Hilbert's Thesis: Some Considerations about Formalizations of Mathematics

by

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ABSTRACT

In this dissertation I discuss Hilbert's thesis, the thesis that all acceptable mathematical arguments can be formalized using no logic stronger than first-order logic. In the first chapter, I present and criticize an argument for Hilbert's thesis that is often found in the literature. The argument concludes that Hilbert's thesis is true since all mathematics is reducible to set theory and set theory is a first-order theory. I argue that the reduction mentioned is not enough to establish Hilbert's thesis unless we presuppose that Hilbert's thesis is true.

In the second chapter I abstractly characterize logics and proof procedures. I then state Lindstrom's theorem (the theorem that, roughly, first-order logic is the only logic for which the completeness and Skolem-Löwenheim theorems are true) using these characterizations of logics and proof procedures.

In the third chapter, I look at some common philosophical reasons for thinking that any logic used to formalize a mathematical theory should satisfy the completeness theorem. Then I examine the Frege-Hilbert correspondence and show how Frege's position in that correspondence entails that the logic used to formalize Euclidean geometry should not be complete. I end by using Frege's position to criticize again the argument for Hilbert's thesis discussed in chapter one.

In the fourth chapter, I reconstruct Hilbert's philosophy of mathematics using notions from contemporary mathematical logic. I then use this version of Hilbert's philosophy and Lindstrom's theorem to argue that Hilbert's thesis is true. Then I examine this argument in light of (i) the use of non-deductive methods in mathematics and (ii) the standard refutation of Hilbert's program.

In the fifth chapter, I offer some speculative conclusions. I make a distinction between two uses of a formal logic and show how the argument for Hilbert's thesis described in chapter four can be used in

light of this distinction.

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If you ask what constitutes the value of mathematical knowledge the answer must be: not so much what is known as how it is known.

--G. Frege

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INTRODUCTION

Although this essay is about what has been called "Hilbert's thesis", it is with Frege that we should begin. Frege attempted to formalize mathematics: he invented a formal language and then tried to express truths of mathematics by using formulas of this language and to prove theorems of mathematics by constructing sequences of formulas of his artificial language. In this way he hoped to express the truths of mathematics precisely and to prove the theorems of mathematics rigorously.

Russell noticed[1] that "a great deal of the [mathematical] argumentation [he] had been told to accept was obviously fallacious." Frege held a similar view. In part, Frege hoped to clean up mathematics. Natural languages, he thought, are not suited for scientific discourse; they induce mathematical error. Frege, therefore, tried to construct a language, in which we can do mathematics, that does not share the vaguenesses and ambiguities of ordinary languages. In such a language, he hoped, mathematical results could be formulated more precisely, although perhaps less concisely. In this way, he thought, error could be removed from mathematics.

Unfortunately, Frege's formalization did not result in a mathematics without error. Some of the sentences of his formal language that he thought express truths in fact express falsehoods. Thus, not only did mathematics have to be cleaned up by means of a formalization using Frege's formal language, but there were errors in the resulting formalization that also had to be removed. The removal of these errors led to the construction of new sorts of formal systems: type theories and set theories. The proper formalization of mathematics, it is now claimed, is not by means of a Fregean formal system, but by means of a set theory or a type theory.[3]

Contemporary interest in Frege's work has two sides. First, there is a technical and historical interest in Frege's formal language(s). The language Frege described in the Begriffschrift[2] is one of the first examples of a formal language. Furthermore, Frege presented one of the first systems of quantification theory as we know it. Second, there is interest in Frege's programmatic attempt to express and to prove mathematical truths using a formal language. Frege was the first to construct a formal language in which a significant portion of mathematics can be expressed and proved.

In this dissertation, I shall begin examining a principle that is endorsed by Frege and by his critics who prefer either a set theoretic or a type theoretic formalization of mathematics. All hold that mathematical results are expressed and proved in an imprecise language and that theorems of mathematics might be expressed more precisely and proved more exactly if a formal language is used. They then go on to conclude that there is one formal language adequate for this task. According to Frege, this formal language is a version of the concept-script; according to his critics, it is the language of set theory (or a version of type theory). I shall be interested primarily in a view associated with Frege's set theorist critics. It is the view that the informal and imprecise notion of proof, as used in mathematics, is formally and precisely represented by the technical notion of first-order proof. This view is called by M. Davis "Hilbert's thesis".

My dissertation divides naturally into four parts. In the first part I formulate Hilbert's thesis and present several examples of statements that cannot be expressed using only formulas of first-order logic. I argue, however, that first-order logic's limited power of expression is not enough to refute Hilbert's thesis. I then examine an argument for Hilbert's thesis that is often found in the literature. The argument concludes that Hilbert's thesis is true using the claim that all mathematics is reducible to set theory. I point out a circularity in this argument by showing that the sort of reduction mentioned is not enough to establish Hilbert's thesis unless we presuppose that Hilbert's thesis is true.

The second part is a technical discussion of several notions involved in the formalization of a mathematical theory. I carefully formulate an abstract characterization of logics and define what can be called "Lindstrom logics". Then, after presenting an abstract characterization of proof procedures, I state Lindstrom's theorem, the theorem that, roughly, first-order logic is the only logic for which the completeness and Löwenheim-Skolem theorems are true. These technical details are important, since they are used in the third and fourth parts of this dissertation. The reader who is impatient with logic is, however, advised to move on to the third and fourth parts of this essay, referring back to this technical part as needed.

In the third part, I look closely at some philosophical reasons for thinking that any logic used to formalize mathematics should be complete. I conclude that the more common reasons are not entirely compelling. Then I examine the Frege-Hilbert controversy about the possibility of proving the independence of the parallel axiom from the other axioms for Euclidean geometry. I show how Frege's position in this debate presupposes a conception of logic according to which the logic used when formalizing Euclidean geometry is not complete. I conclude by pointing out the consequences of Frege's view as it relates to the discussion of Hilbert's thesis in chapter one.

Finally, in the fourth part of this dissertation, it is seen that Hilbert's thesis has the right name. I construct an argument for Hilbert's thesis using principles of a version of Hilbert's philosophy of mathematics and Lindstrom's theorem. I conclude with an examination of the status of this argument in light of (I) the use of non-deductive methods of argumentation in mathematics and (II) the standard refutation of Hilbert's program.

A word about methodology: in this dissertation I talk unhesitantly of numbers, sets, structures, standard models, Euclidean points and a host of other abstract objects. This may trouble those readers with nominalistic scruples. But that is all right. I am not doing ontology and thus feel free to quantify over all abstract objects matter-of-factly discussed by mathematicians.

Footnotes:

1. Bertrand Russell, Autobiography.

2. G. Frege, Begriffsschrift, eine der arithmeticshen nachgebildete Formelsprache des reinen Denkens, 1879.

3. Although the attempt to formalize mathematics using type theories may seem archaic to some readers, I mention them in light of the work of S. Feferman. See "Theories of Finite Type Related to Mathematical Practice" in the Handbook of Mathematical Logic as well as the promised Explicit Content of Actual Mathematical Analysis.

Chapter 1

MATHEMATICS, SETS AND PROOFS

(i)

A good way to begin our discussion is to consider an argument accepted by mathematicians as establishing that

(Propositional) For every ring there is exactly one unital morphism

is true. A unital morphism, μ, is a morphism from the integers into a ring, R, such that

(1) $\mu(a) + \mu(b) = \mu(a+b)$

(2) $\mu(a) \cdot \mu(b) = \mu(a \cdot b)$

(3) $\mu(1)$ is the unit element of R.

The argument is due to MacLane and Birkhoff[1] and is as follows:

> We have just shown that the only possible choice for μ is $\mu(n) = n\ 1'$, where $1'$ is the unit for R. The function μ so defined is clearly a morphism of addition and of units. To show it a morphism of multiplication, we need only show
>
> $$(m\ 1')\cdot(n\ 1') = (m\cdot n)\ 1' \quad m,n \in Z \quad (7)$$
>
> If m is non-negative, this may be proved by induction. Indeed, (7) is immediate for m=0, so make the induction assumption that (7) holds for some m>0 and all n. Then
>
> $$\begin{aligned}((m+1)\ 1'\cdot(n\ 1') &= (m1'+1')\cdot(n1') \\ &= (m1')\cdot(n1') + n1' \\ &= (m\cdot n)1' + n1' \\ &= (m\cdot n+n)1' = ((m+1)\cdot n)1'\end{aligned}$$

> This is (7) for (m+1), so the induction is complete. Finally if m is negative, (7) follows from the case when m is positive by Rule 3 above.
>
> [Rule 3 is: For all a,b in R, $(-a)\cdot b = -(a\cdot b)$]

Why is this argument a good mathematical argument? Why is it that after studying MacLane's and Birkhoff's argument, anyone familiar with rudimentary algebra would accept proposition 1? Why does MacLane's and Birkhoff's argument establish that proposition 1 is true? The answer to all these questions is, of course, that their argument is a proof. MacLane and Birkhoff have proved that for every ring there is exactly one unital morphism. This accords with the quite plausible view that a mathematical argument is a good argument only if it is a proof. As Putnam has put it[2]:

> It does seem at first blush as if the sole method that mathematicians do use or can use is the method of mathematical proof...

Putnam, as we shall see, goes on to disavow this view about the mathematician's methods, and in chapter 4 I shall discuss his argument in some detail. Nevertheless, he does note that there is a (seemingly) plausible view according to which all good mathematical arguments (whatever it may mean to call an argument 'good') are proofs and according to which when we try to solve mathematical problems, we look for proofs. If we want to know whether a mathematical statement, ψ, is true, according to this view, we see whether we can prove it. If we come up with a proof of ψ, we know that it is true. If we come up with a proof of the negation of ψ, we know that ψ is false. If we find neither a proof of ψ nor a proof of the negation of ψ, we withhold judgment. But just what is involved when a mathematical statement is proved? What is a proof?

Putnam continues[3] his description with

> ...and as if that method consists simply in deriving conclusions from axioms which have been fixed once and for all by rules of derivation which have been fixed once and for all.

This leads directly to a view about proofs that has become standard, according to which proofs are sorts of sequences of sentences. In particular this standard view has it that

> (SVP) A proof is a sequence of sentences every member of which is either an axiom or follows from earlier members of the sequence by a rule of inference.

is true. According to the standard view of proofs, then, two conditions must be met by all proofs. They must, first, be sequences of sentences of some correct kind of language using the (or some) correct rules of inference. Proofs must also only mention the (or some) correct axioms.

There is an obvious problem with (SVP) that should be mentioned now. The standard view of proofs apparently violates our ordinary use of 'proof'. I called MacLane's and Birkhoff's argument a proof unashamedly. I said that their argument is a good argument because it is a proof. However, it is, I think, clear that MacLane's and Birkhoff's argument is not a sequence of sentences every member of which is either an axiom or follows from earlier members by means of a rule of inference. In the first place, MacLane's and Birkhoff's argument is not a sequence of sentences; rather, it is a paragraph of English augmented with a few technical symbols. Sequences, we know, are a sort of set, and every sequence of length greater than one contains some set or other. Paragraphs, however, do not contain sets; they contain sentences organized according to, among other things, stylistic considerations. So MacLane's and Birkhoff's argument is not a sequence; hence, by (SVP), their argument is not a proof.[4] So (SVP) and our ordinary use of 'proof' conflict.

There is, of course, an obvious thing that can be said in defense of (SVP) and in response to the above objection. The fact that MacLane's and Birkhoff's argument is a paragraph of English augmented with a few technical symbols is (merely) a matter of presentation, of the way in which their proof is displayed. In fact, if necessary, we can present the argument so that it is a sequence, not a paragraph. We can take MacLane's and Birkhoff's argument to be the sequence whose first member is the first sentence occurring in the paragraph displayed above, whose second member is the second sentence occurring in the paragraph displayed above, and so on. Thus, we can correctly and in consonance with (SVP) call MacLane's and Birkhoff's argument a proof. We can say that, strictly speaking, their argument is a sequence, although for reasons of style it is presented as a paragraph of English.

If we accept the response of the last paragraph -- and, I think, it is reasonable to do so -- we still have problems with the views that MacLane's and Birkhoff's argument is a proof and that (SVP) is true. Consider

(i) If m is non-negative, this may be proved by induction,

the fourth component of the sequence obtained above. It is not an axiom, nor does it follow from the first, second and third components of that sequence by means of a rule of inference. Rather the function of (i) is to make clear to the reader what sort of argument is needed to establish what MacLane and Birkhoff call (7). The next three sentences describe in some, but not all, detail how this argument looks. Thus even if we present MacLane's and Birkhoff's argument as a sequence -- the suggestion made in the previous paragraph -- according to (SVP), their argument is not a proof. What are we to say, then, about MacLane's and Birkhoff's argument?

I think we should say exactly what we thought should be said about MacLane's and Birkhoff's argument. Their argument is a proof, even though it is not a sequence of sentences every component of which is either an axiom or follows from earlier components by a rule of inference. What this means, of course, is that the standard view of proofs is just plain wrong. What might attract someone to the standard view of proofs, despite the fact that it conflicts with our usual use of 'proof', is the conviction that every proof can be rewritten rigorously and precisely as a sequence of sentences each component of which is either an axiom or follows from earlier components by means of a rule of inference. It is, I think, the beliefs that mathematics is a rigorous science and that every (acceptable) argument of (informal) mathematics can be formalized -- or made more precise -- that lead to the identification of proofs with sequences of a certain sort. We believe that the arguments and theorems of mathematics are such that "there is a fairly simple axiom system from which it is possible to derive almost all mathematical theorems and truths mechanically."[5] So, if we formalize MacLane's and Birkhoff's argument, it can be claimed, we will obtain an argument that is a proof in the sense given by (SVP). It is the conviction that (acceptable) mathematical arguments can be formalized that is behind the standard view of proofs.

It is also the conviction that mathematical arguments can be formalized that leads to the belief that not only can acceptable mathematical arguments be presented as the right sort of sequences of sentences, but that acceptable mathematical arguments can be presented as the right sort of sequences of sentences of the right sort of language. After all, if mathematics is truly a rigorous science, then not only should it be possible to recast the arguments of mathematics so that they are sequences generated in the correct way from axioms and rules of inference, but it should also be possible to express the statements of mathematics in a language in which there are no ambiguities and for which there can be no doubt when a given rule of inference applies. An extended quotation from Wang makes clear this sentiment:

> Language is employed for expression and communication of thoughts. Failure in communication may either be caused by inadequate mastery of the language, or by internal deficiencies of the language...Language is also sometimes used for talking nonsense. Here again certain languages just seem to offer stronger temptations for doing so. And sometimes the language user is not careful enough, or he merely parrots others. In such cases he does not have thoughts...to express, and there is, of course, no question of correct communication. A less serious disease is confused thinking, often involving internal inconsistency. This again is sometimes the fault of the language, such as the ambiguity of words and a misleading grammar.[6]

Wang then goes on to claim that

> the creation of an ideal language would yield a solution of these difficulties once and for all. Such a language should be so rich, clear, and exact as to be sufficient both for expressing all thoughts...with unmisunderstandable clarity, and for precluding nonsense.[7]

Thus, the belief that mathematical arguments can be reformulated so that they are rigorous and precise leads directly to the view that, in addition to being sequences of sentences every component of which is either an axiom or the result of applying a rule of inference to earlier components, formalized mathematical arguments have as components sentences of some ideal, formal (or artificial) language.

Let me call such sequences derivations. Then the view that should replace (SVP) is that all acceptable arguments of (informal) mathematics are proofs and that proofs can be formalized as derivations. I shall call this view Leibniz's thesis. It is stated again for future reference:

(Leibniz's thesis)

(i) Every acceptable argument of (informal) mathematics is a proof;

and

(ii) Every proof can be formalized as a derivation.

