical language does not allow for interpretation and truth. Of course, the interpretation of mathematics and truth in mathematics need not (and should not) be construed as fundamentally the same as the kind of interpretation and the notion of truth that we apply to, say, propositions of physics. But it is equally clear that mathematical sentences are interpreted as contentful, even if not descriptive, and that we do call some mathematical sentences true and others false.

Second, this content should not be understood (à la Weir) as given independently of the practices in which mathematics is embedded and applied. Otherwise, we will be at a loss to explain just how mathematics can be meaningful in isolation if it does not play a descriptive role. After all, how can we understand the operations of arithmetic as meaningful independently of, say, our practices of counting things?[21]

Third, we should be able to have an answer to the three canonical criticisms of formalism—the problems of application and the metatheory, as well as the problem of all mathematical systems' being on a par—but in so doing we should avoid the mistake made by Weir (and Field, for that matter) of taking such answers to require us to provide certain mathematical results. In particular, we should be able to answer the problem of application without giving conservative extension results.

All this is easier said than done, but here goes.

[21] Note that even Shapiro's *ante rem* structuralism is truer to this observation than Weir's non-referential formalism. For, while Shapiro cashes out these ideas in terms of the structure of the natural numbers, he does explain our understanding of this structure in terms of our reflecting on our practice of counting, our system of numerals, and so on. (See Shapiro 1997)

I propose—similarly to Wittgenstein, Azzouni and Weir—that we say that a mathematical sentence is true iff it is proved and false iff it is refuted. Proof is the sole criterion of truth in mathematics as actually practiced, and (perhaps with the exception of a few conjectures) the mathematical community treats only those sentences which have been proved as true. Even in the case of those conjectures which are widely accepted despite the absence of a proof (with worries about their being undecidable in the background), a proof will still be the ultimate arbiter of their truth or falsity. A conjecture will still only have a determinate truth value in case there is a proof of it or its negation.

This, of course, needs to be cashed out further. Truth and falsity must also be understood as relative to the systems in which such proofs are carried out, or else we will have discrepancies in the truth values of certain sentences. But this is a natural consequence of identifying truth with proof, for proofs are always carried out in the context of one set of axioms or another, whether implicit or explicit. Note that this need not commit us to relativism as such about mathematical truth. For that would only happen if we wanted to claim that, say, the concept of "right angle" as deployed in Euclidean geometry is the same as the concept of "right angle" in hyperbolic geometry for instance. In that case, we might say that the sentence "all quadrilaterals with three right angles have a fourth right angle" means the same in both systems, but is true relative to Euclidean geometry and false relative to hyperbolic geometry. But we should be careful in identifying the concepts used in one system with those used in another. I will return to this shortly.

The kind of proof relevant to mathematical truth must be specified more carefully as well. The standard formalist answer will be given in terms of formal derivations. Identifying truth with formal derivation will not do, however, since the *vast* majority of proofs are not formal. (Indeed, formal derivations are a very recent innovation. Are we to say that Euclid really did not establish the infinitude of the primes, since he gave no formal derivation?) And so, for instance, Azzouni claims that a proof ought to *indicate* a formal derivation or a class of formal derivations (Azzouni 2005). But specifying how this indication relation works is very difficult. For instance, in applying a de Morgan law to infer p & q from $\sim(\sim p \vee \sim q)$, does this serve to *indicate* the full derivation in the propositional calculus, in which it is not permitted to make this move outright? In applying the method of multiplication taught to elementary school students to conclude that 15 × 63 = 945, does one indicate, say, the (extremely long) derivation in Peano Arithmetic of the same sentence? There is, of course, something to the idea that the "shortcuts" taken in ordinary mathematical proofs serve to indicate something fuller, that some of the reasoning involved is not explicit. Nonetheless, it seems to me that "indication" of formal derivations is too vague to do what is asked of it.

I think it would be better to construe the relation between ordinary proof and formal derivation as little more than an analogy, and it seems to me that treating ordinary proofs as "derivation-indicators" takes this analogy a bit too far. What is important about formal derivations with regard to a non-referential account of mathematical language is that the steps are made purely syntactically, rather than on the basis of the meanings of the formulas used in the derivations. For this analogy to do the work required of it, ordinary proofs must be like formal derivations in the sense that one could in principle carry them out purely procedurally,[22] even if the rules governing ordinary proof procedures are significantly different from those governing related formal derivations.

[22] In many cases, it would make sense to say that proofs, like formal derivations, could be carried out purely syntactically, like when children use certain "algorithms" to perform long division. But this would seem to exclude methods of proof which rely heavily on diagrams, and in this sense the "syntactical" criterion is too restrictive. However, both in cases where the "syntactical" criterion makes sense and in cases where diagrams play a fundamental role in proofs, there are still certain procedures by means of which proofs are constructed, including procedures for using and manipulating diagrams. Surely, more needs to be said about this requirement and how it is to be applied, but unfortunately I cannot say more here.

The standards of rigor in mathematics do often seem to dictate that an ordinary proof should be able to be formalized with relative ease, but this in itself does not license the "derivation-indicator" view. I think it better to understand the sort of proof relevant to mathematical truth as ordinary proof, subject to the ordinary standards of proof for the area of mathematics in which the sentence appears, which, in any case, need not proceed on the basis of the meanings of the mathematical terms involved. This is not to say that a formal derivation will not do this work too, but only that a formal derivation is not necessary to do this work.

So, a mathematical sentence *p* is true in system S iff there is some (ordinary or formal) proof of *p* in S and false in S iff there is some (ordinary or formal) refutation of *p* in S. Where does this leave us with regard to the *meaning* of mathematical sentences? Does *p* therefore *mean*: "there is a proof of *p* in S" or something of the like? I think not. Consider first the system-relativity of mathematical assertions. That truth values are assigned relative to systems does not entail that a mathematical sentence *p means* that *p* has a proof in system S, but merely reflects the fact that such statements are *used* in some system or another. The sentence "any quadrilateral with three right angles also has a fourth right angle" does not *mean* "'any quadrilateral …' has a proof in Euclidean geometry," but it will be asserted *in* a Euclidean geometry, as it will be asserted in a purely mathematical context at the end of a proof in a Euclidean geometry. System-relativity need not make reference to systems part of the content of mathematical assertions, but rather the system serves as part of the context in which such assertions are made.