Leibniz's thesis is, I think, generally accepted -- both by the mathematical community and, with some notable exceptions[8], by the philosophical community. Claim (ii) of Leibniz's thesis is endorsed by writers like Steiner, who claims[9] that

> proof is formal proof. Arbitrarily we pick a system -- Church's "applied first-order functional calculus." Then, proof is proof from premises...in Church's sense. Usual usage is looser... because informal arguments are universally described as proofs...[T]he mathematical community...has been persuaded that no proof is rigorous if not "formalizable"...[It is agreed that] nothing is a proof if not formalizable.

Steiner goes on to claim that derivations are "the Platonic ideal in virtue of which the informal argument is valid." (It should perhaps be noted that although we have seen Steiner endorsing claim (ii) of Leibniz's thesis, he does not, in fact, endorse claim (i). As will become clearer in chapter four, Steiner holds that there are acceptable arguments in informal mathematics that are not proofs. These are arguments that have the same structure as the ordinary inductive arguments found in empirical sciences.)

According to Leibniz's thesis, although the actual arguments made by mathematicians are not derivations, they might be. If we wanted to, we could construct from, say, MacLane's and Birkhoff's remarks, a sequence of sentences of a formal language every component of which is either an axiom or follows from earlier members of the sequence by a rule of inference.[10] The arguments mathematicians ordinarily use play a dual role; not only do they convince us that a given theorem is true, they also indicate how to construct a derivation of (a formalization of) that theorem.

Now Leibniz's thesis tells us nothing about which rules of inference and which formal languages are to be used when formalizing the arguments of informal mathematics. In fact, consistent with Leibniz's thesis is the claim that there is more than one formal language that can be used to formalize given arguments of informal mathematics, and that, similarly, there may be more than one set of rules of inference. All that Leibniz's thesis entails is that for any given argument of informal mathematics there is a formal language and a set of rules of inference that can be used to formalize that argument. If we add to Leibniz's thesis the claim that only one formal language and only one set of rules of inference are needed to formalize adequately[11] all mathematical arguments, we have

(Frege's thesis)

(i) Leibniz's thesis is true

(ii) There is a formal language and a set of rules of inference that can be used to formalize adequately all proofs.

I shall be especially interested in one version of Frege's thesis called <u>Hilbert's thesis</u>. It is, roughly, the view that all arguments of informal mathematics can be formalized adequately using only the first-order predicate calculus. As Barwise[12] described it, Hilbert's thesis is the view that

> ...there is no logic beyond first-order logic in the sense that when one is forced to make all one's mathematical (extra-logical) assumptions explicit, these axioms are always expressible in first-order logic, and that the informal notion of <u>provable</u> used in mathematics is made precise by the formal notion <u>provable in first-order logic</u>.

Three warnings should be given, perhaps unnecessarily. Leibniz's thesis was not explicitly endorsed by Leibniz, and Frege's thesis was not explicitly endorsed by Frege. Nor was Hilbert's thesis explicitly endorsed by Hilbert. However, as we shall see, reasons for endorsing Hilbert's thesis can be extracted from Hilbert's philosophy of mathematics.

Also, I have formulated Leibniz's thesis, Frege's thesis and Hilbert's thesis so that we may see the steps of presuppositions behind Hilbert's thesis, and so that, in the future, we may see what arguments for and against Hilbert's thesis are supporting or attacking. After the next chapter we shall be able to formulate more technical versions of these theses. The reader will have to wait until then to resolve any questions that may seem to derive from the vague form in which these theses have been stated.

(ii)

There are plausible reasons for denying Hilbert's thesis. The limitations of a first-order language's powers of expression have often been pointed out. In addition, the most natural formalizations of statements of elementary mathematics frequently are not first-order. If we try formalizing MacLane's and Birkhoff's argument, for instance, we soon find that using only first-order notation, although possible, is unnatural and tricky. Expressions like "all morphisms" and "all integers" suggest non-first-order formalizations, and MacLane and Birkhoff, in the course of their argument, seem to be quantifying over morphisms and integers unhesitantly. Even proposition 1 seems to be of a form that often defies first-order formalization. It looks as if the form of proposition 1 is

(A) For every A, there is exactly one B,

and it is not difficult to see that (A) is a form with no first-order analog.[13] For example, consider

(B) For every natural number, there is exactly one real number.

(B) apparently is of the form (A); so if (A) had a first-order analog, we should be able to formalize (B) using a formula, $\Psi(N,R)$, of first-order logic, containing only two non-logical constants. Now, (B) is true if and only if there is a one to one correspondence between the real numbers and the natural numbers. So (B) is true if and only if the cardinality of the set of natural numbers is at least as great as the cardinality of the set of real numbers. Let $\mathfrak{R}$ be a standard model of the theory of real numbers. Then $\mathfrak{R} \models {}^{\sim}\Psi(N,R)$, since in a standard model of the real numbers there are uncountably many reals but only countably many naturals, and we have supposed that $\Psi(N,R)$ is a foramlization of (B). However, by a strong form of the downward Löwenheim-Skolem theorem, there is a countable submodel, $\mathfrak{R}'$, containing any countable subset of $\mathfrak{R}$, e.g., N, and for every first-order sentence, γ, if $\mathfrak{R} \models \gamma$, then $\mathfrak{R}' \models \gamma$. So $\mathfrak{R} \models {}^{\sim}\Psi(N,R)$. But this is impossible as there are countably many reals and countably many naturals in $\mathfrak{R}'$. $\Psi(N,R)$, therefore, cannot be first-order.[14] So, it seems, no construction of first-order logic is a formal analog of (A).

We cannot, however, conclude from this argument that proposition 1 is of a form with no formal analog among the formulas of first-order logic. Proposition 1 can be understood so that it says the same thing as

(B') For every ring, there is exactly one unital morphism of that ring.

(B') is not subject to the sort of argument that led us to conclude that (B) is of a form with no formal analog among the formulas of first-order logic. The fact that a particular unital morphism is a morphism of a particular ring is crucial. We cannot define a unital morphism without reference to a ring. So, propositionl, as it turns out, is of a form that has a first-order analog. But what reason do we have for thinking that every statement of ordinary mathematics that is expressed by a sentence (apparently) having form (A) can be further analyzed so that it is expressed by a sentence of, for instance, form

(A') For every A, there is exactly one B of that A,

or of some other form represented by a first-order construction? No obvious reason, I think, other than something like Hilbert's thesis; although, as we shall soon see, there may be convincing, but unobvious, reasons for concluding that all admissible formalizations are first-order formulas, and that, therefore, every statement of ordinary mathematics can be expressed by a sentence having a form with an analog among the formulas of first-order logic.

We do not have these sorts of worries, of course, if we are willing to give up Hilbert's thesis. (A) has a formal analog among the formulas of second-order logic. There is a second-order formula true in all and only those structures, containing A's and B's, in which for every A there is exactly one B. Reading "Ax" and "Bx" for "x is an A" and "x is a B" respectively,

(C) $\exists\mu[(\forall x)(Ax\rightarrow B\mu(x))\ \&\ (\forall x)\ (\forall y)\ (\mu(x) = \mu(y)\rightarrow x=y)$
$\&\ (Bx\rightarrow(\exists z_{\varepsilon A})\,(\mu(z)=x))]$

will do. Thus, all statements expressed by sentences having form (A) can be expressed by formulas of second-order logic.

I should mention that (A) is not alone in this regard. There are many constructions that we seem ordinarily to use, but that have no first-order analogs.[15] Perhaps the most famous is Frege's definition of 'ancestor'. An individual, x, is the ancestor of an individual, y, just in case x is the parent of a parent of a parent of...of a parent of y. So, it seems, we should be able to define the relation <u>is an</u>

ancestor of in terms only of the relation is a parent of. No such definition is possible, however, using exclusively first-order notation. But if we use second-order class variables, we can define is an ancestor in terms of is a parent of without using any other non-logical constants. We can say that x is an ancestor of y if and only if (i) x is not identical with y and (ii) x is a member of every class, α, and z' is a parent of z, then z' is a member of α. Thus, using second-order notation we can define is an ancestor of in terms of is a parent of, in a (relatively) natural way, even though no such construction is possible using only first-order notation.

Similar remarks apply to the relation is identical with. Identity has an odd status for logicians who accept Hilbert's thesis. On the one hand, they want to count the identity sign as a logical constant, on the same footing as 'and' and 'or'. On the other hand, since they endorse Hilbert's thesis, they are unable to define identity in terms of obviously logical operations. Thus, they are forced to introduce identity as a primitive logical operator.[16] No such problems face the logician who denies Hilbert's thesis because identity can be defined using second-order notation. We can say that an object, x, is identical with an object, y, if and only if for all classes, α, x is a member of α if and only if y is a member of α. Thus, using second-order notation we can see straightforwardly that identity is a logical relation.

Finally, it should be pointed out that general cardinality claims cannot be made in a straightforward fashion using only notation belonging to first-order logic.

(C') There are just as many cats as dogs,

for example, is naturally formalized by, first, defining the relation has as many members as (there are several standard ways to do this in second-order logic), and, then, saying that (C') is true if and only if the class of cats has as many members as the class of dogs. We can do this straightforwardly using second-order logic, although there is no obvious way to proceed using notation that is exclusively first-order.

I think that it is fair to conclude, in light of these sorts of examples, that many natural constructions cannot be carried out in a straightforward manner using exclusively first-order notation, although they can be carried out using formulas from second-order logic.

In addition, the proof of proposition 1 also seems to resist first-order formalization. We seem to need weak second-order logic or ω-logic, in order to formalize MacLane's and Birkhoff's argument adequately. Consider its first sentence. The first sentence has approximately the same meaning as "every function that is a unital morphism takes n to the result of multiplying 1' by itself n times". This sentence seems most naturally formalized as

(D) $(\forall\mu)\ (F\mu\rightarrow (\forall n)\ (\mu(n)=n1'))$,

a sentence with not only functional quantifiers, but with quantifiers ranging over natural numbers as well. Furthermore, in order to make sense of the notation "n1", we must presuppose that there are natural numbers distinct from the other elements in our universe of discourse, for "n1'" is supposed to denote the result of interating multiplication of 1' by 1' n- times, that is, n1' is

$$\underbrace{1' \cdot 1' \cdot 1' \cdot \ldots \cdot 1'}_{\text{n-times}}$$

and so the notation in question only makes sense if "n" refers to a natural number. Thus, in order to understand the proof of proposition 1, we must suppose that the notion of a natural number is understood; Maclane's and Birkhoff's argument presupposes facts about the natural numbers. One might, therefore, expect the derivation formalizing MacLane's and Birkhoff's proof to reflect this fact. To do this, it is reasonable to suppose, weak second-order logic must be used. The most natural formalization of MacLane's and Birkhoff's proof, then, is a sequence of non-first-order formulas.[17]

The sort of considerations raised so far suggest that Hilbert's thesis is false. We have seen, for example, that no formula of first-order logic can be construed as an analog of (A). Since (B) appears to have the form (A), we might conclude that (B) cannot be formalized into a formula of first-order logic. This, in turn, suggests that Hilbert's thesis is false, since (B) certainly looks like the sort of sentence a mathematician would use. This conclusion, however, is too hasty.

What the argument following (B) shows is that if (B) must be formalized by a formal analog of (A), then (B) cannot be formalized using a formula of first-order logic. That argument does not, however, show that the statement (B) expresses can be expressed by no first-order formula. For example, we might express that statement using the sentence letter 'p' by insisting that 'p' is true in a model if and only if (B) is.[18] 'p', however, is an admittedly poor formalization of (B), and I do not think we would take the fact that we can express the statement expressed by (B) using 'p' as good evidence for Hilbert's thesis, or even for the claim that (B) can be formalized into a formula of first-order logic. At least two sorts of considerations influence our choice of formalizations.[19]

First, I think it is obvious that if ψ is an adequate formalization of an arbitrary sentence, V, then ψ must have the same truth conditions as V. But this is not enough to justify the claim that V can be adequately formalized as ψ. Not only must ψ and V have the same truth conditions, but if ψ is an adequate formalization of V, then we think, ψ must be structurally related to V in a natural way. It is the latter sort of consideration that leads to the claim, in light of the argument following (B), that (B) has no adequate first-order formalization. On the face of it, (B) contains only two non-logical constants -- 'natural number' and 'real number'. So, we expect (B) to be formalized using a formula, $\Psi(N,R)$, containing only two non-logical constants. But we can find no such item, as we have seen, among the formulas of first-order logic, that has the same truth conditions as (B). (It can, of course, be claimed that if we fix the interpretation of N and R, then we can find such an item among the formulas of first-order logic. More about this sort of consideration will be discussed as we go along. For the moment let us adopt the view that N and R do not have fixed interpretations.) It is for this reason that we conclude (B) has no first-order formalization.

What it means to say that an adequate formalization of a sentence must be related to that sentence in some natural way is unclear. The issues raised by such a claim are notoriously complex, and I do not intend to pursue them here. I will suppose that we have a rough idea of what it is for a formalization to be structurally related to a sentence in a natural way -- we will not need anything but a rough idea, and we will probably not even need that. But now let us ask, in light of what has been said, why would anybody think that Hilbert's thesis is true? Given that the natural formalizations of such statements as (B), apparently, are not first-order, what sorts of reasons can be given for Hilbert's thesis?

Naturalness is not always a virtue. Philosophical considerations may favor one formalization over another, even though the latter is a more natural formalization than the former. We may have good reasons for not formalizing a sentence in the most natural way. A good example of philosophical considerations overriding considerations of naturalness can be found in the work of Nelson Goodman.[20] Goodman, a nominalist, is troubled by the use of class quantifiers. Nevertheless, he wants to be able to formalize (C') (see above). So, Goodman proposes that instead of the second-order formalization of (C'), we formalize (C') by first noticing that it is true if and only if

(C") Everything of which every cat and dog is a part has as many cat parts as dog parts

is true. If we then introduce new relation symbols, 'H' and 'P', interpreted as "has as many dog parts as cat parts" and "is a part of", respectively, we can easily formalize (C") without using class quantifiers. By similarly introducing new relation symbols, Goodman claims, we can formalize every claim making general cardinality comparisons without using class quantifiers. The fact that these formalizations of general cardinality claims are not as natural as their second-order counterparts does not trouble Goodman; nominalistic considerations, he thinks, outweigh considerations of naturalness. In a similar fashion, it can be hoped, there may be philosophical reasons for preferring, in general, first-order formalizations to more natural second-order formalizations, thus vindicating Hilbert's thesis.

It is often noted that we can dispense with non-first-order notation, perhaps at the cost of naturalness, by using a first-order language with one binary relation symbol, 'ε'. For instance, something like '$(\exists F)(F3)$' becomes '$(\exists x)(3 \varepsilon x)$. Statements naturally formalized using functional quantifiers, like those occurring in (C) and (D) require a bit more attention. For an example, consider

(E) There is a function that maps 0 to 1.

(E), I have suggested, would naturally be formalized as

(F) $(\exists \mu)\ (\mu(0)=1)$,

a sentence with a second-order functional quantifier. We can, however, using well-known techniques, formalize (E) as a first-order sentence. First, as is usual, define the ordered-pair consisting of x and y, $\langle x,y \rangle$, (in that order) as $\{\{x\}, \{x,y\}\}$. Next, define a function to be a set of ordered pairs, α, such that whenever $\langle x,y \rangle$, $\langle x,z \rangle \varepsilon \alpha$, $y=z$. Keeping this in mind, we formalize (E) as

(F') $(\exists x)$ (x is a function & $\langle 0,1 \rangle \varepsilon x$).

(F') is an abbreviation for a formula in the (first-order) language of set theory. If we could find convincing reasons, then, for preferring set theoretic to second-order formalizations, we would have the beginnings of (one kind of) a defense of Hilbert's thesis.

We should conclude this section by noting that although not every sentence ordinarily used by mathematicians has a natural first-order formalization, we can dispense with a large number of non-first-order notations if we use the language of set theory, and since this language is first-order, there is a sense in which sentences naturally formalized using second-order quantifiers can be formalized without them. An important thing to note about this point, though, is that 'ε' must be interpreted set theoretically.

(iii)

That there is a sense in which the use of non-first-order formulas can be eliminated and statements expressed by non-first-order sentences paraphrased by using first-order formulas of the language of set theory is often cited as a point in favor of Hilbert's thesis. It is often claimed that all of mathematics is reducible to set theory and that since set theory is a theory in a first-order language whose only non-logical constant is ε, anything provable in mathematics is first-order provable. This argument can be found in philosophical literature as well as in mathematical literature. Morley[21] writes:

> Another way to reduce mathematics to first-order logic is to observe that:
>
> (i) all mathematics can be reduced to set theory
>
> and
>
> (ii) the intuitive content of set theory is expressible in a set of first-order axioms about the binary relation ε.

I shall call this line of reasoning "Morley's argument", although I do not mean to credit (or discredit) him as its originator. Others make similar claims. Monk[22], for instance, asserts that first order proof

> ...is our rigorous formulation of the intuitive notion of a proof. In fact...we consider mathematics itself to be formalized on the basis of set theory....Mathematical language can be identified with a certain definitional expansion of the language of set theory...The axioms Γ of mathematics are just the usual axioms of set theory together with all the defined symbols...It is our conviction that any mathematical proof can be expanded somewhat routinely to eventually reach the form of a formal [first-order] proof from Γ.

He then adds:

> Of course this conviction is another instance...of a judgement of applied mathematics that is not subject to a rigorous proof.

This dissertation will examine reasons for and against this judgement.

To evaluate Morley's argument we must look closely at the claim that all mathematics is reducible to set theory. We have already seen that there is a function mapping 0 to 1, in a sense, can be expressed in the language of set theory. But the task of a reduction of mathematics to set theory is not only to show how statements of mathematics can be expressed in the language of set theory. Its task is also to show how ordinary proofs of theorems of mathematics can be presented as -- or, in Monk's words, "expanded...to eventually reach the form of" -- a formal derivation from the axioms of set theory. Quine[23] distinguishes between doctrinal and conceptual aspects of reductions. Showing how to reduce the concepts of a scientific discipline to epistemologically sound concepts is the conceptual aspect of a reduction. For example, if we could show how to express statements about physical objects using only terms referring to sense data, we would have reduced physical concepts to phenomenal ones, and would have accomplished the conceptual aspect of a reduction. On the other hand, when we show how the truths discovered by a group of scientists can be derived in an obvious manner from a set of obviously true statements, we have reduced the doctrines of that science to simpler ones and have accomplished the doctrinal aspect of a reduction. Showing, for example, how to derive the truths of (Euclidean) geometry from (Euclid's) axioms is the doctrinal aspect of a reduction.

It is worth our while to think carefully about these matters as they reflect on claim (i) of Morley's argument, the claim that ordinary mathematics can be reduced to set theory. On the one hand, we have the ordinary statements and argumentation of mathematics. On the other hand, we have a formal system, set theory, consisting of an infinite set of axioms and rules of inference permitting the derivation of theorems from those axioms. What would justify the claim that the former is reducible to the latter? A little thought shows that three things are needed. First, we need a set of reductive definitions. These would be definitions of concepts of ordinary mathematics (like <u>point</u>, <u>real number</u> and <u>group</u>) in terms of concepts of set theory, that is, concepts that can be defined using only ε. (We might, of course, despair of ever explicitly making these definitions; however, a sketch of how to go about forming reductive definitions would, in most cases, do.) Second, we need a set of instructions showing how to replace arguments of ordinary mathematics with derivations in the language of set theory. (Again, a sketch of these instructions might be enough.) Finally, we need some sort of argument showing that, using the reductive definitions and the instructions showing how to form derivations in the language of set theory from ordinary arguments of mathematics, we can derive (set theoretic statements expressing the) theorems of mathematics from the axioms of set theory. We can call each of these three aspects of a reduction "the conceptual aspect", "the dialectical aspect", and "the doctrinal aspect", respectively.

It must be stressed that conceptual, dialectical and doctrinal aspects of reductions cannot be performed independently of one another.[24] Often our only reasons for thinking we have accomplished the conceptual aspect of a reduction are the successes of the dialectical and doctrinal aspects of that reduction. We might, for example, amend an apparently good conceptual aspect of a reduction in order to make better the dialectical and doctrinal aspects. Performed in a vacuum the conceptual aspect of a reduction (and similarly for doctrinal and dialectical aspects) may have no interest. This is not meant to deny that sometimes the conceptual aspect of a reduction may be what we are mainly interested in; however, the evidence for the adequacy of the conceptual aspect of that reduction, in part, depends on the dialectical and doctrinal aspects.

Now, a reduction of mathematics to set theory, if it is to be used as evidence for Hilbert's thesis, must not only show how the concepts of set theory can be used to define the concepts of mathematics, it must also show how the arguments of ordinary mathematics can be replaced by first-order derivations from the axioms of set theory. For some purposes only the conceptual aspect of a reduction of mathematics to set theory is important; we can sometimes leave the dialectical and doctrinal aspects of that reduction unclear. For example, if we want to provide mathematical language with a formal semantics, we might be able to ignore the issues raised by the doctrinal and dialectical aspects of a reduction of mathematics to set theory. We might only be concerned with giving set theoretic truth conditions for given statements of mathematics. We saw how to do this for statements about ordered pairs and functions. But Hilbert's thesis is not only a claim about what sort of language must be used in order to express statements of mathematics. It is a claim about mathematical proofs. It, in part, is the claim that the informal notion provable is formalized adequately by the notion first-order provable. defining mathematical concepts using sets and expressing mathematical statements using the language of set theory are only the first steps towards showing that ordinary arguments of mathematics can be presented as formal derivations from the axioms of set theory; and it is this last claim that justifies the conclusion that Hilbert's thesis is true given claims (i) and (ii) of Morley's argument. The conceptual aspect of a reduction of mathematics to set theory is only going to be evidence for Hilbert's thesis if there is reason to believe that something like

> (G) If ψ, an ordinary statement of mathematics, can be expressed by φ, a sentence in the language of set theory, then ψ is provable only if φ is first-order derivable from the axioms of set theory.

is true; and evidence for (G) can only be obtained through the dialectical and doctrinal aspects of a reduction. Not only must we show how to construe functions as sets of ordered pairs and how to express statements about functions using the language of set theory, if we are to justify Hilbert's thesis using Morley's argument, we must also show that arguments that certain functions exist can be presented as first-order derivations that certain sets of ordered pairs exist.

This last point is important, especially in light of Cohen's and others' contributions. We know that some statements of mathematics cannot be expressed by sentences of the language of set theory that have first-order derivations from the most obvious axiomatization of set theory, ZF (or even ZF+Choice). If, for example, ZF is consistent (in the sense that there is no derivation of some sentence from the axioms of ZF), then there is no derivation from the axioms of ZF of (the sentence expressing) the statement that there is a dense linearly ordered set such that each collection of disjoint open intervals is at most countable, and there is no derivation of the negation of this statement. This is known as Suslin's problem. In 1920 Suslin asked whether every linearly ordered set that is dense and unbounded (that is, for every a, b, if $a<b$ there is a c such that $a<c<b$ and there is no greatest and no least element), complete (that is every Cauchy sequence has a limit) and satisfying the countable chain condition (that is, every collection of disjoint open intervals is at most countable) is isomorphic to the real line. If we express an affirmative answer to Suslin's problem in the language of set theory, we will have a sentence, ψ, such that there is no first-order derivation from the axioms of ZF+Choice of ψ and there is no first-order derivation from the axioms of ZF+Choice of $\overline{\psi}$. Then if (G) is true, there is no way to prove whether or not Suslin's problem has an affirmative answer. And so, if we believe that the "sole method that mathematicians do use...is the method of mathematical proof", we have to conclude that Suslin's problem has no answer.

This is a very disturbing conclusion to have to make. Suslin's problem is the sort of problem a topologist naturally would address. Topologists interested in characterizing the real line would be interested in resolving Suslin's problem. However, if the power of mathematical proof does not outrun the power of set theoretic proof, there is no proof resolving Suslin's problem. The same is true, as is well-known, of the Contiuum hypothesis. So accepting Morley's argument as reason for Hilbert's thesis, apparently entails that there are no proofs yielding answers to questions mathematicians normally would ask.[25] Given this disconcerting conclusion, why would we want to reduce mathematical proof to set theoretic proof? What reasons can be adduced in favor of claim (i) of Morley's argument?

Zermelo[26] once described set theory as

> that branch of mathematics whose task is to investigate the fundamental notions "number", "order", and "function", taking them in their pristine form, and to develop thereby the logical foundations of all arithmetic and analysis.

Zermelo's belief can be traced to the successes of Dedekind, Cantor and others, who, using only simple set theoretic operations, were able to construct the rational numbers, the real numbers and even the complex numbers, starting only with the natural numbers. Their methods are well-known and the story of their successes is exciting. I will not repeat all of it here. What we should recall is that given the natural numbers, rational numbers can be construed as equivalence classes of ordered pairs of integers. Then (depending on our tastes) we can construe the real numbers either as equivalence classes of Cauchy sequences or as Dedekind cuts. It is an insight of some importance that these constructions can all be based on a simple albeit infinite set of axioms, namely the axioms of ZF. in ZF we are able to construct isomorphic copies of the natural numbers. Then, using the axioms of ZF, we are able to isolate an isomorphic copy of the rational numbers by taking equivalence classes of ordered pairs of elements of one of the isomorphic copies of the natural numbers. We can then show that Dedekind cuts of these equivalence classes exist, using only the axioms of ZF. Thus, in ZF, we can show that any class of mathematical objects ordinarily needed for the purposes of arithmetic and analysis exist. This is what (to a large extent) first motivated -- and still motivates -- the study of set theory, and it is this fact that is behind Zermelo's claim as well as claim (i) of Morley's argument.

However, carrying out the Cantor-Dedekind constructions using only the axioms of ZF does not show that mathematical proof is no stronger than formal set theoretic proof. Only the conceptual aspect of a reduction of mathematics to set theory has so far been accomplished. The Cantor-Dedekind constructions provide a set of reductive definitions of the main concepts of ordinary mathematics in terms of sets. They also provide part of the dialectical and doctrinal aspects of the reduction. They show us how to present a good many proofs of theorems of mathematics as derivations from the axioms of ZF. But, I think it is fair to say, their reduction leaves open the question whether only arguments that can be handled by means of the Cantor-Dedekind constructions are available to the working mathematician. We have not yet seen any reason for thinking that <u>all</u> the arguments of ordinary mathematics can be presented as derivations from the axioms of ZF. We do have enough of the doctrinal and dialectical aspects of a reduction of mathematics to set theory to conclude that the conceptual aspect of the reduction is sound. But we, as yet, do not have enough of the doctrinal and dialectical aspects to conclude that everything mathematically provable can be presented as a formula having a (non-trivial) first-order derivation. So, it seems, we do not yet have enough evidence to conclude from Morley's argument that Hilbert's thesis is true.

In "Epistemology Naturalized"[27], Quine gives reasons for thinking that once we have the conceptual aspect of a reduction its doctrinal aspect is not far behind.

> The two ideals are linked. For, if you define all the concepts by use of some favored set of them, you thereby show how to translate theorems into these favored terms. The clearer these terms are, the likelier it is that truths couched in them will be obviously true, or derivable from obvious truths. If in particular, the concepts of mathematics were all reducible to the clear terms of logic, then all the truths of mathematics would go over into truths of logic; and surely the truths of logic are obvious or at least potentially obvious, i.e., derivable from obvious truths by individually obvious steps.

Quine, of course, despairs of reducing the truths of mathematics to truths of logic, recognizing that the most that can be hoped for is a reduction of mathematics to set theory. Nevertheless, the idea is the same, namely, that the concept of a <u>set</u> is clearer than the concepts of mathematics in general, and that since it is easier to know whether something readily understood is true than whether something complex is true, what we know about sets cannot be less than what we know about mathematics in general. That is, the power or ordinary mathematical proof does not outstrip the power of set theoretic proof.

Quine's argument seems plausible. However, there are good reasons for rejecting it. I already hinted at some before; but there are others. The most obvious and convincing is that although the truths of mathematics may be reduced to set theoretic truths, they are by no means reduced to simple set theoretic truths. If we sit down and try to write out in the language of set theory a simple truth about the real numbers, we soon find that writing out the most common assertions about the real numbers in a first-order language whose only non-logical constant is ε is an enormously difficult task. It might even be impossible to survey the sentence we obtain so that it can, in any sense, be said to be understood. Furthermore, I think it doubtful whether the set theoretic truths needed to deduce truths about the real numbers are in any sense epistemologically preferable to truths about the real numbers. The reason, I think, it so often is said that truths about sets are so clear and understandable is that only very small, finite sets are considered. Yes, we can easily understand what it is to take the power set of a three membered set. We can imagine partitioning the set into its subsets. However, when we start to consider the very large finite sets such visualization becomes impossible. And when the sorts of operations needed in order to construct the real numbers are considered, the claim that set theoretic truths are more obvious than ordinary mathematical statements is hard to defend. Finally, it should be remarked that certain truths about the real numbers can be proved only if we make assumptions that very large cardinals exist. These so-called "large cardinal axioms" are by no means obviously true. Thus, although it is possible to reduce portions of mathematics to set theory, it is by no means clear that the truths so obtained are in any sense more obviously true than the ordinary mathematical truths with which we started. A Quinean argument, then, that the conceptual aspect of a reduction of mathematics to set theory yields the doctrinal aspect is not as cogent as it first appears.

My criticism of Morley's argument can be summed up as follows. In order to use claims (i) and (ii) to conclude that Hilbert's thesis is true, we need not only the conceptual aspect of a reduction of mathematics to set theory, but the dialectical and doctrinal aspects as well. However, these aspects of the reduction are not yet completed enough to warrant our concluding that Hilbert's thesis is true. Numbers, for example, might be reducible to sets without it being the case that every truth of number theory has a first-order derivation from the axioms of ZF. My criticism of Morley's argument, then, is very weak. Morley's argument, I have suggested, rests on the hope that the dialectical and doctrinal aspects of a reduction of mathematics to set theory can be completed. All that I have claimed is that as yet we have no evidence that this is so. It looks, then, as if all we can conclude is that Morley's argument for Hilbert's thesis might not work; I have not ruled out the possibility of the dialectical and doctrinal aspects of a reduction of mathematics to set theory being eventually completed. If they should be completed, it might be claimed, then Morley's argument can be used to establish Hilbert's thesis. It might be a tenet of the mathematician's faith that these aspects of the reduction can be completed, and so Morley's argument demonstrates that it is a tenet of the (consistent) mathematician's faith that Hilbert's thesis is true.

There are, however, what I think are good reasons for denying that Morley's argument can ever give us the sort of evidence we need for Hilbert's thesis. There are compelling reasons for believing that the doctrinal and dialectical aspects of a reduction of mathematics to set theory can never be completed. The method of mathematical proof is open ended; it evolves. The history of mathematics is replete with examples of portions of mathematics changing radically not because of a new proof, but because of a new method of proof. The introduction of forcing by Cohen is a recent, but by no means isolated, example. This open ended character of the method of mathematical proof suggests that the dialectical and doctrinal aspects of the reduction of mathematics to set theory will always be incomplete; we will always have to leave room for altering them, and we can never be sure that a new method of proof might not be introduced, a method which forces us to give up our hope of completing the doctrinal and dialectical aspects of the reduction of mathematics to set theory.