But there is a further problem with identifying the meaning of a mathematical assertion with the claim that it has a proof in some system. Here, I think we can get some help from Wittgenstein. If we construe mathematics as a collection of purely formal, syntactic games, then it is hard to make sense of the meaning of the expressions of mathematics. However, in reality, we do not use the expressions of mathematics in precisely this way. To understand the meaning of mathematical expressions we must look more carefully at how they are used both within and outside of mathematics as such.

Within mathematics, we use such expressions in proving certain sentences from others. It is for this reason that Wittgenstein claimed that a new proof of a proposition effectively alters its meaning. The new proof creates new connections between the proposition proved and other propositions of the same system—new patterns of use, one might say. To depart from Wittgenstein, who held that undecided sentences are not meaningful sentences of the system in which they are undecided, in this way we can understand the meaning within mathematics of conjectures in terms of the proofs which they facilitate. Of course, if we cannot prove the conjecture within the system in question, then it is still not a properly meaningful sentence of that system per se, but we may like its consequences enough to extend the system in order to allow it to be proved.[23]

But, to echo Wittgenstein once again, the use of mathematical expressions outside of mathematics is necessary for grounding their meaning in the ordinary sense. In the absence of such an application, mathematics is meaningful only in the way that, say, chess pieces and positions are meaningful. These too are used within a sophisticated syntactical system and are richly interrelated, but they hardly have the kind of content that one would be inclined to attribute to mathematical expressions, and we would hardly call a move in a game of chess an inference in the way we would like to apply the term to mathematics. (Cf. Wittgenstein, RFM V §2, quoted in §2.1.2 of this thesis) The only reason why a platonist can treat mathematics

[23] I will consider this idea in more detail with respect to undecidable sentences in general when I discuss Gödel phenomena in Chapter 3. A further reason to extend a system to allow an unprovable conjecture to be proved—to anticipate what I say about extra-mathematical application—would be that we found it better allowed us to capture the concepts that we are interested in modeling mathematically, to the extent that this is different from "liking the consequences" of a conjecture.

as meaningful independently of its application is that the formal apparatus of mathematics purportedly serves to describe mathematical objects, but even in that case some role must be given to extra-mathematical application.

Consider, for example, the sort of arithmetic that is taught in elementary schools. Children of course learn certain operations for transposing symbols (the numerals, as well as "+", "–", "×", "÷", and "="). A child doing homework exercises might engage in the activity as if playing a game, not thinking of the meanings of the symbols, but merely making the requisite moves. But there is more to learning arithmetic than that. This symbol manipulation is embedded in a wider practice of counting, adding, subtracting, etc. in the ordinary, non-mathematical senses of the words. For instance, if one counts five fingers on one hand and five fingers on the other, then if one counts the fingers on both hands, one arrives at ten fingers. One can teach multiplication in terms of dot matrices, and so if we have a rectangle composed of dots, and we count three dots and four dots respectively on each side, we know that we will count twelve dots in the rectangle:

• • • •
• • • •
• • • •

What I want to say is that the sign game that children learn is only contentful arithmetic insofar as signs like "+" and "×" are parasitic upon the meanings of addition, multiplication and the like in contexts outside of the sign game, such as the ones just considered. This does not make the activity of doing long division any less a syntactic manipulation of symbols, but it grounds the meaning of the symbols involved.

Of course, in higher mathematics these applications are less apparent, and one may do very good mathematical work in blissful ignorance of its potential extra-mathematical applications. Riemannian geometry certainly was not initially developed with the intention of facilitating Einstein's general theory of relativity. But insofar as it is contentful in a way that chess is not, the concepts employed must be used outside of mathematical sign games. Not to mention, even the strangest geometries, insofar as they are understood as geometries, involve some concepts like shape, size, space and so on, which are related to ordinary, non-mathematical concepts. And particular mathematicizations of these concepts serve to regulate our application of such concepts outside of mathematics—and so, for instance, the mathematics of pseudo-Riemannian manifolds contributes to defining the concept of space-time in general relativity.

Given the particularities of the use of mathematical expressions, it would not be very fruitful to try to paraphrase their meanings in any kind of categorical way. However, it should be clear at least that mathematical assertions are not assertions about proofs in systems, despite the fact that their truth depends on their being proved in certain systems.

2.3. A non-referential take on three canonical problems

At this point, let us return to the three canonical problems considered in §2.1.1 in order to see how the account just sketched out fares.

2.3.1. The problem of the application of mathematics

First, let us consider the problem of the application of mathematics. Recall Frege's first criticism of game formalism in §2.1.1. According to Frege, the only thing that makes arithmetic something more than a game is its applicability outside of arithmetic. But a prerequisite for such an application is that sentences of arithmetic "express a thought"—or have a content. The traditional formalist claims that arithmetic is just contentless symbol manipulation, and so the traditional formalist cannot explain the applicability of arithmetic, the very thing which makes it valuable.

However, if I am right, then application and contentful mathematics go hand in hand. The signs we use in arithmetic *qua* sign game are based on concepts from our pre- or extraarithmetical practice, and we use the syntactic game as a means to regulate these very concepts. If so, then the applicability of arithmetic is no great mystery. We build our mathematics up in part upon the concepts to which it will apply. If anything, the platonist account of the applicability of mathematics *qua* description of abstract objects must be far more mysterious. For then the platonist must explain how facts about objects in the Platonic Heavens can be applied so well to mundane, terrestrial matters.

Nonetheless, just as Weir criticizes Wittgenstein for making much of the applicability of mathematics without providing any systematic theory of this applicability or any conservative extension proofs, one might find this account lacking for the very same reasons. I think that this would be misguided. Mathematics as mathematics rather than a mere sign game is built upon the concepts we use when we apply it, and when we apply it, we use mathematics to govern those very concepts. Since mathematics plays a conceptual, rather than an empirical role, there is little sense in the claim that in order to be justified in applying mathematics in, say, physics, we must prove that adding mathematics to a set of true empirical propositions will not allow us to infer any false empirical propositions or something of the like—setting aside the fact that conservative extension theorems as a means to justify the application of mathematics involve a vicious circularity (see my treatments of Field's and Weir's use of conservative extension theorems in §§1.2.2 and 2.1.3). Mathematics, on my account, serves to govern those very concepts and forms part of the rich inferential structure that such concepts have. No more systematic account than this will do justice to the roles played by applied mathematics. Nonetheless, I will return to a more sophisticated version of this objection, which concerns the semantics of "mixed sentences"—sentences in which both mathematical and non-mathematical terms occur—in the next chapter.