This point can be nicely illustrated by some recent work of Nelson's[28]. He presents a formal system, called "Internal Set Theory", that can be used to formalize Robinson's non-standard analysis. Robinson developed a new method of proof, which greatly simplifies many existing results in analysis, and internal set theory is a theory that formalizes these methods. Nelson's proposal is that ZF be extended as follows. First, the language of set theory is expanded so that it contains a new unary relation sign, S. Then Nelson adds to the axioms fo ZF three new axiom schemata involving the predicate, S.[29] Now, if we believe that Internal Set Theory formalizes the arguments of non-standard analysis, we have to conclude that the dialectical and doctrinal aspects of the reduction of mathematics to set theory cannot be completed. There are some arguments, namely, those of non-standard analysis, that are formalized as sequences of sentences containing the new predicate S and that, therefore, cannot be formalized as derivations from the axioms of ZF. This is not to claim that the conceptual aspect of the reduction of mathematics to set theory cannot be completed. We may, for example, have good reasons for thinking that every theorem of mathematics can be formalized without using S, and Nelson proves that if such a sentence is a theorem of Internal Set Theory, it is a theorem of ZF+Choice. However, we have seen that if Internal Set Theory formalizes the methods of non-standard analysis, the dialectical and doctrinal aspects of the reduction cannot be completed.

This example by no means shows that Hilbert's thesis is false -- only that Morley's argument for Hilbert's thesis is not conclusive. It can be claimed, however, that a new argument, similar to Morley's, can be constructed for Hilbert's thesis.[30] One might argue as follows: (1) Mathematics can be reduced to ZF and Internal Set theory; and (2) Both ZF and Internal Set Theory are first-order theories; therefore (3) Hilbert's thesis is true. The same cricism, mutatis mutandis, I levelled against Morley's argument can, however, be levelled against this new one. The open ended character of the method of mathematical proof suggests that the dialectical and doctrinal aspects of a reduction of mathematics to ZF and Internal Set Theory must be left incomplete. We will always be left with the following question: will the next new method of mathematical proof be formalizable using a first-order theory? To claim that the answer to this auestion is always yes, is, I think, equivalent to claiming that Hilbert's thesis is true. It looks, then, as if Morley's argument cannot be used to establish Hilbert's thesis unless we suppose that Hilbert's thesis is true.

There is another sort of problem with Morley's argument for Hilbert's thesis that can be illustrated by means of the following story. Imagine a mathematician being confronted with sets for the first time. Somehow or other, someone or other explains to him what sorts of items sets are supposed to be. This mathematician then retires to his study, occasionally contemplating these newly confronted objects. Perhaps after a while he begins to think of sets as being precisely the sorts of collections on which (some sort of) mathematical induction can be performed. He accepts, in other words, a version of the axiom of choice and is willing to assent to

(a) Every set can be well ordered.

(Such a mathematician is not too hard to imagine. After all, at first (and still) many set theorists thought that (a) is obviously true.) So far, our mathematician friend has thought only in terms of <u>pure</u> set theory; he never has thought in terms of sets <u>of</u> some sort of thing. He realizes, however, that some use could be made of sets in mathematics if he were to think in terms of sets of integers, sets of real numbers, sets of sets of real numbers,...Imagine further that this mathematician throughout his career has worked extensively with the real numbers, using facts about the continuum constantly. He has, in the course of his work, come to the conclusion that

(b) Every collection of real numbers is Lebesgue measurable

is true, although he has never thought of trying to prove that (b) is true. His new interest in applying set theory leads him to reformulate (b) as

(b') Every set of real numbers is Lebesgue measurable.

So the situation is this: we have a mathematician thinking about pure set theory coming to believe that (a) is true; however, when he applies set theory to areas of mathematics with which he is intimately familiar, he is led to believe that (b') is true.

The problem, of course, is that (a) and (b') are inconsistent. We can prove, using the axioms of ZF, that if every set is well-ordered, there is a non-Lebesgue measurable set of real numbers. Our mathematician, then, seems to have two alternatives: (1) deny that (a) is true; or (2) deny that (b) is true. But, in fact, if we look closer at the reasoning of our imagined mathematician there is another alternative open to him. He can deny that (b) is expressed correctly by (b'). He might conclude that although his thoughts concerning *pure* sets and his thoughts concerning the real numbers are *in toto* correct, his application of pure set theory to the real numbers is ill advised.

The mathematician believing (b) might, furthermore, be led to deny that the collection of real numbers is a set. (b), after all, is in conflict with (a). His beliefs about the real numbers and his beliefs about sets might lead him directly to the belief that, in fact, a set theoretic reduction of real number theory is impossible. In light of (a) and (b) he might deny that any *set* can be identified with the collection of all real numbers. Thus, he would deny that (claim i) is true.[31]

Now, it might be claimed that this mathematician has misunderstood what set theory is all about. Set theory, it can be claimed, is *not* a theory about things in the way that, say, biology is. Set theory only has import insofar as it forms the framework of a foundation of mathematics. We do not, it can be continued, contemplate and study pure sets, discover truths about such sets, and then apply those truths to portions of ordinary mathematics. Rather, the order is reversed. We want to reduce ordinary mathematics to as simple a theory as possible. Experience suggests that such a theory is to be found by looking at theories about sets, and so we reach the conclusion that a reduction of mathematics to a theory about sets is worth having. What truths we accept about sets, then, is a function of what we need to facilitate this reduction. Thus, we accept, for example, large cardinal axioms because they allow us to prove believable things about the real numbers, and not because they are, in some sense, obvious truths about sets. This is, I think, the correct position to adopt regarding set theory. If, in other words, set theoretic truths have any evidence and justification -- and, therefore, any content -- it is only to be found in ordinary mathematics.

This is where our imagined mathematician has gone wrong. He tried to investigate sets (whatever that may mean) independently of the reduction of mathematics to a theory about sets. But it is also, I think, where Morley's argument goes wrong. If it is true that "the intuitive content of set theory is expressible in a set of first-order axioms", then evidence that this is so can only be found in ordinary mathematics. What axioms we accept about sets is determined by what we need in order to reduce mathematics to a theory about sets. But now it begins to look as if the justification for claim (ii) of Morley's argument is that Hilbert's thesis is true; our reason for thinking that mathematics can be reduced to a set theory whose "intuitive content is expressible in a set of first-order axioms" is the belief that Hilbert's thesis is true. Rather than establishing Hilbert's thesis, the premises of Morley's argument presuppose that Hilbert's thesis is true.

(iv)

I want to conclude this chapter with a final argument against Morley's claim (i) that, I believe, can be answered but only in a way that begs the question whether Hilbert's thesis is true. So far all the evidence adduced in favor of the reduction of mathematics to set theory has been from arithmetic and analysis. We have shown how to reconstrue statements about the real numbers as statements about sets. But mathematics is more than analysis and arithmetic. One often reads proofs in, for instance, Category theory that begin with the phrase "Take the category of all sets," or "consider the category of all ordinals." None of these phrases, "category of all sets" or "category of all ordinals" refers to a set, for, as we know, there is no set of all sets and there is no set of all ordinals. This suggests that there is no straightforward manner category theory can be reduced to set theory; the ontology of category theory quickly outruns the size of any set. Category theory is not alone among mathematical disciplines in this respect. Model theorists, who use their techniques to obtain interesting results in algebra, topology and other areas, posit objects whose existence cannot be proved using only the axioms of ZF. Some of the most interesting objects studied by the model theorist are models of ZF itself. If ZF were powerful enough to demonstrate that these objects exist, then, by Gödel's results, ZF would be inconsistent. Thus, ironically, either ZF is inconsistent, or not every object posited by mathematicians and used to obtain interesting and fruitful results can be proved to exist using only the axioms of ZF.

This sort of consideration leaves claim (i) of Morley's argument seeming unattractive. The constructions of Dedekind, Cantor and others provided most of the reasons for asserting claim (i). However, constructions of that sort cannot be generalized so that the objects used in other areas of mathematics can be shown to exist using only the methods of ZF. Thus Morley's argument seems to loose alot of its appeal. We need additional, and sometimes implausible, axioms to handle the methods of model theorists set theoretically. Therefore, one might conclude, set theoretic proof is not a good formalization of informal mathematical proof.

This objection, however, is too fast. Take any fragment of mathematics. A minimal condition that fragment must meet is that it is consistent in the sense that we cannot prove every sentence using just the methods of that fragment. So it can be argued, by completeness, that fragment has a set theoretic model.[32] In other words, it is possible to make all the truths of that fragment true in a universe consisting only of sets. That is to say, (borrowing a phrase from Quine) any fragment of mathematics is only ontically committed to sets. _No consistent mathematical theory needs a universe larger than any set_. In this sense, Morley's argument's claim (i) is vindicated.

However, since we are interested in Hilbert's thesis, this vindication of Morley's argument is unsatisfactory. To salvage claim (i) against the objection that some portions of mathematics seem to posit objects larger than any set, we supposed that the proper formalization of the notion _mathematically provable_ was such that the logic used in mathematics is complete. But what evidence do we have for supposing that the (or a) logic appropriate for formalizing mathematics is complete? Perhaps we should be willing to accept a logic and a notion of proof for which the completeness theorem does not hold. Thus, if Morley's argument is to justify Hilbert's thesis, we must show why the logic we accept as appropriate for mathematics is complete.

In the next chapter I shall look closely at some of the technical notions behind completeness and formal logics. In chapter three I shall look closer at the property of completeness, trying to see why it might be considered a virtue of a logic that it is complete.

Let us conclude this chapter by noting two things. First, in order to establish Hilbert's thesis the conceptual aspect of a reduction of mathematics to a first-order theory is not enough -- we need the dialectical and doctrinal aspects as well. Second, it looks as if evidence that these aspects of the reduction of mathematics to a first-order theory can be completed is only to be found by carefully scrutinizing the methods mathematicians ordinarily employ, and not by analyzing the objects mathematicians posit.

Footnotes for Chapter One:

1. Saunders MacLane and Garrett Birkhoff, Algebra, The Macmillan Company Collier-MacMillan Limited, London (1967). Page 121.

2. Hilary Putnam, Mathematics, Matter and Method, Cambridge University Press, Cambridge (1975). Page 61.

3. Ibid.

4. This is a version of an argument due to Benacerraf. It was first brought to my attention by L. Wetzel.

5. Hao Wang, "On Formalization" in Irving M. Copi and James A. Gould, Contemporary Readings in Logical Theory, The Macmillan Company, New York (1967). Page 29.

6. Ibid., page 37.

7. Ibid.

8. Wittgenstein, for one; Polya for another.

9. M. Steiner, Mathematical Knowledge, Cornell University Press, Ithaca (1975). Page 97.

10. See, for example, Quine's discussion of regimentation in Word and Object, The M.I.T. Press, Cambridge (1960) for the details of one such view.

11. I should mention at this point that I never really explain what it is to formalize adequately a portion of mathematics. It is a puzzling notion and needs explication. In this work, I guess, it has to be treated as a sort of primitive.

12. J. Barwise, "The Realm of First-order Logic" in Barwise (ed.) The Handbook of Mathematical Logic, North-Holland Publishing Company (1977).

13. This line of discussion is suggested in papers by George Boolos.

14. The similarity between this argument and one given by Skolem should be noted. Skolem tried to argue that no formalization of mathematics is adequate since every consistent theory has a countable model and mathematicians often prove results about uncountable structures. The argument in the text uses Skolem's point of view to show that a particular sentence's structure has no first-order analog. Notice, incidentally, how Skolem's argument presupposes Hilbert's thesis.

15. See W.V.O. Quine, Methods of Logic, revised edition, Holt, Rinehart and Winston, New York (1959) page 225 for excellent treatments of the three examples that follow.

16. See. J.J. Katz, "The Dilemma Between Orthodoxy and Identity" Philosophia vol. 5, no. 3 (July 1975) for a criticism of this treatment of identity.

17. In fact, Boolos points out that this is generally the case in mathematics. See "Second-Order Logic", Journal of Philosophy (1975). Also relevant is George Kriesel, "Survey of Proof Theory", Journal of Symbolic Logic.

18. The example is due to G. Boolos. R.L. Cartwright has made a similar point in some unpublished remarks.

19. I must thank G. Boolos and P. Horwich for helping me see the importance of this point to the discussion in the text.

20. L. Wetzel pointed this example out to me. Goodman's definition is a little more complicated than I make it seem in the text.

21. Morley, Introduction to Model Theory.

22. J. Donald Monk, Mathematical Logic, Springer-Verlag, New York (1976) page 172.

23. W.V.O. Quine, "Epistemology Naturalized" in Ontological Relativity and Other Essays, Columbia University Press (1969), pages 69ff.

24. George Boolos and Paul Horwich helped me to see the importance of this point.

25. Notice, I said "no proofs". This does not mean that there is no way to answer these questions, unless we suppose that the only method available to the working mathematician is the method of mathematical proof.

26. Zermelo, "Investigations in the foundations of set theory, I" in Jean van Heijenoort (ed.) From Frege to Gödel: A Source Book in Mathematical Logic, Harvard University Press, Cambridge (1967).

27. Quine, op. cit.

28. E. Nelson, "Internal Set Theory: A New Approach to Non-Standard Analysis", Bull. of Amer. Math. Soc. V.83, number 6, November 1977. G. Boolos pointed this example out to me.

29. The reader is referred to Nelson's article for the details.

30. P. Horwich pointed this out to me.

31. There are, of course, other things our imagined mathematician might do. He might, as Boolos has suggested, claim that not every set of real numbers in a collection, asserting that, say, only π: sets are collections. The point of this example is not, however, to describe the correct way to think about these matters, but only to describe one way of thinking about them.

32. J. Barwise, "The Realm of First-Order Logic", _op_. _cit_.

Chapter 2

SOME FORMALITIES

In chapter 1 I discussed, informally and roughly, Hilbert's thesis and reasons for thinking it true or false. In this chapter, I want to provide a technical framework in which some issues raised in chapter one might be made more precise. I shall outline some rudimentary notions needed to talk rigorously about formalizations of portions of ordinary mathematics.

Logical truths and, correspondingly, logical implications can be characterized informally in two ways. Sometimes it is said that a statement, ψ, is a logical truth if and only if ψ is true under all logically possible circumstances. (The apparent circularity need not trouble us now.) This idea is also put as follows: logical truths are true in virtue of the logical terms occurring in them, although what it is for a truth to be true in virtue of logical terms is often left obscure. This sort of claim is a semantic characterization of logical truth. Sometimes logical truths are characterized differently. It is often noted that logical truths have logical proofs; and so logical truths are characterized as truths that can be proved logically. (Again, the apparent circularity need not concern us.) Benacerraf [1] recently has stressed that the latter characterization of logical truths was presupposed by Frege when he criticized Kant, while Kant himself presupposed the former characterization. The historical roots, however, of these two sorts of characterizations of logical truths need not concern us. It is the subject of another thesis. In this chapter, I shall begin making these two characterizations of logical truths more precise. Let me state them here informally in terms of logical implication for future reference:

> (D1) A set of statements, Γ, logically implies a statement, ψ, if and only if it is logically impossible for all members of Γ to be true while ψ is false.

and

> (D2) A set of statements, Γ, logically implies a statement, ψ, if and only if there is a logical proof of ψ that uses only elements of Γ as premises.

(Again, we may ignore what appears to be a circularity.)

(i)

In this section, I shall review a minimal characterization of logic (or logics). My treatment relies heavily on work by J. Barwise[2] and Monk[2a]. Their work, in turn is an extension and generalization of some results of P. Lindstrom[3]. The ideas are simple and ultimately derive from Tarski's[4] definition of 'truth'. A quote[5] from Barwise's article sums up what will be accomplished:

> A logic is...an operation which assigns to each set L of symbols a syntax and a semantics such that:
>
> (1) elementary syntactical operations (like relativizing and renaming symbols) are performable,
>
> (2) isomorphic structures satisfy the same sentences.