2.3.2. The problem of the metatheory

Second, recall Frege's criticism of traditional formalism with regard to the distinction between the game and the theory of the game. Even if we take arithmetic to be a contentless, syntactic game, we can still prove things about the game in the theory of the game, and these statements will be contentful insofar as they tell us something about the properties of the game. Moreover, the mathematics we do at the level of the metatheory then seems to commit us to an infinite number of abstract objects like expression types, symbol strings, and so on.

This is slightly trickier, but still accommodated by the account just proposed. For at the level of the metatheory, we are still providing proofs by means of syntactic operations. But, insofar as we take a proof in the metatheory to tell us something about the game, we are working with a piece of *applied mathematics,* which just happens to be applied in this case to another piece of mathematics. Just as arithmetic *qua* game is based upon ordinary concepts and related practices like counting, adding and so on, so is metamathematics *qua* game based upon ordinary concepts applied to mathematics such as proof, truth, completeness, and so on. We build up a metatheory as a piece of mathematics which models such concepts. One clear example is Tarski's model theoretical truth definitions, which are meant to reflect the ordinary concept of truth-by-correspondence within the formal game of axiomatic set theory. We may set up this sort of game by augmenting one formal game with the formal apparatus needed to provide a proper Tarskian "truth" predicate along these lines. If we then interpret the formal system thus constructed in terms of ordinary concepts like truth, proof, and so on, then we can use it to make statements about the initial game. But it would not be quite right to say that we have a mathematical proof that the formal theory has certain properties, but rather that we *apply* mathematics and that we *mathematize* certain concepts in order to give such descriptions.

I will return to this issue in the next chapter when discussing the more sophisticated argument against this view on the basis of certain mathematical phenomena exemplified in Gödel's incompleteness theorems.

2.3.3. The problem of systems' being on a par

Finally, consider the objection that formalism, since it treats mathematics as a collection of contentless formal games, must treat all systems as being on a par. It cannot explain why mathematicians almost unanimously prefer standard arithmetic over arithmetic modulo 563 or an extension of Peano Arithmetic in which the Gödel sentence for Peano Arithmetic is provable over an extension in which its negation is provable. The platonist can appeal to objects, but the formalist does not have any content to fall back on. Of course, the platonist faces this problem too. Why should we be interested in *these* mathematical objects rather than *those*? But let's set that aside.

For now let us stick with arithmetic and arithmetic modulo 563. The answer rests in the treatment I gave of the application of mathematics and the relation between mathematics and ordinary concepts. In the case of arithmetic and arithmetic modulo 563, our practices of counting are such that we can continue counting indefinitely without looping around. Arithmetic as a formal game is built upon that practice. If, as children, we learned that once we reached 563, we could continue either with 564, 565, …, or with 1, 2, … and that we would be doing the same thing in either case, then we would be working with different number concepts which would be reflected by arithmetic modulo 563. Of course, if that were the case, we would have to have *very* different concepts and practices than we actually do.[24]

[24] Of course, modular arithmetic is interesting in itself, and it has extra-mathematical applications (e.g., 12- and 24-hour clocks).

So, at least with respect to this sort of example, we can explain the relatively greater interest of certain systems over others, on which the mathematical community almost unanimously agrees, in terms of our shared ordinary concepts and practices, without invoking mathematical objects.

In the case of Gödel sentences more needs to be said. Ultimately, I think we can also answer this objection by invoking shared concepts and practices. What complicates this is the fact that explanations of why mathematicians prefer extensions of, say, Peano Arithmetic in which the Gödel sentence for Peano Arithmetic is provable are very often given in terms of mathematicians' sharing a conception of truth which is incompatible with the account I have given thus far. Given this complication, I will consider this issue in more detail in my general treatment of Gödel phenomena in the next chapter.

In any case, I hope to have shown by now that the non-referential account of mathematics that I have proposed can avoid many of the most serious problems with existing non-referential accounts and is a viable alternative to referential accounts—one which ought at the very least to be taken seriously. In the next section, I will consider some more difficult problems—particularly with respect to Gödel's incompleteness theorems and the semantics of mathematical and non-mathematical language—and argue that the non-referential account that I have proposed can handle them.

3. Responses to Objections

3.1. Gödel phenomena

One more sophisticated objection to the view I proposed in the previous chapter is based on Gödel's incompleteness theorems and related phenomena in mathematics. It is related to, but much more subtle than the objection that Gödel proves *tout court* that there are true sentences of arithmetic unprovable in any recursively enumerable formalization of arithmetic. Though typically presented as an objection to deflationism about truth—the view that truth is metaphysically insubstantial and that asserting that "*p*" is true is to do no more and no less than to assert that *p*—it generalizes to any view which identifies truth in mathematics with proof.

To make things more concrete, let us consider the example of Peano Arithmetic (henceforth PA): what Gödel's first incompleteness theorem (as extended by Rosser) tells us is that if PA is consistent,[25] then there is a well-formed formula G in the language of PA which is independent of the axioms. This is, of course, bad news for a formalization of arithmetic, since then it cannot be both consistent and complete. Further, Gödel's second incompleteness theorem tells us that G can be proved within PA to be equivalent to Con_{PA}, the statement within PA of PA's consistency,[26] and so, since the Gödel sentence is

[25] Gödel's original proof of the incompleteness theorems assumes the ω-consistency, rather than the consistency, of the system in question—in the case of the original proof, the system proposed in Whitehead and Russell's *Principia Mathematica* (Whitehead and Russell 1910; 1912; 1913). The assumption of ω-consistency is slightly stronger than that of consistency proper. PA is ω-consistent iff there is no formula $\Phi(x)$, with one free variable, such that $\sim\forall x\Phi(x)$ is provable, but $\Phi(n)$ is provable for all natural numbers n (though there are alternative, equivalent definitions of this property). The ω-consistency of PA entails its consistency, but not vice versa. However, Rosser (1936) has shown how the proof of Gödel's incompleteness theorems may be modified so as to assume only the consistency of PA, and so I will talk about consistency rather than ω-consistency even when I am apparently discussing Gödel's original proofs.

[26] I may at times speak loosely of a sentence of PA as *expressing* some metamathematical property like the consistency of PA. In the context of Gödel's incompleteness theorems, on my account, this must be understood in terms of a conception of metamathematics as applied mathematics. Gödel numbering is in this sense an application of arithmetic to the study of the formal system PA, and the metamathematical sentences thus coded

undecidable given our assumption that PA is consistent, so is the statement of PA's consistency; that is, if PA is consistent, then we cannot prove that PA is consistent with the resources of PA alone.

Nonetheless, mathematicians almost unanimously agree that the Gödel sentence of PA is true and "prefer" in some way extensions of systems which can prove those systems' Gödel sentences over ones in which the negation of the Gödel sentence is provable (except insofar as they are interested in non-standard models of arithmetic). And there is a sense in which many, Gödel included, feel that accepting the axioms of PA commits us to accepting such undecidable sentences, including accepting that PA is consistent. This is something that calls out for an explanation.

A less sophisticated argument might go something like this: Gödel's incompleteness theorems show us that truth must transcend proof in mathematics. We can *see* that the Gödel sentence is true and so that PA is consistent despite the fact that these cannot be proved in PA, assuming PA's consistency. The Gödel sentence in a sense *says* that it is not provable, and it *is* not provable.

We can look back to Wittgenstein's oft-misunderstood remarks on Gödel's incompleteness theorems with respect to his own identification of truth with proof in mathematics (covered briefly in §2.1.2 of this thesis) to see why one cannot get very far with this sort of argument. Gödel's theorem is certainly not *inconsistent* with identifying truth and proof. Insofar as the Gödel sentence G is undecidable in PA, it is neither true nor false in PA. Indeed, since it is independent from the axioms of PA, it is not properly understood as

are to be understood in terms of applying the deductive system PA to the study of that very deductive system. It is in this sense that one and the same formula of PA qua formal game may be used as either a sentence of arithmetic or a sentence stating that the deductive system PA has some property. And it is in this sense that one can say that Con_{PA} expresses the consistency of PA. In PA used as a piece of applied mathematics, it is how we express PA's consistency.

belonging to the system PA, even if it is expressed in the language of PA, as it were. Insofar as *G* is a true mathematical proposition which is independent of the axioms of PA, it has a proof in some other system and is (therefore) true relative to that other system.

But Shapiro (1998) and Ketland (1999) have proposed a much more subtle argument on the basis of the incompleteness theorems. Presumably, if one identifies truth with proof, then one requires of a metatheory which introduces a truth predicate that it not allow us to prove any sentence expressible in the language of the base theory to be true unless it is provable in the base theory itself. That is, a metatheory introducing a truth predicate must be a *conservative extension* of the base theory. As a result, Gödel's results have the consequence that someone who identifies truth and proof in mathematics cannot use a notion of truth to provide the metatheoretical resources to prove that a base theory has certain properties that are very important to logicians, such as its soundness, completeness and so on, or to explain why mathematicians almost unanimously accept Gödel sentences. If one accepts a Tarskian, correspondence conception of truth in mathematics, then they can claim that it is the axioms *plus our conception of truth* which commit us to sentences undecidable in the base theory. If we augment our base theory with axioms corresponding to Tarski's definition of truth as satisfiability, we model our ordinary conception of truth within our metatheory, and by means of this metatheory we can not only prove that the Gödel sentence is true, but also that PA is sound, consistent, and so on. But if truth is identified with proof, then truth cannot do this work.

As it turns out, this in itself is no problem if we identify truth with proof. For we can add any of a number of different axioms to the metatheory in order to allow proofs of the Gödel sentence, the consistency of PA, and so on—all without invoking truth. In the simplest case, in extending PA, we might add *G* (the Gödel sentence of PA) or Con_{PA} (the sentence expressing by means of Gödel numbering the consistency of PA) as new axioms. Adding either one would allow us to prove the other. If we did not want to baldly postulate that *G* or that PA is consistent, then we could postulate the soundness of PA by adding the axiom schema $Provable_{PA}$('φ') → φ, where $Provable_{PA}$('φ') abbreviates the sentence in the language of PA which codes by Gödel numbering the statement that "φ" is provable in PA. There is, in any case, no reason why a mathematician could not do as little as this if they wanted to decide certain undecidable sentences of PA rather than invoke the mathematically stronger Tarskian apparatus. In either case, on the account that I have proposed, they would be deciding these sentences in a *new* system.

However, this may be unsatisfactory, as it involves merely *stipulating* what a Tarskian axiomatization of truth allows us to *prove*. Still, there are more subtle ways to deploy the strategy of using a non-conservative metatheory without a notion of truth to do the work that a Tarskian, correspondence conception of truth is sometimes expected to do.

This comes out particularly nicely in an exchange between Neil Tennant and Jeffrey Ketland (Ketland 1999; Tennant 2002; Ketland 2005; Tennant 2005). Tennant, in effect, argues that no notion of truth is needed to mirror the reasoning in the "semantic argument" for the truth of the Gödel sentence. The argument goes something like this: *G* is a sentence of the form $\forall n\Phi(n)$. $\Phi(n)$ is provable in PA for all substitutions of natural numbers for *n*. Since proof guarantees truth in PA, $\Phi(n)$ is true for all natural numbers *n*. Since *G* is just a universal quantification over the numerical instances of $\Phi(n)$, *G* must also be true. (Cf. Tennant 2002, p. 556) Since $\Phi(x)$ is a primitive recursive predicate (allowing all its instances to be decidable in PA, among other things), Tennant proposes that we can reconstruct the reasoning above without invoking truth by means of a comparatively modest "uniform primitive recursive reflection principle": namely, $\forall n Provable_{PA}$('$\Psi(n)$') → $\forall m\Psi(m)$, where Ψ is primitive recursive. The reasoning then goes like this: Since all numerical instances of $\Psi(n)$ are provable in PA and we accept what is provable in PA, then we should also accept $\forall n\Psi(n)$. Not only can we capture the crucial step in the semantic argument in this way, thus showing that it is not even truth *per se* which does the work needed for this argument to go through, but we can do so with much weaker *mathematical* principles than a Tarskian theory would require.