From now on let us assume that we have fixed a countable set of symbols $\mathfrak{U}$ $\mathfrak{U}$ can be presented as the union of three pairwise disjoint sets (of symbols), $\mathcal{V}$, $\mathcal{P}$ and $\mathfrak{R}$, which, in turn, can be described as follows. $\mathcal{V}$, called the set of variables, is a countable union of pairwise disjoint sets, called kinds. Each kind contains countably many elements, called variables. Thus,

$$\mathcal{V} = K_0 \cup K_1 \cup K_2 \cup \ldots,$$

where each K_n is a kind and contains countably many symbols. We also suppose that we have fixed an enumeration of each K_i. Thus, $K_i = \{V_0^i, V_1^i, V_2^i \ldots\}$. The set $\mathcal{P}$, on the other hand, is finite. It has only eight members. They are: $\neg$, the negation sign; v, the disjunction sign; &, the conjunction sign; $\forall$, the universal quantification sign: $\exists$, the existential quantification sign; =, the identity sign; (, the left parenthesis; and), the right parenthesis. Finally, $\mathcal{R}$ can be described as a countable union of pairwise disjoint sets, R_n, called degrees. Each degree, in turn, contains countably many symbols, called relation signs. I assume that, as usual, if P is a relation sign in R_n, then P has n argument places. Thus,

$$\mathcal{R} = R_1 \cup R_2 \cup R_3 \cup \ldots,$$

where for each i, R_i contains a countable infinity of relation signs with i argument places.

In addition, let us suppose that $\mathcal{U}$ can be gödel numbered. More precisely, let us suppose that there is an effective 1:1 function, g, from $\mathcal{U}$ into the set of natural numbers and that $g[\![\mathcal{V}]\!]$ and $g[\![\mathcal{R}]\!]$ are recursive sets. Notice that since $\mathcal{P}$ is a finite set, $g[\![\mathcal{U}]\!]$ is, therefore, recursive as well. Furthermore, for reasons that will become clear below, let us suppose that the complement of the range of g (that is, $\omega - g[\![\mathcal{U}]\!]$) contains an infinite recursive set. There are many such gödel numberings that we might use. Let us suppose that one has been fixed.

With these assumptions behind us, we are now ready to make the following definition:

DEFINITION 1: A language, L, is a subset of $\mathcal{R} \cup \mathcal{P}$ such that $\mathcal{P} \subset L$ and $g[\![L]\!]$ is recursive.

Before continuing I should make some remarks about definition 1. First, in this chapter, and the remainder of this dissertaion, when I use 'language' I shall be using it in the sense of definition 1, unless the context makes perfectly clear either (i) how definition 1 should be amended, or (ii) that I am talking about a natural language, like Italian. Second, all languages, in the sense of definition 1 are subsets of $\mathcal{U}$, and so no language has more than a countable infinity of symbols. In fact, we do not have available in all the languages, in the sense of definition 1, more than ω symbols. This will, for our purposes, surely be enough. However, for some purposes it is not enough. For example, model theorists use and need names for every real number. This brings us to a third point about definition 1. No language contains any operation signs or constant signs. This is an unusual stipulation to make; however, it will make the formal results I want to report in this chapter easier to state. In addition, as shall be seen, in light of further assumptions and definitions, requiring that languages contain no operation and constant symbols does not involve a genuine loss of generality. Finally, according to definition 1, every relation sign in a language is of fixed degree. It has recently been proposed that logic be extended so that relation signs with varying degrees may be used.[6] Although this proposal is certainly deserving of serious attention, I shall not attend to it here. So far many interesting portions of mathematics have been formalized using only relation signs of fixed degrees, and since these sorts of relation signs are standard, I shall insist that only such relation signs are members of languages.

DEFINITION 2: A logical language, L, is the union of a language and finitely many kinds (see above) such that if $K_m \subset L$ and $n<m$, then $K_n \subset L$.

We shall say that a logical language, L, is <u>of kind</u> m, where m is a natural number, provided that m is the greatest number for which $K_m \subset L$. Notice what a logical language, in the sense of definition 2, is: a logical language is a countable set of relation signs along with the eight (special) symbols in P and a countably infinite stock of variables of finitely many different kinds. If the notion is not clear, it will, I think, become so after we consider some examples. But first, we need another definition.

DEFINITION 3: A logical syntax, *, is an operation on logical languages of some fixed kind, m, such that, where L is a logical language of kind m, L* is a set of sequences of members of L (L* is the set of wffs whose non-logical symbols are members of L) and such that:

(i) If φ is a member of L*, there is a logical language, L(φ), of kind m containing only finitely many relation signs, and for every logical language, K, of kind m, $\varphi \in K^*$ if and only if $L(\varphi) \subset K$;

(ii) If L and K are logical languages of kind m, and $L \subset K$, then $L^* \subset K^*$;

(iii) If K is a logical language of kind m, x is a variable in K, and φ, ψ are members of K*, then

(a) $(\neg\varphi)$
(b) $(\varphi\vee\psi)$
(c) $(\varphi\&\psi)$
(d) $(\forall x)(\varphi)$
(e) $(\exists x)(\varphi)$

and all members of K*;

(iv) For every logical language, K, of kind m there is a function, g^{+K}, from K* into the natural numbers that is an extension of g and that is such that $g^{+K}[\![K^*]\!]$ is a recursive set[5a];

(v) For every logical language, K, of kind m, there is a recursive function, $comp^K$, from $g^{+K}[\![K^*]\!]$ into $g^{+K}[\![K^*]\!]$ such that for all φ in K*, $comp^{+K}(g^{+K}(\varphi))=g^{+K}((\neg\varphi))$;

(vi) There is a binary recursive function, un^K, for every language, K, of kind m, such that $un^K(g^{+K}(\varphi), g^{+K}(\psi))=g^{+K}(\varphi\vee\psi)$, for all φ, ψ in K*.

Definitions 2 and 3 capture some of (what we think are) the essential features of (almost all) formal languages, in the sense in which we talk of a formal language that corresponds to the predicate calculus, or the sense in which we call Frege's concept-script a formal language. Almost all such formal languages that have been used successfully to formalize portions of mathematics can be presented so that they are the result of applying a logical syntax to a logical language. The exceptions all, I think, violate clause (i) of definition 3. That clause requires that each wff contains only finitely many non-logical constants (although it does not require that each wff contains only finitely many occurrences of non-logical constants, nor does it require that each wff contains only finitely many logical constants). There are some versions of formal languages permitting the formation of infinite conjunctions and disjunctions that allow wffs to contain infinitely many non-logical constants; these formal languages are often of interest, even though they violate clause (i). However, for our purposes, such formal languages can be ignored; the formal languages that can be presented so that they are generated by applying a logical syntax to a logical language are rich and diverse enough (as the examples we will consider show) to justify including clause (i) in definition 3. Clauses (iv)-(vi) of definition 3 insure that every formal language generated by the application of a logical syntax to a logical language has a gödel numbering. Since Gödel's work, it has become nearly impossible to imagine a formal language that cannot be gödel numbered. Proof theoretic studies of logic constantly appeal to gödel numberings, and the richness of these studies justifies clauses (iv)-(vi). The best way, however, to understand the motivation for definitions 2 and 3 is to look at an example.

THE LOGICAL SYNTAX FOR FIRST-ORDER LOGIC, $*_{fo}$, is an operation on the set of all logical languages of kind 0. Given a logical language, L, of kind 0, the result of applying $*_{fo}$ to L (called $\underline{L^{fo}}$) is the smallest set satisfying:

(i) If R is an n-place relation sign in L, and v_0, $v_1 \ldots, v_n$ are in K_0 (that is, they are variables of kind 0), then the result of concatenating R, v_0, $v_1 \ldots$and v_n in that order is in L^{fo}, and $(v_0=v_1)$ is in L^{fo};

(ii) If θ is in L^{fo}, then $(\neg\theta)$ is in L^{fo};

(iii) If θ is in L^{fo} and x is in K_0, then $(\exists x)(\theta)$ and $(\forall x)(\theta)$ are in L^{fo};

(iv) If θ and γ are in L^{fo}, then $(\theta\vee\gamma)$ and $(\theta\&\gamma)$ are in L^{fo}.

There are many ways to extend g so that we have a gödel numbering of the formulas of first-order logic satisfying clauses (vi)-(vi) of definition 3 (see page 55). So that things can be said more concisely, I shall make two conventions. First, where * is a logical syntax, I shall let "kd(*)" denote the kind of the logical languages in the domain of *. Thus, $kd(*_{fo})$ is 0. Also, if L is a language (not a logical language) and * is a syntax, I shall write "L*" to denote the result of applying * to the smallest logical language, K, of kd(*) such that $K \supset L$.

The definition that follows is standard and known to all students of logic.

DEFINITION 4: Let L be a language. An L-structure, $\mathfrak{a}$, is a partial function on L such that $\forall$ (written instead of '$\mathfrak{a}(\forall)$') is a non-empty set, called the universe of $\mathfrak{a}$, and such that for every n-place relation sign, P, in L, $P^{\mathfrak{a}}$ is a set of n-tuples of members of the universe of $\mathfrak{a}$.

For convenience, when K is a logical language, I shall use 'K-structure' to mean the same as 'L-structure, where L is the largest language contained in K'.

A 1:1 function, f, from a language, L, onto another language, L', is called <u>an interpretation of</u> L <u>in</u> L', if for every relation sign, P, in L, f(P) is a relation sign in L' with the same number of argument places as P, and if f is the identity function on the set $\mathbf{P}$. If f is an interpretation of L in K and if $\mathfrak{a}$ is an L-structure, then $\mathfrak{a}_f$ is the K-structure such that (i) the universe of $\mathfrak{a}_f$ is the same as the universe of $\mathfrak{a}$, and (ii) for every relation symbol, P, in K, $P^{\mathfrak{a}_f} = (f^{-1}P)^{\mathfrak{a}}$. Notice that if f is an interpretation of L in K, there is a natural extension of f into a function f^+ from $L \cup L^*$ onto $K \cup K^*$, where * is a logical syntax. If φ is in L^*, I will write 'φ^f' to denote $f^+(\varphi)$. φ^f is, roughly, the result of substituting f(R) in φ, for each relation sign R occurring in φ. Thus if φ is $(\forall x)(Rx \to Fxy)$ and f(R) is F while f(F) is W, then φ^f is $(\forall x)(Gx \to Wxy)$. We are now ready for the first in a series leading to the main definition of this chapter.

DEFINITION 5: A Barwise logic is an ordered pair, $\langle *, \models \rangle$, where * is a logical syntax and $\models$ is a relation between structures and sentences such that

(i) For all languages, L, if φ is in L^* and $\mathfrak{A}$ is an L-structure with $\mathfrak{A} \models \varphi$, then if $\mathfrak{B}$ is an L-structure that is isomorphic to $\mathfrak{A}$, $\mathfrak{B} \models \varphi$;

(ii)(a) Let L and K be languages. Let f be an interpretation of L in K. Then $\{\mathfrak{A} \mid \mathfrak{A} \models \varphi\}$ is the same as $\{\mathfrak{A} \mid \mathfrak{A}_f \models \varphi^f\}$;

(b) For all languages, L, $\mathfrak{A} \models \varphi$, where φ is in L^*, if and only if $\mathfrak{A} \restriction L(\varphi) \models \varphi$;

(c) Let L be a language, and let 'S_L' denote the class of all L-structures. An $L, \langle *, \models \rangle$-e.c. is a subset of S_L, A, such that for some φ in L^*, $\mathfrak{A}$ is in A if and only if $\mathfrak{A} \models \varphi$. Then if K is a language and A is a $K, \langle *, \models \rangle$-e.c., then if $K' \supset K$, $\{\mathfrak{A} \in S_{K'} \mid \mathfrak{A} \restriction K \text{ is in } A\}$ is a $K', \langle *, \models \rangle$-e.c.;

(iii) Let L be a language. Then for every φ in L^*, $\{\mathfrak{A} \mid \mathfrak{A} \models \varphi\}$ is the same as $S_L - \{\mathfrak{A} \mid \mathfrak{A} \models \neg\varphi\}$;

(iv) For all languages, L, if φ and ψ are in L^*,

$\{\mathfrak{A} \mid \mathfrak{A} \models \varphi\} \cap \{\mathfrak{A} \mid \mathfrak{A} \models \psi\}$ is $\{\mathfrak{A} \mid \mathfrak{A} \models \varphi \& \psi\}$, and

$\{\mathfrak{A} \mid \mathfrak{A} \models \varphi\} \cup \{\mathfrak{A} \mid \mathfrak{A} \models \psi\}$ is $\{\mathfrak{A} \mid \mathfrak{A} \models \varphi \vee \psi\}$.

Let $\langle *, \models \rangle$ be a Barwise logic. Then to simplify our talk, "$\mathrm{Mod}_{L, \langle *, \models \rangle}(\varphi)$" will denote the class of L-structures, $\mathfrak{A}$, for which $\mathfrak{A} \models \varphi$. Usually mention of L will be suppressed. A $\langle *, \models \rangle$-sentence is a sentence, φ, for which there is a language L, such that φ is in L*. The example that motivates the definition of a Barwise logic, as we shall soon see, is the first-order predicate calculus. Before discussion that example, however, let us look at two more definitions.

DEFINITION 6: Let $<*, \models>$ be a Barwise logic, and let L be a logical language of kd(*). Then a sentence, φ, of L* is L, $<*, \models>$-valid if for all L-structures, $\mathfrak{A}$, $\mathfrak{A} \models \varphi$.

DEFINITION 7: Let $<*, \models>$ be a Barwise logic and let L be a logical language of kd (*). Also, let Γ be a subset of L* and let φ be a member of L*. Then Γ L, $<*, \models>$-implies φ if for all L-structures, $\mathfrak{A}$, if $\mathfrak{A} \models \gamma$, for every γ in Γ, then $\mathfrak{A} \models \varphi$.

Usually when using the notions defined in 6 and 7 I shall suppress mention of L and talk about, say, $<*, \models>$-valid sentences. The context will make clear what language is involved, and when it does not, we can suppose, using clause (i) of definition 3, that the language in question is the smallest language from which the sentence(s) mentioned can be generated. Definitions 6 and 7 make clear the motivation behind clause (ii)(a)-(c) of our definition of a Barwise logic. It is usual to suppose that logical implication and logical validity are determined by the logical structure(s) of the sentence(s) in question. Clause (ii) guarantees that the implications and validities determined by a Barwise logic depend only on logical structure, and not on the relation signs involved. We can uniformly replace relation signs, by clause (ii) of definition 5, without making a valid sentence invalid (or _vice versa_) and without altering relations of implication. It is this, I think, that motivates clause (ii). Clause (i) of definition 5, I think, needs no motivation. It is hard to imagine wanting isomorphic structures not to satisfy the same sentences. I shall soon discuss the motivations for clauses (iii) and (iv). For now, we can conclude that defintion 7 is the formal analog of (D1).

The best way to understand defintion 5 is, of course, to look at an example.

> FIRST-ORDER LOGIC, $\mathcal{L}_{fo}$, can be presented as a Barwise logic whose first component is $*_{fo}$, and whose second component is a relation, $\models_{fo}$, defined as follows: Let L be a logical language of kind 0, and let $\mathfrak{a}$ be an L-structure. Then inductively define a three place relation, $\Vdash$, between $\mathfrak{a}$, members of L* and elements of $(V^{\mathfrak{a}})^{\omega}$ (the set of ω-tuples on $V^{\mathfrak{a}}$) using the following clauses:
>
> (i) $\mathfrak{a} \Vdash (v_i = v_j)$ [x] iff the i-th component, $(x)_i$, of x is the same as the j-th, $(x)_j$, component of x;
>
> (ii) If R is an n-place relation sign of L, then $\mathfrak{a} \Vdash (Rv_{i_1} \ldots v_{i_n}($[x] iff $((x)_{i_1}, \ldots, (x)_{i_n})$ is in R^a;
>
> (iii) If φ and ψ are in L*, then
> $\mathfrak{a} \Vdash (\varphi \& \psi)[x]$ iff $\mathfrak{a} \Vdash \varphi[x]$ and $\mathfrak{a} \Vdash \psi[x]$; and
> $\mathfrak{a} \Vdash (\varphi \vee \psi)[x]$ iff $\mathfrak{a} \Vdash \varphi[x]$ or $\mathfrak{a} \Vdash \psi[x]$;
>
> (iv) If φ is in L*, then
> $\mathfrak{a} \Vdash \neg\varphi[x]$ iff not: $\mathfrak{a} \Vdash \varphi[x]$;
> $\mathfrak{a} \Vdash (\forall v_i)\ (\varphi)[x]$ iff for all y that result from x by replacing the i-th component of x with an element of $V^{\mathfrak{a}}$, $\mathfrak{a} \Vdash \varphi[y]$;
>
> (The existential quantifier can be handled similarly.)
>
> Then for φ in L*, say $\mathfrak{a} \models_{fo} \varphi$ if and only if $\mathfrak{a} \Vdash \varphi$ [x] for all x in $(V^{\mathfrak{a}})^{\omega}$.

Therefore, we have $\mathcal{L}_{fo}=\langle *_{fo}, \models_{fo}\rangle$.[8]

The reader probably has noticed that every Barwise logic validates the law of excluded middle. That is to say

> Proposition: Let $\mathcal{L}$ be a Barwise logic and φ a sentence generated using the syntax of $\mathcal{L}$. Then φ is $\mathcal{L}$-valid

is true. The proposition follows easily from clauses (iii) and (iv) of definition 5 and from clause (iii)(a) and (b) of definition 3. It might be objected, then, that the definition of a Barwise logic ignores the intuitionists' objections to the law of excluded middle. Intuitionistic Precicate Calculus, as it turns out, is not a Barwise logic. Since this dissertation ultimately is an investigation of some issues concerning the question which formal logic is appropriately used when formalizing mathematics, the definition of a Barwise logic seems too exclusive. We are unable to say that a Barwise logic can be used to formalize adequately mathematics without ignoring the intuitionists' objections.

In defense of definition 5 it can only be said that in this dissertation I am not interested in examining the intuitionists' objections to classical logic, nor am I interested in examining any objections to Hilbert's thesis based on the claim that first-order logic is too strong and that many arguments that seem formalizable as derivations of first-order logic are not valid. I am only, at least in this work, concerned with objections to Hilbert's thesis from above, that is, objections to Hilbert's thesis based on the claim that a logic stronger than classical first-order logic is needed in order to formalize adequately some portion of mathematics. Now, I admit that this excludes from consideration a vast amount of philosophical work that is both interesting and important. We should notice, however, that Hilbert's thesis claims that first-order logic <u>can</u> be used to formalize adequately mathematical argumentation. Now, it might turn out to be the case that only a proper part of first-order logic is needed to carry out such a task. If this does turn out to be the case we cannot (necessarily) argue that Hilbert's thesis is false; it might be that Hilbert's thesis is not wrong, just not strong enough. A more interesting claim about what logic is appropriately used to formalize mathematical argumentation might be true, even though Hilbert's thesis is not false.

The following question can, therefore, be asked[9]: why not just consider logics that contain first-order logic? This question motivates the following definitions.

DEFINITION 8: Let $\mathcal{L}$ and $\mathcal{L}'$ be two Barwise logics. $\mathcal{L}$ is included in $\mathcal{L}'$ ($\mathcal{L}\subset\mathcal{L}'$) provided every $\mathcal{L}$-e.c. is an $\mathcal{L}'$-e.c.; $\mathcal{L}$ and $\mathcal{L}'$ are equivalent ($\mathcal{L}\equiv\mathcal{L}'$) provided $\mathcal{L}\subset\mathcal{L}'$ and $\mathcal{L}'\subset\mathcal{L}$.

DEFINITION 9: A Lindstrom logic is a Barwise logic, $\langle *,\models\rangle$, for which the following hold:

(i) $\mathcal{L}_{fo}\subseteq\langle *,\models\rangle$;

(ii) If h is the function associated with * (see definition 3 clause (iv)), then there is a unary recursive function, T, such that for any language, L, and any L, f.o.-sentence, φ, there is a L,$\langle *,\models\rangle$-sentence, θ, such that $T(g^{+L}_{fo}(\varphi))=h^{+L}(\theta)$ and $\mathrm{Mod}_{fo}(\varphi)=\mathrm{Mod}\,\langle *,\models\rangle^{(\theta)}$.

Clause (i) of definition 9 is motivated by the question mentioned above. Since in this work only objections to Hilbert's thesis from above will be considered, why not build first-order logic into our notion of a formal logic? This will make the formal result to be described in this chapter easier to state. The second clause of definition 9 may seem a little restrictive. It guarantees that for any Lindstrom logic we have an effective way to find, for given first-order sentences, a formula in that Lindstrom logic with the same formal truth-conditions. This requirement is not so strong as it seems, however. For almost all the Lindstrom logics we consider, T will be the identity function. Definition 9 is the most important definition of this chapter. From now on, whenever I talk about foramlizations of logic, I shall be talking about Lindstrom logics.

Taking Lindstrom logics to be formalizations of logic has an important benefit. I already noted that definition 1 seems overly restrictive; it entails that languages contain no operation or constant signs. This involves no loss of generality, however, if we consider only Lindstrom logics. There are well-known ways that first-order logic can be used to eliminate operation and constant signs[10]. Since every Lindstrom logic contains first-order logic, these methods can always be used. Thus, weakening definition 1 so that languages may contain constant and operation signs, in the context with which we are concerned, does not have any (important) consequences. There are, of course, some (interesting) Barwise logics that do not contain first-order logic and that therefore are not Lindstrom logics; we shall look at one in chapter 4. We shall, however, take Lindstrom logics to be our candidates for formalizations of logic. As the examples that follow show, this is not a very restrictive step; a good many formal logics can be presented as Lindstrom logics.

WEAK SECOND-ORDER LOGIC, $\langle *_{WS}, \models_{WS} \rangle = \mathcal{L}_{WS}$, is a Lindstrom logic whose first component is an operation defined on logical languages of kind 1. $*_{WS}$ agrees with $*_{fo}$ on that part of a language of kind 1 that is of kind 0. In addition, where L is a language of kind 1, $*_{WS}$ must satisfy:

(i) $v_i^0 \varepsilon v_j^1$ is in L^{WS} for all v_i^0 in K_0 and v_j^1 in K_1; and $v_i^e = v_j^f$, for $e = 1$ or 0 and $f = 1$ or 0;

(ii) $(\exists v_i^0)(\theta)$, $(\forall v_i^0)(\theta)$, $(\exists v_i^1)(\theta)$, $(\forall v_i^1)(\theta)$ and $(\neg\theta)$ are in L^{WS} whenever θ is in L^{WS};

(iii) $(\theta \& \varphi)$ and $(\theta \vee \varphi)$ are in L^{WS}, whenever θ and φ are in L^{WS}.

$\models_{WS}$ is defined very similarly to $\models_{fo}$. Let L be a logical language of kind 1 and let $\mathfrak{a}$ be an L-structure. Then inductively define a four-place relation, $\Vdash$, between $\mathfrak{a}$, members of L^{WS}, elements of $(V^{\mathfrak{a}})^{\omega}$, and elements of $(V^{\mathfrak{a}})^{\omega}$, using the following clauses:

(i) $\mathfrak{a} \Vdash (v_i^e = v_j^c)[x][y]$ iff either

(a) $e = c = 0$ and $(x)_i$ is the same as $(x)_j$; or

(b) $e = c = 1$ and $(y)_i$ is the same as $(y)_j$; or

(c) $e = 0$ and $c = 1$ and $(x)_i$ is the same as $(y)_j$; or

(d) $e = 1$ and $c = 0$ and $(x)_j$ is the same as $(y)_i$;

(ii) $\mathfrak{a} \Vdash (v_i^0 \varepsilon v_j^1)[x][y]$ iff $(x)_i$ is a member of $(y)_j$;

(iii) $\mathfrak{A} \Vdash Rv_i^0 \ldots v_i^0[x][y]$ iff $<(x)_i, \ldots, (x)_i>$ is in $R^{\mathfrak{A}}$;

(iv) If φ and ψ are in L^{WS}, then

$\mathfrak{A} \Vdash (\varphi \& \psi)[x][y]$ iff $\mathfrak{A} \Vdash \varphi[x][y]$ and $\Vdash \psi[x][y]$; and

$\mathfrak{A} \Vdash (\varphi \vee \psi)[x][y]$ iff $\mathfrak{A} \Vdash \varphi[x][y]$ or $\mathfrak{A} \Vdash \psi[x][y]$;

(v) If θ is in L^{WS}, then

$\mathfrak{A} \Vdash \neg\theta[x][y]$ iff not: $\mathfrak{A} \Vdash \theta[x][y]$;

$\mathfrak{A} \Vdash (\forall v_i^0)(\theta)[x][y]$ iff for all z that result from x by replacing i-th component of x with an element of V, $\mathfrak{A} \Vdash \theta\ [z][y]$;

$\mathfrak{A} \Vdash (\exists v_i^0)(\theta)[x][y]$ iff for some z that results from x by replacing the i-th component of x with an element of V, $\mathfrak{A} \Vdash \theta[z][y]$;

$\mathfrak{A} \Vdash (\forall v_i^1)(\theta)[x][y]$ iff for all z that result from y by replacing the i-th component of y with a finite element of $PV^{\mathfrak{A}}$ (where PX is the set of subsets of X), $\mathfrak{A} \Vdash \theta[x][z]$; and

$\mathfrak{A} \Vdash (\exists v_i^1)(\theta)[x][y]$ iff for some z that results from y by replacing the i-th component of y with a finite element of PV, $\mathfrak{A} \Vdash \theta[x][z]$.

Then, for γ in L^{WS}, say that $\mathfrak{A} \models_{WS} \gamma$ iff $\mathfrak{A} \Vdash \gamma[x][y]$ for all x in $(V^{\mathfrak{A}})^{\omega}$ and y in $(PV^{\mathfrak{A}})^{\omega}$.

I have presented weak second-order logic in such excruciating detail so that we might have two worked out versions of Lindstrom logics, and so that definition 9 might look more plausible, palatable and natural. In the future most of the details will be suppressed. Notice that $\mathcal{L}_{WS}$ meets the conditions of definition 9. $\mathcal{L}_{fo}$ certainly is contained in $\mathcal{L}_{WS}$, and we can take T to be the identity function. The reader, no doubt, has noticed that variables of kind 1 are not permitted in the argument places of relation signs (Note: ε, strictly speaking is <u>not</u> a relation sign; rather it is a logical constant on equal footing with =). Thus, strings like

(1) $(\exists v_i^1)(\forall v_j^0)(v_j^0 \epsilon v_i^1 \ \& \ Pv_i^1)$

are not sentences of $\mathfrak{L}_{WS}$. At first, this seems disturbing, since (1) is the sort of string we might have thought formalizes

> (2) There is a finite set that contains everything and it is big,

a sentence that should be formalizable using weak second-order logic. Judicious manipulation of the identity sign, however, allows us to overcome this and any similar problem.

(3) $(\exists v_i^1)(\forall v_j^0)(v_j^0 \epsilon v_i^1 \ \& \ (\forall v_h^0)(v_h^0 = v_i^1 \rightarrow Pv_h^0))$

can be used in all places where we might want to use (1). (Notice: the last conjunct in the matrix of (3) is really an abbreviation for a formula of $\mathfrak{L}_{WS}$; it can be expanded in the usual way.)

> SECOND-ORDER LOGIC, $\mathfrak{L}_{SO}$, is a Lindstrom logic whose first component, $*_{SO}$, is the same as $*_{WS}$ and whose second component is defined just as $\models_{WS}$ was defined except that the two occurrences of 'finite' are deleted from clause (v).

> $\aleph$-LOGIC, $\mathfrak{L}_{\aleph}$, where $\aleph$ is a cardinal is defined just as second-order logic was defined except that the last two clauses of clause (v) are replaced by
> $\mathfrak{A} \models (\forall_i^1)(\theta)[x][y]$ iff for all z that result from y by replacing the i-th component of y with an element of $V^{\mathfrak{A}}$ whose cardinality is $\aleph$, $\mathfrak{A} \models \theta[x][z]$;
> and

$\mathfrak{A} \Vdash (\exists v_i^1)(\theta)[x][y]$ iff for some z that results from y by replacing the i-th component of y with ent of $V^{\mathfrak{A}}$ whose cardinality is $\aleph$, $\mathfrak{A} \Vdash \theta[x][z]$.

INFINITARY LOGIC, $\mathcal{L}_I$, is a Linc. . .n logic, $<*_I, \models_I>$, where $*_I$ is defined as $*_{fo}$ except that this time if Γ is an infinite set of f.o.-sentences for which there is a finite language, L, such that Γ is a subset of L^{fo} then $\wedge\Gamma$ and $\vee\Gamma$ are $\mathcal{L}_I$-sentences. The definition of $\models_I$ is similar to that of $\models_{fo}$, but in addition we also have:

$\mathfrak{A} \models_I \vee\Gamma$ iff for some γ in Γ, $\mathfrak{A} \models_{fo} \gamma$; and

$\mathfrak{A} \models_I \wedge\Gamma$ iff for all γ in Γ, $\mathfrak{A} \models_{fo} \gamma$.

Many of the logics standardly proposed as (more powerful) alternatives to first-order logic are Lindstrom logics. As already noted, though, not every logic is a Lindstrom logic. Type theories, for example, cannot be presented as Lindstrom logics; the definition of a structure has to be amended in order to handle type theories. Modal logics, also, are not Lindstrom logics. These two sorts of logics play so important a role in the philosophy of logic that excluding them from consideration seems to be an egregious omission. Perhaps it is. We have to start somewhere, though; and since so many logics standardly proposed as extensions of first-order logic are Lindstrom logics, I shall assume that mathematics can be adequately formalized using a Lindstrom logic. In light of the wide range of logics that are Lindstrom logics, this does not seem like such a dangerous supposition.

In chapter one I spoke alot about formalizations or portions of mathematics. In light of the above remarks, I can now make more precise what I mean by a formalization of a portion of mathematics.

Traditionally, mathematical theories are thought of as determined by a set of axioms along with a logic telling us when a set of sentences logically implies another sentence. Thus, Euclidean geometry can be thought of as the collection of truths that logically follow from five specified axioms. Similarly, set theory is taken to be determined by first-order logic and a set of axioms. As Morley put it: the intuitive content of set theory is expressible in a set of first-order axioms (see chapter one, section three). In light of this tradition and the remarks in the preceeding paragraph, the following definition suggests itself:

> DEFINITION 10: A formalization of a portion of mathematics is an ordered triple, $\langle L,\Gamma,\mu\rangle$, where L is a logical language, μ is a Lindstrom logic whose syntax is on languages of the same kind as L, and Γ is a set of L,μ-sentences.

Thus, for example, a formalization of set theory is an ordered triple the first-component of which is $P\cup\{\varepsilon\}\cup K_o$ (note: this ε is not the same symbol used in the definition of $\pounds_{WS}$, $\pounds_{SO}$ and $\pounds_{\aleph}$), the second component of which is the set of axioms for ZF, and the third component of which is $\pounds_{fo}$. If $\langle L,\Gamma,\mu\rangle$ is a formalization of a portion of mathematics and γ is an L,μ-sentence, then we will say that γ is $\langle L,\Gamma,\mu\rangle$-valid ($VAL_{\langle L,\Gamma,\mu\rangle}(\gamma)$) if and only if Γ μ-implies γ.