Ketland (2005) objects that Tennant still *stipulates* something that one can *prove* by means of a conception of truth that one already has. The point is that our acceptance of a theory of truth of the kind axiomatized by Tarski, explains *why* we feel that accepting a theory like PA commits us to accepting further statements which are undecidable in the theory. This is also supposed to explain why we accept a reflection principle like Tennant's. Tennant (2005) answers that whether we extend a theory on the basis of truth theoretical principles or on the basis of the above primitive recursive reflection principle, we are *stipulating* further principles. In this respect, the difference between a Tarskian theory of truth and the primitive reflection principle is that the latter is mathematically much weaker. But it is not that we can justify the Tarskian axioms on the basis of our conception of truth, while other principles cannot be justified. One may very well say, as Tennant does, "One comes to see the truth of a mathematical claim by proving it mathematically, from acceptable principles, and not necessarily by theorizing about truth. Why […] should matters be any different with a Gödel sentence?" (Tennant 2005, p. 90). This debate continued a little longer (see Cieslinski 2010; Tennant 2010; Ketland 2010), but here is where I must leave it.

The objections raised by Ketland do not lose their force even if we accept the claim I made in the previous chapter that metamathematics, insofar as it is contentful, is a piece of applied mathematics. I claim there that metamathematics, insofar as it is *about* the mathematical systems it studies, mathematizes ordinary concepts like proof and truth. If metamathematics works in this way, then why should we not claim that—since it is standard practice to formalize truth along Tarskian lines and since this formalization allows us to prove undecidable sentences of our base theory in accord with the judgments of the mathematical community—the conception of mathematical truth at play in mathematical practice is a Tarskian, correspondence conception? Surely, to deny this would be unduly revisionary, akin to telling mathematicians that, say, their typical use of non-constructive proofs is impermissible.

But this would be wrong. For when it comes to using a general purpose concept like truth in a mathematical context, we are dealing with a *conceptual* issue distinct from the conceptual work that happens in mathematics proper. We might try to formalize an extra-mathematical concept mathematically and see what follows from that particular formalization; this would be a kind of conceptual work that one could do mathematically. In the case of applying a Tarskian conception of truth in metamathematics, we might legitimately proceed by asking what follows if we adopt this formalization as a conception of truth in mathematics. But the question of how our *actual* concept of truth in mathematics *actually* works is an issue that cannot be resolved by giving proofs, but only by looking at how we actually use the concept of truth *generally* in mathematics. Ultimately, I think, Tarskian formalizations of truth fail to adequately model this ordinary concept of truth in mathematics.

In practice, proof is taken to be *the* criterion of truth in mathematics, and mathematicians do seek to *prove* Gödel sentences, the consistency of PA, and so on—albeit in stronger systems. The appeal to a Tarskian correspondence conception of truth in the case of Gödel's incompleteness theorems or in general in model theory as a branch of mathematical logic does not show that this ordinary concept of truth is not the one at work when we talk about truths of mathematics.

Moreover, we can, without invoking a Tarskian conception of truth, explain why we extend systems in the way we do. Say we want to decide certain undecidable sentences in PA. We may—in what Tennant calls the "time-honoured mathematicians' way of extending any system" (Tennant 2005, p. 90)—stipulate Con_{PA}. After all, in simply *using* PA as a system of arithmetic, we tacitly assume its consistency; if it were inconsistent, then it would not be in line with our ordinary concepts of addition, multiplication, and so on, for it would allow us to derive any sentence we wanted in the language of PA.[27] The Gödel sentence of PA would then be a new theorem of PA+Con_{PA}. In this fairly minimal way, we can explain why accepting the axioms of PA seems to commit us to accepting the Gödel sentence of PA without treating it as a true but undecidable proposition *of PA*. We can justify Tennant's even more minimal primitive recursive reflection principle on the same grounds. If we are willing to accept the axioms of PA, we must be willing to accept those sentences which are provable in PA. If we accept all numerical instances of a primitive recursive predicate, then we must be willing to accept the universal quantification over those instances. And so the axiom schema $\forall n Provable_{\mathrm{PA}}(`\Psi(n)') \rightarrow \forall m \Psi(m)$ for primitive recursive predicates Ψ comes naturally. If we do not think that the axioms for introducing a Tarskian truth predicate capture the concept of mathematical truth as it is actually used, then these new stipulations are *much* better justified than the stipulation of the much stronger Tarskian axioms.

In this way, the account that I have proposed can handle not just straightforward arguments on the basis of Gödel's incompleteness theorems, but also seemingly damning observations about how mathematics is practiced in the wake of the theorems.

---

[27] With regard to the problem of systems' being on a par, considered in §2.3.3, it is in this way that one can explain the preferability of extensions of PA in which the Gödel sentence for PA is provable by means of shared concepts and practices without invoking a conception of truth, Tarskian or otherwise.

3.2. Applicability, mixed sentences, and semantic uniformity

Another difficult objection concerns the lack of a “systematic” theory of the application of mathematics. In my account I make much of the application of mathematics in relation to the content (or lack thereof) of mathematical sentences. I claim that a mathematical system is contentful if (but not necessarily only if) it is used to formalize and regulate certain concepts in use outside of mathematics. If so, then there is no reason to think that a conservative extension theorem is required in order to justify the application of a given system of mathematics in, say, a physical theory. The role of the mathematics when applied to the world is in large part to define and regulate the concepts involved, including what can be inferred from sentences in which those concepts are used. Given this, there is little sense in asking whether mathematics allows us to derive from empirical premises other empirical propositions which do not follow from those premises.

Nonetheless, one might think, a systematic theory of the application of mathematics, even if it does not require the proof of such a theorem, is still in order. Of course, as Steiner (1998; 2005) astutely observes, there are many, very different ways in which mathematics is applied. This leads me to doubt whether such a systematic theory is possible or desirable, at least in one typical sense of the term “systematic.” But there are in any case certain crucial aspects of the application of mathematics which stand in need of an explanation.