(ii)

Definition 7 is our rigorous version of (D1); in this section I shall formulate a rigorous version of (D2). In an introductory logic class, when students are shown how to prove that a first-order sentence is valid, one of two sorts of methods is learned. The first, deduction, involves learning how to derive valid sentences from a specified set of sentences, called "logical axioms". The second involves learning how to apply operations to sentences and to recognize which outcomes are the result of applying operations to valid sentences. A standard example of the second sort of method is the Smullyan-Beth tree construction. The reader unfamiliar with it is referred to Smullyan's First-Order Logic[11]. The general account of logical proof I shall present in this section covers both sorts of methods; however, when formulating that account it is useful to think in terms of the Smullyan-Beth tree construction.

What are the characteristics of the Smullyan-Beth tree construction that any such method of logical proof has? First, there is a set of operations, Ω, and a set of outcomes, Π, such that for every first-order valid sentence, ψ, there are operations in Ω such that the result of applying those operations to ψ is in Π. The following definition is, I think, suggested.

DEFINITION 11: A proof-procedure is an ordered triple, $<*,\Omega,\Pi>$, where $*$ is a logical syntax and

(i) Ω is a set of (codes for) partial recursive functions; and

(ii) Ω and Π are recursively enumerable sets.

Clause (i) of definition 11 can be motivated as follows. The elements of Ω are supposed to be (codes for) the operations that we apply to (codes for) *-sentences. We would like these to be operations that anyone can perform by simply following instructions (not necessarily by following simple instructions), and we want these operations to be repeatable and deterministic without any appeal to luck. In short, the operations applied to (codes for) *-sentences should be effective. By Church's thesis[12], then, we have clause (i). Clause (ii) also is motivated, in part, by Church's thesis. We want there to be an effective method for recognizing the operations that may be applied to *-sentences, and we want an effective method for recognizing when an outcome demonstrates that a *-sentence is valid. The comments above definition 11 along with Church's thesis thereby motivate clause (ii).

When $<*,\Omega,\Pi>$ is a proof-procedure, I shall write "$o[\psi]$"-for *-sentences, ψ, and o in Ω -- to denote the result of applying the function whose code is o to the gödel number of ψ, that is $o[\psi]$ is $\{o\}\ (g^{+L(\psi)}(\psi))$, where $g^{+L(\psi)}$ is as in definition 3 clause (v) and $\{o\}$ is the function whose code is o. Also, I shall say that a *-sentence, ψ, is $<*,\Omega,\Pi>$ -provable if and only if the is an o in Ω such that $o[\psi]$ is in Π.

DEFINITION 12: Let $<*,\Omega,\Pi>$ be a proof-procedure. Suppose that Γ is a set of *-sentences. Then a Γ, $<*,\Omega,\Pi>$-validating conditional of ψ is a $<*,\Omega,\Pi>$-provable conditional, $\gamma_1\to\gamma_2\to\ldots\to\gamma_n\to\psi$ with ψ as consequent and each γ_i a member of Γ.

DEFINITION 13: Let $<*,\Omega,\Pi>$ be a proof-procedure. Then a set of *-sentences, Γ, $<*,\Omega,\Pi>$-implies a *-sentence, ψ, if and only if there is a Γ, $<*,\Omega,\Pi>$-validating conditional of ψ.

Definition 13 is our rigorous version of (D2).

Proof-procedures and Lindstrom logics need not correspond to one another. The following two definitions, however, cover the cases when (happily) they do.

DEFINITION 14: A Lindstrom logic $<*, \models>$ is $<*, \Omega, \Pi>$-complete if and only if for every logical language, L of Kd(*), every sentence, ψ in L* and every Γ that is a subset of L*, Γ $<*, \models>$-implies ψ only if Γ $<*, \Omega, \Pi>$ -implies ψ.

DEFINITION 15: A proof-procedure, $<*, \Omega, \Pi>$, is is $<*, \models>$-sound, where $<*, \models>$ is a Lindstrom logic, if and only if for every logical language of kd(*), every sentence, ψ, in L* and every subset, Γ, of L*, Γ $<*, \Omega, \Pi>$-implies ψ only if Γ $<*, \models>$-implies ψ.

Definitions 14 and 15 are generalizations of well-known logical properties. So is the definition that follows:

DEFINITION 16: A Lindstrom logic, $\mathcal{L}$, has the Löwenheim property if and only if every $\mathcal{L}$-e.c. contains a countable structure. (see definition 5 clause (ii)b)

We can now state Lindstrom's characterization of $\mathcal{L}_{fo}$.

Theorem (Lindstrom): $\mathcal{L}_{fo}$ is the only Lindstrom logic, $\mathcal{L}$, (up to equivalence) that has the Löwenheim property and for which there is a proof-procedure, ρ, such that $\mathcal{L}$ is ρ-complete.

A proof of Lindstrom's theorem can be found in Monk's Mathematical Logic[13]. It should be noted that Lindstrom's theorem cannot be weakened. Every $\mathcal{L}_{WS}$-e.c. contains a countable structure; but $_{WS}$ is ρ-complete for no proof-procedure, ρ. (One consequence of a Lindstrom logic's being ρ-complete is that the (gödel numbers of) $\pounds$-valid sentences are recursively enumerable. It is well-known that $\pounds_{WS}$-valid sentences are not recursively enumerable.[14]) On the other hand, consider the Lindstrom logic, $\pounds_Q$, whose definition can be obtained from the definition of $\pounds_{WS}$ by substituting 'uncountable' for 'finite' in the last two conditions in clause (v). $\pounds_Q$ obviously does not have the Löwenheim property. Nevertheless, there is a proof-procedure, ρ, such that $\pounds_Q$ is ρ-complete[15].

There are two more definitions, whose motivations are clear, that will be useful to us in the future.

DEFINITION 17: A proof system for a portion of mathematics is an ordered triple, $<L,\Gamma,\rho>$, where L is a language, ρ is a proof-procedure whose syntax is $*$, L is of kd($*$), Γ is a subset of L^* and the godel numbers of members of Γ are recursively enumerable.

DEFINITION 18: Let $<L,\Gamma,\rho>$ be a proof system for a portion of mathematics. A $<L,\Gamma,\rho>$-derivation of a sentence, ψ, is a finite sequence $<\gamma_1,\ldots,\gamma_m>$, where γ_m is ψ and for all $n \leq m$, either

(i) γ_n is in Γ; or

(ii) there is a $\{\gamma_k | k<n\}, \rho$-validating conditional of γ_n.

If ψ has a $<L,\Gamma,\rho>$-derivation we say that it is $<L,\Gamma,\rho>$-provable, and write: $PR_{<L,\Gamma,\rho>}(\psi)$.

Before concluding this chapter, I want to note that (as should be expected), where ρ is a proof procedure whose syntax is $*$ and where Γ is a set of $*$-sentences, I shall call Γ <u>ρ-consistent</u> if and only if for some *-sentence, ψ, Γ does not ρ-imply ψ.

There is one final comment that should be made. I noted that given the definition of a Lindstrom logic, definition 1 causes no real loss of generality. If ψ is a sentence (in a general sense) containing operation and constant signs, we can always find a sentence, $T(\psi)$, (in our more restricted sense) with the same formal truth conditions as ψ. Sometimes during the discussion that follows, the reader will have to extend a little charity. I shall talk about ψ, a sentence containing operation and constants symbols; the reader will have to realize that my remarks can be re-expressed as remarks about $T(\psi)$.

Footnotes for Chapter Two:

1. Paul Benacerraf, "Frege: the Last Logicist" in Midwest Studies in Philosophy (vol. 6).

2. J. Barwise, "Axioms for Abstract Model Theory", in Annals of Mathematical Logic 7, North-Holland Publishing Company. Pages 221-265.

2a. J. Donald Monk, Mathematical Logic, Springer-Verlag, New York (1976). Pages 417ff.

3. See P. Lindstrom, "First-Order Logic and Generalized Quantifiers" Theoria 32 (1966). Pages 186-195, and P. Lindstrom, "On Extensions of Elementary Logic", Theoria 35 (1969). Pages 1-11.

4. Alfred Tarski, "The Concept of Truth in Formalized Languages" in Logic, Semantics and Metamathematics, Oxford University Press (1956).

5. J. Barwise, op. cit. page 224.

5a. In order that this extension of g exists, we had to suppose (see page 53) that the complement of the range of g contains an infinite recursive set.

7. But see Nelson Goodman, Problems and Projects, The Bobbs-Merrill Company, Indianapolis (1972), page 154.

8. The definition of $\models_{fo}$ is due to Tarski.

9. It was asked by G. Boolos; and I should thank him for doing so. It prevented me from making an egregious error.

10. See Monk, op. cit. pages 206ff, and Quine's Methods of Logic.

11. R. Smullyan, First-Order Logic, Springer-Verlag, New York (1968) has a good account of the tree method.

12. For a good discussion of Church's thesis see Schöenfield, Mathematical Logic, page 119ff.

13. Monk, op. cit. His definitions are slightly different from those in the text; however, it is not hard to use his proof with our definitions.

14. Monk, ibid.

15. J. Bell and A. Slomson, Models and Ultraproducts: An Introduction, North-Holland Publishing Company (1969), pages 279ff.

Chapter 3

PROOFS, TRUTHS AND COMPLETENESS

In this chapter, I shall begin looking at completeness. I shall do primarily two things. In the first section I examine critically an argument purportedly establishing that only complete logics should be used when formalizing portions of mathematics; in the second section I look at another argument that apparently entails that sometimes incomplete logics must be used to formalize portions of mathematics. Examining these two (very different) arguments will help us begin to understand the philosophical issues behind completeness. In chapter four the issues raised here will be scrutinized carefully.

A word (or two) about completeness is needed. In chapter two we said that a logic is complete if and only if its valid sentences can all be proved using one proof procedure. This, in turn, entails that there is an effective method for generating all the valid sentences of that logic. Now, in light of definition 10 we can say that a portion of mathematics is formalized when we specify a set of axioms and characterize a formal logic so that we have an account of what it is for a sentence to follow from those axioms. If the axioms specified form an effective set, and if the formal logic characterized is complete, then we can effectively generate all the truths of that portion of mathematics. Clearly such a state of affairs is always desirable. We would like to be able to generate effectively the truths of every portion of mathematics. So, clearly, it would be nice if every portion of mathematics could be formalized using a complete logic. Unfortunately, Gödel shattered the hope of ever effectively generating the truths of arithmetic; the set of arithmetic truths cannot be effectively generated. So we seem to have three choices: (1) claim that arithmetic is not formalizable; (2) claim that arithmetic is formalizable, but not using an effectively generated set of axioms; or (3) claim that arithmetic is formalizable, but not using a complete logic.

Alternative (2), I think, should be rejected. Sets of axioms should be as simple as possible. One virtue of axioms is that they allow us to see what truths are "basic" to the field being investigated. Requiring that the set of axioms can be effectively generated is a plausible and natural way to avoid trivializing the enterprise of formalizing mathematics. After all, what other condition eliminates the possibility of taking all arithmetic truths as axioms, thus formalizing arithmetic without any need of a non-trivial logic. (We can call a logic, μ, "trivial" provided Γ μ-implies γ if and only if γ is in Γ.) For these sorts of reason, in this work, I shall ignore alternative (2).[1]

If we reject alternative (2), we have only alternatives (1) and (3). So either not every portion of mathematics can be formalized or some portions require the use of incomplete logics. Both horns of the dilemma are, admittedly, bad. The question motivating this chapter, then, is: given the alternatives, should we insist that every formal logic used to formalize mathematics be complete?

(i)

In practice complete Lindstrom logics are preferred to incomplete Lindstrom logics. The fact that the Lindstrom logic, L_Q, with the quantifier "there exists uncountably many x such that" is complete (with respect to some proof procedure) gives that logic a higher status -- even among the staunchest proponents of Hilbert's thesis -- than, say, the Lindstrom logic with the quantifier "there exist infinitely many x such that". Barwise[1a], for example, says that "sometimes, late at night, one can almost imagine some other world where...axioms [for L_Q] are considered laws of logic in the same way that we accept the laws of first-order logic." The admission is significant, coming, as it does, in the context of a defense of Hilbert's thesis. But why? Why should the fact that L_Q is complete tell in its favor?

One reason (maybe the main philosophical reason) is that mathematics is thought to derive its special character from its <u>deductive</u> nature. Mathematicians are believed to know things by <u>proving</u> them. Recall the passage from Putnam that was quoted in chapter one. That "mathematicians have as their <u>sole</u> method the method of mathematical proof" is a not uncommon view. We have, for example, Hilbert's claim[1b] that non-axiomatic statements of a mathematical theory "have validity only if one can derive them from those axioms by a finite number of logical inferences." Such a view suggests that any logic used to formalize a portion of mathematics should be complete. For, say that ψ is valid if and only if every model satisfying Γ, a set of axioms, satisfies ψ. Then, if ψ, a non-axiom, is valid only if ψ can be derived from Γ by a finite number of logical steps, then

ψ is valid only if ψ has what in chapter two was called a "Γ-validating conditional"; and this last claim suggests that the logic in question is complete. In light of this argument we can take Hilbert to be claiming that

> (HC) The logic that is part of any adequate formalization of a portion of mathematics should be complete.

Later I shall discuss an argument, implicit in work by Steiner, against Hilbert's reasons for (HC). Now, however, we should note that there is a tremendous amount of sentiment in favor of (HC). We already saw that it is implicit in Barwise's late night imaginings. It is also behind Quine's glib rejection of Henkin's branching quantifiers. Quine simply rejects them because the logic that results by adding branching quantifiers to first-order logic is not complete[2].

> The classical logic of quantification has a complete proof procedure for validity and a complete proof procedure for inconsistency...Classical...quantificational theory is on this score maximal; it is as far out as you can go and still have complete validity and inconsistency by the Skolem proof procedure.

It is (HC), I think, that makes Quine's remarks tell against Henkin's branching quantifiers.

Unfortunately, (HC) is argued for infrequently, and arguments that might be construed as supporting (HC) are not very convincing. Quine, for example, sometimes argues that (HC) is true because first-order logic is complete and first-order logic is the prototype of what (a) logic is. Quine claims that even if first-order logic does not formalize all logic, it does capture what is intrinsic to any formalization of logic, and that since first-order logic is complete, any formalization of logic must be complete. But Quine's argument is not at all cogent. Why believe that first-order logic is the prototype of what (a) logic is? Why not take, for example, the propositional calculus as the prototype of logic?[3] Quantification theory, after all, is a very recent invention (or discovery), due primarily to Frege. If the propositional calculus is the prototype of what (a) logic is, capturing all that is intrinsic to any formalization of logic, then why not argue, mimicing Quine, that since the propositional calculus is decidable <u>all</u> formalizations of logic must be decidable and that therefore first-order logic is not an adequate formalization of logic. The point is that although first-order logic is a very neat formal system and does capture much we believe ought to be captured by a formalization of logic we do not have any reason for thinking that all first-order logic's properties should be had by any adequate formalization of logic. More, much more, needs to be said before we can conclude that since first-order logic is complete, every formalization of logic should be complete. (Of course, in this context it would be inappropriate to argue that since Hilbert's thesis is true, all formalizations of logic should be complete, since first-order logic is complete.) Quine's argument raises too many questions to be a convincing reason for endorsing (HC).