In particular, “mixed sentences”—sentences in which both mathematical and non-mathematical terms occur—are central to our application of mathematics, not just in our scientific theories, but generally in everyday life. So it seems that some kind of story about the semantics of mixed sentences is in order. This, someone skeptical about my general non-referential story might say, is made very difficult by the fact that it appears that a different semantics must be given for ordinary, non-mathematical language and mathematical language on my non-referential account. It is important in this respect to be able to give a uniform semantics covering both mathematical and non-mathematical language. (Cf. Bueno 2013, §2.3)

A platonist, so the objection goes, can give a uniform semantics for mathematical and non-mathematical language. Mathematical language involves reference to mathematical objects, while non-mathematical language involves reference to other kinds of objects. "*a* is *F*" in either case is true iff there is an object named by "*a*" which has the property designated by "*F*", and "some *F* is *G*" in either case is true iff there is some object which has the properties designated by "*F*" and "*G*"—and so on. It is then relatively easy to have a semantics for mixed statements. To return to an example from §2.1.2, "The number of guests is odd" is true just in case the mathematical object corresponding to the number of the guests has the property of being odd.

However, if my arguments in §1.3 were to the point, then even this much is misleading. Much of non-mathematical language is not referential in this sense, unless the platonist wants to retreat to the position that anything expressed by a singular term is an object, including character traits, events, years, courses of action, words, concepts, and so on. But treating all singular terms as referring to objects will already make for a very implausible, awkward semantics—as bad as, if not worse than, having to understand the semantics of mathematical language in very different terms from the semantics of non-mathematical language.

In §1.3, I argued that the requirement of uniform semantics, insofar as it can be met for ordinary, non-mathematical language, does not force an ontology of abstract objects upon us. Now I will argue that we can have a uniform semantic understanding of mathematical and non-mathematical language in the sense required to understand mixed statements without positing a referential or "objectual" semantics. Of course, this will not be of an entirely standard sort, at least with respect to the philosophy of language. In philosophy, it is typical to treat a sentence of the form "*a* is *F*" as being composed of a singular term *a*, which takes an object as its semantic value, and a predicate *F*, which takes a function from objects to truth values (or something of the sort) as its semantic value. The structure of such a sentence latches on, so to speak, to the structure of the world. If we take this sort of view, then we can give a uniform semantics for mathematical and non-mathematical language if in both cases sentences of the same form "latch on" to the structure of the world in the same way. (Cf. Speaks 2014)

But this is not the sort of uniformity we would need if we wanted to give a uniform semantics for mathematical and non-mathematical language in general, with the aim of being able to account for the semantics of mixed sentences in a straightforward way. In order to understand semantically the sentence "three is odd and Barack Obama is the 44th president of the United States" all we need are rules to get from the meanings (*not* construed as entities) of the constituent expressions to the meaning of the expression as a whole. It does *not* require that the conjuncts "three is odd" and "Barack Obama is the 44th president of the United States" are both made true by a relation between objects and predicates. In this case, "three is odd" is something that is true in virtue of being provable in arithmetic, while "Barack Obama is the 44th president of the Unites States" is true in virtue of the fact that the referent of "Barack Obama" really is the 44th president of the United States. Nonetheless, the difference in the "truth-makers" of the two conjuncts in no way impedes our understanding the complex expression on the basis of our understanding the expressions from which it is formed.

Likewise, when we consider existential quantification, which is ordinarily understood model-theoretically and thus in terms of objects (or at least "individuals"), we may understand this substitutionally: an existentially quantified sentence of the form "some *F* is *G*" is true just in case some sentence of the form "this *F* is *G*" is true. This principle applies then even to cases when we do not refer to objects.

Of course, substitutional quantification is not uncontroversial, particularly if understood as an account of the meanings of the quantifiers (as in van Inwagen 1981). But what is important here is not whether substitutional quantification is the only game in town or whether it is always the best understanding of what quantifiers *really mean*, but only that it can be made to capture how we understand sentences of a certain form on the basis of our understanding of their parts. We can understand the (very odd) sentence "Something is a prime number or lives on a dog" as being true just in case "*a* is a prime number or *a* lives on a dog" is true for some uniform substitution of a term for *a*. "Seventeen is a prime number or seventeen lives on a dog" is true (even if the second disjunct is absurd) and so is our existentially quantified sentence. It does not matter to our understanding of the quantified sentence on the basis of its parts whether "Seventeen is a prime number" is made true by an object (seventeen) having a property (being prime) or by being provable in arithmetic.[28]

Of course, we still have to deal with what we might call "logically atomic" mixed sentences like our earlier example of "the number of guests at the dinner is odd." But this is not very difficult given an understanding of the concepts of contentful mathematics as parasitic upon related ordinary language concepts. In this case we know that the number of

[28] This is not to say that there is a consensus that such an application of a substitutional understanding of quantification is unproblematic. Due to limits of time and space, I unfortunately cannot say much about this matter here. One relatively old (and pressing) objection concerns the putative inability of a substitutional understanding of quantification to account for sentences like "Some planets are unnamed." According to this objection, a supporter of substitutional quantification for such sentences must treat it as false, since one cannot substitute the name of an unnamed planet, though the sentence is obviously true. Quine likewise writes, "If we succeed in showing that every result of substituting a name for the variable in a certain open sentence is true in the theory, but at the same time we disprove the universal quantification of the sentence, then certainly we have shown that the universe of the theory contained some nameless objects. This is a case where an absolute decision can be reached in favor of referential quantification, without ever retreating to a background theory" (1969, p. 64). But what we need for the substitutional approach to work in such cases which seem to favor objectual quantification is not for all such objects to be named, but for all such objects to be *nameable* (and this is true even when what we are naming are not "objects" in the sense that planets are objects, but even when we are naming, say, numbers). Moreover, as Ben-Yami (unpublished) argues, this is an idealization—indeed, no more of an idealization than the assumption that all individuals in the domain can be the values of interpretation functions, which is required by an objectual approach to quantification. There is certainly much more to be said about this and other objections to substitutional quantification, especially as applied generally to natural language (and to mathematics), but, unfortunately, I cannot get into it here.

guests is *n* if, were we to count all of the guests (without counting anyone more than once), we would arrive at *n*. Arithmetic tells us that an integer *n* will be odd just in case it has a remainder when divided by two (a syntactical or procedural operation). On this basis, we can understand the sentence perfectly well without invoking mathematical objects or trying to give cumbersome paraphrases of the mathematical content, and we can infer other useful things from it. If this sentence is true, we can infer, say, that there is no way to assign guests to tables such that there will be an even number of guests at each one of the tables. And so we may opt for round instead of rectangular tables in order to avoid an awkward seating arrangement.

There may be great variation in how the meanings of mixed sentences of this kind are determined. This may depend both on the branch of mathematics that we are applying and on what we are applying it to. The above example involves number theory, and so also practices of counting and dividing discrete things, upon which the concepts of number theory are in a sense parasitic. But things would be different if we were applying, say, geometry or if we were applying number theory to, say, the life-cycles of periodical cicadas.

In the case of applying geometry, the difference is clear: we would be applying different concepts with different associated practices. However, if we were applying number theory to the periodic cycles of cicadas, then this would also involve an additional kind of idealization which we do not seem to find in the case of "The number of guests is odd." It is easy to treat human beings as discrete for the purpose of counting them, but in "This species of cicada has a periodic cycle of emergence which is a prime number of years" we must abstract away from the fact that years are not discrete in the same way that guests are. The number of guests, except in very strange cases (if such cases make sense), will always be a whole number. Not so for years: it makes perfect sense for me to say, for example, that I have been living in Budapest for a year and three-quarters. To make sense of such an application, we need to treat years as discrete and countable (in the ordinary sense of the term)—ignoring small variations in the cycle of a few weeks here and there, for instance. And if we do so, it turns out that treating the cycles of periodical cicadas as prime numbers (either thirteen or seventeen years) serves well in explaining why they evolved to have such a life-cycle.[29]

I should emphasize that this variation in how mixed sentences must be treated is *not* the result of taking mathematical language to be non-referential. Such variation would be required even if we had an objectual semantics for both mathematical and non-mathematical language. Moreover, such variation is not a real barrier to understanding complex expressions in which both mathematical and non-mathematical terms occur.

Given these and similar considerations, I do not think it worthwhile to attempt to find a semantic account covering both mathematical and non-mathematical language which goes much beyond the level of uniformity that I have described here. In the end, my point is just that there is no semantic barrier to understanding mathematical language as non-referential because, to put it simply, reference is not essential to understanding composite sentences on the basis of their parts. And such an understanding of composite sentences on the basis of their parts is all we need to understand mixed sentences in an adequate way. A uniform semantics in general and the semantics of mixed sentences in particular are no barrier to understanding the applicability of mathematics understood in non-referential terms.

[29] One such explanation is that a life cycle of a large, prime number of years prevents predators from developing cycles which are divisors of the cicadas' life cycle (Goles et al. 2001). Another explanation is that it prevented hybridization of cicadas with different cycles (Cox & Carlton 1988; Tanaka et al. 2009). Such explanations more or less require treating these cycles in terms of number theory.

4. Concluding Remarks

In this thesis, I have argued that, despite appearances to the contrary, mathematical language is best understood in non-referential terms, even taken at face value. Not only is the acceptance of a referential semantics at the heart of some of the most serious problems with both platonism and the various forms of nominalism that embrace it as a good approach to the face-value meanings of mathematical expressions, but it rests, I think, on a misleading picture of language more generally, which has problems accommodating very large expanses of non-mathematical language. If nothing else, I hope to have shown that we are not *forced* by the linguistic or logical structure of mathematical language to treat mathematical language as purporting (at the very least) to refer to mathematical objects.

I have also tried to present, in relatively broad strokes, a viable non-referential alternative: we should identify truth in mathematics with proof in a system and more generally reject the claim that it has a descriptive content. However, we should not deny that mathematics can be contentful; I propose that the sentences of a mathematical system are contentful when the concepts they employ are parasitic in a certain sense on extra-mathematical concepts—such as number in arithmetic and space in geometry—and that one role of such systems is to define and regulate our use of such concepts outside of mathematics proper. In this way I think we can not only avoid the problems that have traditionally plagued non-referential accounts of mathematics, but also significantly improve on referential accounts of mathematics.

This is not to say that the account that I have provided is without limitations. I have surely not addressed every problem with which non-referential accounts of mathematical language are confronted, and some of the details of the account have been left rather vague. For instance, my contention that proofs ought in principle to be able to be constructed by means of procedures that do not invoke the meanings of mathematical terms relies upon an ordinary understanding of the term procedure, and I have not provided any additional information about how it ought to be applied. This and other problems remain to be addressed in further work.

Nonetheless, I hope to have shown that this account and non-referential accounts generally are plausible and to be taken seriously, despite their unorthodoxy. Whether they will hold up to additional scrutiny remains to be seen, of course. But, in any case, it is certainly not to be taken for granted that mathematical language, taken at face value or otherwise, is fundamentally referential.

References

Azzouni, Jody. 2004. *Deflating Existential Consequence: A Case for Nominalism.* Oxford: Oxford University Press.

—. 2005. "How to Nominalize Formalism." *Philosophia Mathematica* 13: 135-159.

—. 1994. *Metaphysical Myths, Mathematical Practice.* Cambridge: Cambridge University Press.

Benacerraf, Paul. 1973. "Mathematical Truth." *The Journal of Philosophy* 70 (19): 661-679.

Ben-Yami, Hanoch. Unpublished. "Truth and Proof without Models."

Bernays, Paul. 1983. "On Platonism in Mathematics." In *Philosophy of Mathematics: Selected Readings*, edited by Paul Benacerraf and Hilary Putnam. Cambridge: Cambridge University Press.

Brown, James Robert. 1999. *Philosophy of Mathematics: A Contemporary Introduction to the World of Proofs and Pictures.* New York: Routledge.

Bueno, Otavio. 2013. "Nominalism in the Philosophy of Mathematics." *Stanford Encyclopedia of Philosophy.* September 16.

Carnap, Rudolf. 1937. *Logical Syntax of Language.* Translated by Amethe Smeaton. London: Kegan Paul.

Cieslinski, Cezary. 2010. "Truth, Conservativeness, and Provability." *Mind* 119 (474): 409-422.

Cox, Randel Tom, and C. E. Carlton. 1988. "Paleoclimatic Influences in the Evolution of Periodical Cicadas (Insecta: Homoptera: Cicadidae: Magicicada spp.)." *American Midland Naturalist* 120 (1): 183-193.

Curry, Haskell. 1951. *Outlines of a Formalist Philosophy of Mathematics.* Amsterdam: North Holland.

Field, Hartry. 1992. "A Nominalistic Proof of the Conservativeness of Set Theory." *Journal of Philosophical Logic* 21: 111-123.

—. 1991. "Metalogic and Modality." *Philosophical Studies* 62: 1-22.

—. 1989. *Realism, Mathematics and Modality.* Oxford: Basil Blackwell.

—. 1980. *Science without Numbers.* Princeton: Princeton University Press.

Frege, Gottlob. 1960. "Frege Against the Formalists: Grundgesetze der Arithmetik, Vol. ii, Section 86-137." In *Translations from the Philosophical Writings of Gottlob Frege*, edited by Peter Geach and Max Black, translated by Max Black, 182-233. Oxford: Basil Blackwell.

Friedman, Joel. 2005. "Modal Platonism: An Easy Way to Avoid Ontological Commitment to Abstract Entities." *Journal of Philosophical Logic* 34: 227-273.

Gödel, Kurt. 1986. "On Formally Undecidable Propositions of Principia Mathematica and Related Systems I." In *Kurt Gödel: Collected Works. Vol. 1.*, edited by Solomon Feferman, John W. Dawson, Stephen C. Kleene, Gregory H. Moore, Robert M. Solovay and Jean van Heijenoort, 145-195. New York: Oxford University Press.

—. 1947. "What Is Cantor's Continuum Problem?" *Amer. Math. Monthly* 54: 515-525.

Goles, Eric, Oliver Schultz, and Mario Markus. 2001. "Prime Number Selection of Cycles in a Predator-Prey Model." *Complexity* 6 (4): 33-38.

Hellman, Geoffrey. 1989. *Mathematics without Numbers: Towards a Modal-Structural Interpretation.* Oxford: Clarendon Press.

Hellman, Geoffrey. 1996. "Structuralism without Structures." *Philosophia Mathematica* 4: 100-123.

Hodes, Harold. 1984. "Logicism and the Ontological Commitments of Arithmetic." *Journal of Philosophy* 3: 123-149.
Horsten, Leon. 2012. "Philosophy of Mathematics." *Stanford Encyclopedia of Philosophy.* Edited by Edward N. Zalta. May 2.
Ketland, Jeffrey. 1999. "Deflationism and Tarski's Paradise." *Mind* 108 (429): 69-94.
—. 2005. "Deflationism and the Gödel Phenomena: Reply to Tennant." *Mind* 114 (453): 75-88.
—. 2010. "Truth, Conservativeness, and Provability: Reply to Cieslinski." *Mind* 119 (474): 423-436.
Linnebo, Oystein. 2013. "Platonism in the Philosophy of Mathematics." *Stanford Encyclopedia of Philosophy.* October 23.
Quine, W. V. O. 1969. *Ontological Relativity and Other Essays.* New York: Columbia University Press.
—. 1960. *Word and Object.* Cambridge, Mass.: MIT Press.
Rayo, Agustín. 2009. "Toward a Trivialist Account of Mathematics." In *New Waves in Philosophy of Mathematics,* edited by Otávio Bueno and Oystein Linnebo, 239-260. New York: Palgrave MacMillan.
Resnik, Michael D. 1997. "Patterns and Mathematical Knowledge." In *Mathematics as a Science of Patterns*, by Michael D Resnik, 224-242. Oxford: Clarendon Press.
Rosser, Barkley. 1936. "Extensions of Some Theorems of Gödel and Church." *Journal of Symbolic Logic* 1 (3): 87-91.
Shapiro, Stewart. 1997. "Epistemology and Reference." In *Philosophy of Mathematics: Structure and Ontology*, by Stewart Shapiro, 109-142. Oxford: Oxford University Press.
—. 1998. "Truth and Proof: Through Thick and Thin." *Journal of Philosophy* 95 (10): 493-521.
Speaks, Jeff. 2014. "Theories of Meaning." *Stanford Encyclopedia of Philosophy.*
Steiner, Mark. 2005. "Mathematics: Application and Applicability." In *The Oxford Handbook of the Philosophy of Mathematics and Logic*, edited by Stewart Shapiro, 625-650. Oxford: Oxford University Press.
—. 1998. *The Applicability of Mathematics as a Philosophical Problem.* Cambridge, Mass.: Harvard University Press.
Tanaka, Yumi, Jin Yoshimura, Chris Simon, John R. Cooley, and Kei-ichi Tainaka. 2009. "Allee Effect in the Selection for Prime-Numbered Cycles in Periodical Cicadas." *Proceedings of the National Academy of Sciences of the United States of America* 106 (22): 8975-8979.
Tennant, Neil. 2002. "Deflationism and the Gödel Phenomena." *Mind* 111 (443): 551-582.
—. 2010. "Deflationism and the Gödel Phenomena: Reply to Cieslinski." *Mind* 119 (474): 437-450.
—. 2005. "Deflationism and the Gödel Phenomena: Reply to Ketland." *Mind* 114 (453): 89-96.
van Inwagen, Peter. 1981. "Why I Don't Understand Substitutional Quantification." *Philosophical Studies* 39 (3): 281-285.
Weir, Alan. 1991. "An Instructive Nominalism: Critical Review of H. Field: Realism, Mathematics and Modality." *Philosophical Books* 32: 17-26.
—. 2011. "Formalism in the Philosophy of Mathematics." *Stanford Encyclopedia of Philosophy*. January 12.
—. 1993. "Putnam, Gödel and Mathematical Realism." *International Journal of Philosophical Studies* 1: 255-285.

—. 2010. *Truth through Proof: A Formalist Foundation for Mathematics.* Oxford: Oxford University Press.
Whitehead, Alfred North, and Bertrand Russell. 1910. *Principia Mathematica.* Vol. 1. Cambridge: Cambridge University Press.
—. 1912. *Principia Mathematica.* Vol. 2. Cambridge: Cambridge University Press.
—. 1913. *Principia Mathematica.* Vol. 3. Cambridge: Cambridge University Press.
Wittgenstein, Ludwig. 1974. *Philosophical Grammar.* Edited by Rush Rhees. Translated by Anthony Kenny. Oxford: Basil Blackwell.
—. 2009. *Philosophical Investigations.* Translated by G. E. M. Anscombe, P. M. S. Hacker and Joachim Schulte. West Sussex: Blackwell.
—. 1978. *Remarks on the Foundations of Mathematics.* Edited by G. H. von Wright, R. Rhees and G. E. M. Anscombe. Translated by G. E. M. Anscombe. Oxford: Basil Blackwell.
—. 1922. *Tractatus Logico-Philosophicus.* London: Kegan Paul.