In his <u>Theory of Knowledge</u>[4], Chisholm revives an old argument that can be construed as an argument for (HC), if we are willing to make some assumptions. Chisholm claims that "ordinary empirical procedures yield no knowledge of necessary truths." If we then accept

(A) For any formalization of a mathematical theory (see definition 10 in chapter two), $\mathfrak{I}$, and any sentence, ψ, ψ is $\mathfrak{I}$-valid only if ψ expresses a necessary truth,

we can conclude that, where ψ is a valid sentence of a formalization of a mathematical theory, ordinary empirical procedures yield no knowledge of ψ. So either there are valid sentences of a formalization of a mathematical theory that express unknowable truths, or else (HC) is true. For, it seems, if ordinary empirical methods cannot be used to establish that a statement is true, then we only have recourse to proofs; that is, if we cannot establish ψ using ordinary empirical procedures, then the only way to establish ψ is by deducing it from the axioms of the formalization in question. If we now deny that there are valid statements of a mathematical theory that are in principle unknowable, we have an argument that (HC) is true[5]. I think that something like this argument is behind many claims that (HC) is true: Mathematical truths are necessary truths; we can only know necessary truths by proofs; therefore, the logic of any portion of mathematics must be complete. It is, therefore, worthwhile to look a little closer at this argument for (HC), and to pay especial attention to Chisholm's argument that "ordinary empirical procedures yield no knowledge of necessary truths." (I shall call this argument for (HC) "Chisholm's argument", although, in fairness to Chisholm, it should be noted that he nowhere makes it and only explicitly argues that ordinary empirical procedures yield no knowledge of necessary truths.)

In addition to (A) something like the following premises are used.

(B) A valid statement of mathematical theory is known either by using ordinary empirical procedures, or by proving it;

(C) Ordinary empirical procedures yield no knowledge of necessary truths;

and

(D) All valid statements of a mathematical theory can be known.

Using (A)-(D) we can argue as follows. By (A) and (C), ordinary empirical procedures yield no knowledge of valid statements of a mathematical theory. So by (B) a valid statement of a mathematical theory can only be known by proving it (notice the similarity between this claim and the claim Putnam cites that "the sole method mathematicians...<u>can</u> use is the method of mathematical proof"). So by (D) every valid statement of a mathematical theory has a proof. If every valid statement of a mathematical theory has a proof, our formalization of mathematical theories should reflect this fact. That is, we do not want it to turn out to be the case that there is a sentence ψ such that, where Γ is the set of axioms of a formalization of a portion of mathematics, ψ is true in every structure satisfying all the members of Γ, but there is no proof of ψ from Γ. One natural way to eliminate this possibility is to insist that the logic of our formalization be complete. So, we can conclude, the logic of every formalization of a portion of mathematics should be complete, that is, (HC) is true.

I shall call this argument "Chisholm's argument". It has two parts. The first part purports to establish roughly that every mathematical truth has a proof; the second part concludes, in light of this claim, that the logic of every formalization should be complete. I shall only look at the first part of this argument now, leaving my speculations about the second part for (much) later.

(D) is a version of Hilbert's non ignorabimus, and although it is controversial, I shall not call it into question. Many find it reasonable to suppose that no truth of mathematics is in principle unknowable. Whether their intuitions are correct in this regard will not concern us now. (B), I think, is a version of a dichotomy endorsed by many epistemologists. Methods of justifying beliefs are often divided into two mutually exclusive, mutually exhaustive classes. In its modern form the distinction is between inductive and deductive arguments. I shall not discuss (B) at all. (A), too, is a statement often endorsed, although its content is left unclear. The main statement of Chisholm's argument with which we shall concern ourselves is (C).

Chisholm argues that (C) is true as follows. He claims that no induction can be used to justify our believing that a statement of the form ⌜Necessarily P⌝ is true. To see why he thinks this, let us use an example, say,

> (1) Necessarily, every number not identical with zero is the successor of some number.

Chisholm asks us to consider how an inductive argument establishing (1) would go. First, he claims, we would collect some instances of (1), verifying that particular numbers not equal to zero are successors. For example, we might verify that

(2) $1 \neq 0$ & 1 is the successor of 0

(3) $13 \neq 0$ & 13 is the successor of 12

(4) $5 \neq 0$ & 5 is the successor of 4.

But, Chisholm argues, although a collection of instances along the lines of 2-4 might justify our concluding that

(5) Every number not identical with zero is the successor of some number

is true, no collection of instances like (2)-(4) can justify our concluding that (1) is true. Chisholm then concludes that <u>no</u> induction can verify that (1) is true and that, in general, no induction can establish a necessary truth.

I think every reader will agree that Chisholm's argument as it stands is not very good. First, it does not establish that (C) is true, that "ordinary empirical procedures yield no knowledge of necessary truths." At most Chisholm's argument establishes that ordinary empirical procedures yield no knowledge that given truths are necessary. For Chisholm would, I think, admit that (5) is a necessary truth, and he does not seem loath to admit that (5) can be established by an ordinary empirical procedure. So, it seems, ordinary empirical methods do (or might) yield knowledge of at least some necessary truths. In particular, (5), a necessary truth, is established by collecting inferences along the lines of (2)-(4).

There is only one thing that can be said by Chisholm in response to this point. If he wants to conclude from his argument that (C) is true, he must maintain that (5) <u>cannot</u> be known unless it is known to be necessarily true. That is, Chisholm must maintain that if an individual knows (5), he (or she) must also know (1). Thus, ordinary empirical procedures cannot yield knowledge of (5) because they cannot yield knowledge of (1), and there is no knowledge of (5) without knowledge of (1). Thus, by asserting

> (α) If a person, M, knows that ψ is true and ψ is a mathematical statement, then M knows that ψ is necessarily true,

Chisholm appears able to save his argument from the objection raised in the last paragraph.

Although maintaining (α) appears to be the only way Chisholm has of saving his argument, I do not think that it works. First, (α) just is not a very plausible principle. A person can know biological facts without knowing that they are necessarily true, why should it be that mathematical facts cannot be known unless they are known to be necessarily true? Clearly, a defense of (α) must come to grips with this question, and it must do so without appealling to any claims to the effect that mathematical statements can only be known by non-empirical procedures, for that is precisely what is at stake. But even if we resolve all our doubts about (α), I do not think that it is adequate for the task set it. Notice that we may know that (5) is true by virtue of the sort of inductive argument outlined above, and then we may conclude that (1) is true because we know that (5) is a mathematical statement and we know that all mathematical truths are necessary, that is, we know that (A). We may infer that (1) is true from (5) and the (purported) fact that every mathematical statement if true, is necessarily true. Thus, (α) would be satisfied, and so (1) can be known on the grounds of an ordinary induction after all. Second and, from our point of view, more interesting is the fact that there _is_ an ordinary induction that appears to establish that (1) is true, although it is not the induction that Chisholm mentions, the one that is conducted by collecting instances along the lines of (2)-(4). Instead, we might collect instances along the lines of

(2') 1 is such that necessarily it is not 0 and is a successor

(3') 13 is such that necessarily it is not 0 and is a successor

(4') 5 is such that necessarily it is not 0 and is a successor

Collection of instances along the lines of (2')-(4') might, unlike collection of instances along the lines of (2)-(4), justify our concluding that something strictly stronger than (5) is true. In fact, these instances seem to establish that

(5') For every natural number, necessarily, if that number is not identical with zero, then it is a successor.

It can be claimed that if (5') is true, then (1) is true, and that, therefore, the induction using instances along the lines of (2')-(4') to verify (5') verifies (1) -- contrary to Chisholm's claim that _no_ induction can be used to justify a statement of the form ⌜Necessarily P⌝. There are, however, several points that can be made against this objection to Chisholm's argument; but they reveal that very strong assumptions about the philosophy of mathematics must be made in order to preserve Chisholm's argument.

(1) usually is formally represented by a sentence of the form

(1') $\Box \forall x \psi(x)$,

where '$\Box$' is a modal operator corresponding to the English 'necessarily'. Similarly, (5') is represented as

(5') $\forall x \Box \psi(x)$.

Thus, the claim that if (5') is true, (1) is true -- used in the previous paragraph -- would be represented by something like

(6) $(\forall x \Box \psi(x)) \rightarrow (\Box \forall x \psi(x))$.

(6), however, is a form of the controversial Barcan Formula. So the above objection of Chisholm's argument presupposes that the Barcan formula is valid (at least when we are dealing with the necessity of arithmetic sentences.)

It sometimes seems, however, that there are good reasons for denying that the Barcan Formula is valid when dealing with arithmetic sentences. In defense of Chisholm's argument one could point to the work accomplished by interpreting sentences of the form

(7) $\Box p$

as

(7') $Bew(\ulcorner p \urcorner)$,

where 'Bew( )' is the provability predicate Gödel showed how to construct.[8] A natural extension of this work is to interpret quantified modal sentences like (5") as

(8) $\forall x \mathrm{Bew}(\mathrm{Sub}(\ulcorner\psi(x)\urcorner,x))$

where 'Sub(n,m)' is a (p.r.) term that gives the Gödel number of the result of substituting the mth numeral for the variable 'x' in the formula with gödel number n. Thus, (6) would be interpreted as

(6') $\forall x \mathrm{Bew}(\mathrm{Sub}(\ulcorner\psi(x)\urcorner,x)) \rightarrow \mathrm{Bew}(\ulcorner\forall x\psi(x)\urcorner)$,

and (6'), as we know from Gödel's first theorem, is not generally true[9]. Thus, if (1) and (5') are properly formalized as (1') and (5"), and if (1'), (5") and (6) are correctly interpreted along the lines of (7'), (8) and (6'), respectively, then there is a reply to the objection to Chisholm's argument made two paragraphs back. (6) in that case, is not valid, and so the induction using premises along the lines of (2')-(4') cannot be used to establish (1).

There is, however, a very serious problem with this sort of reply. What reason do we have for thinking that (7') is the (or a) correct interpretation of (7), given that '$\Box$' is supposed to be a modal operator corresponding to the English 'necessarily' as used in (1)? Similarly, why think that (8) is the (or a) correct interpretation of (5")? The only reason for thinking so is the belief that an arithmetic statement, ψ, is necessary just in case it is first-order provable from a specified set of axioms -- in this case the axioms of Peano Arithmetic. But this makes Chisholm's argument superfluous, for we already supposed that (A) several pages back is true, that is, that all truths of arithmetic are necessary. So, it would follow that all truths of arithmetic are provable from the axioms of Peano Arithmetic. But this implies that (HC) is true (and in a sense that is easily refuted; but more of that later). The response made in the last paragraph, then, already presupposes that (HC) as applied to arithmetic is true.

There are other sorts of responses that can be made in defense of Chisholm's argument. We need not appeal to a particular interpretation of the box, as it occurs in (7) and (8), in order to object to (6). We might appeal directly to the existence of (so-called) non-standard models of arithmetic. We could claim, for instance, that if an arithmetic statement is necessary, then it is true in all models of arithmetic. Then, since there are sentences, $\psi(x)$, such that for each standard n, $\psi(\underline{n})$ is true in every model of arithmetic, but such that $\forall x\psi(x)$ is false in some model of arithmetic, we appear to have reason for denying that (6) is true. This defense of Chisholm's argument, however, already presupposes a good deal about what logic is appropriately used for formalizing arithmetic. The non-standard models of arithmetic that make (6) seem dubious cannot be shown to exist no matter what logic is used to formalize arithmetic. To make use of the notion of a model of arithmetic in the defense of Chisholm's argument presupposes that such models can be characterized independently of the logic used when formalizing arithmetic. This presupposition, however, is illegitimate. What models we count as of arithmetic depends upon what logic we use to formalize arithmetic, and if this is so, an appeal to non-standard models of arithmetic cannot be used to defend Chisholm's argument. Consider: the reason we appealed to the existence of non-standard models of arithmetic was to give reasons for thinking that the Barcan formula is false was then used as reason for thinking that the logic used for formalizing (at least) arithmetic is complete. So ultimately the appeal to non-standard models is supposed to give reasons for thinking that one logic rather than another is more adequately used when formalizing arithmetic. But we cannot show that there are non-standard models for every logic, since what models we count as of arithmetic depends on what logic is used to formalize arithmetic. For instance, if weak second-order logic is used and we suppose that, in addition to the Peano axioms, every model of arithmetic satisfies a sentence stating that the set of predecessors of every number is finite, then we cannot prove the existence of non-standard models of the sort that allowed us to conclude above that (6), the Barcan formula, is false. So our reason for thinking that (6) is false has, in turn, for its support the claim that only a specific sort of logic can be used to formalize arithmetic, a logic that allows there to be non-standard models of the relevant kind. It looks, then, as if the above defense of Chisholm's argument begs the question, namely, what sort of formal logic should be used when formalizing arithmetic?

I think it is fair to conclude that Chisholm's argument does not establish that (HC) is true. As pointed out above this is not as parochial a conclusion as might be thought. I think something like Chisholm's argument is very often put forward in defense of (HC). In this section, I concentrated on criticizing claim (c), the claim that non-empirical methods yield no knowledge of necessary truths. We have seen no good reasons for thinking that this claim is true. On the other hand, we have seen no good reasons for thinking that it is false. On the face of it, the induction on page 87 just does not seem to establish that (5') is true. But imagine that we have no other reason for thinking that (5') is true, that we have used (5') successfully repeatedly in our scientific theories and that we have collected millions and millions of instances along the lines of (2')-(4'), all of which confirm (5'). Would (5') be any less established than any contingent truth supported by (similar) empirical procedures? I do not know. We will, however, return to related issues later in chapter four. There are, at any rate, other examples of (seemingly) empirical procedures yielding knowledge of necessary truths. For example: you are in a room and you know that only couples are in the room; you count the people in the room and conclude that the number counted is even. Another example: you decide to make a rectangular jigsaw puzzle of the standard sort; you cut up a piece of plywood, count the pieces and conclude that the number counted is composite.[9a] There do seem to be ordinary empirical procedures that yield knowledge of necessary truths. I shall save further discussion of this issue, however, for chapter four. In the next section, I will continue examining (HC) and shall look at one set of philosophical reasons for thinking that sometimes the logic used to formalize a portion of mathematics should not be complete and that, therefore, Hilbert's thesis and (HC) are false.

(ii)

Although no conclusive reasons have been given yet for thinking that only complete logics should be used when formalizing mathematics, as already noted, in practice complete Lindstrom logics are preferred to incomplete ones. There are, as M. Dummett has stressed[10], technical reasons for wanting complete and sound formalizations of logic. He points out that soundness and completeness proofs show that certain proof-techniques are valid, and notes[11] that some logicians seem wont to say that

> the whole interest of the soundness and completeness proofs for classical sentential logic lies in the effective method they provide for determining whether or not a formula is derivable from some finite set of formulas.

This technical interest in soundness and completeness proofs can be used to argue for (HC), the claim that only complete Lindstrom logics should be used when formalizing portions of mathematics. There are a class of techniques, it can be argued, ordinarily used by mathematicians, that are "provided" by soundness and completeness proofs. Since these techniques are employed by mathematicians, our formalizations of mathematics should preserve and justify those techniques. Therefore, it can be concluded, we should only use complete Lindstrom logics when formalizing portions of mathematics; that is, (HC) is true.

This argument helps explain why in practice complete Lindstrom logics are preferred to others -- why, for instance, the logic with the quantifier "there exist uncountably many x such that" might be preferred to $\mathcal{L}_{WS}$, defined in chapter two. Complete formalizations make the mathematician's job easier. But, as Dummett goes on to note[12], the technical interest that soundness and completeness proof have for the working mathematician is not enough to guarantee philosophic interest in those proofs. To get any philosophic mileage out of the technical interest of soundness and completeness proofs, we must first (philosophically) justify those very techniques soundness and completeness proofs provide. The philosopher's task is not to make the mathematician's job easier. It is, in part, to examine the techniques mathematicians ordinarily use, seeing whether they can be (philosophically) justified and systematized. Mathematicians may want complete formalizations of logic because they allow certain tools to be employed; however, this alone is not enough to justify the philosopher's concluding that the Lindstrom logic of formalizations of mathematics should be complete. There might be philosophical reasons for rejecting, in some cases, the very methods provided by completeness and soundness proofs. Thus, we might be able to adduce philosophical reasons for denying that only complete Lindstrom logics should be part of our formalizations of mathematics.

In this section, I shall look closer at this claim. I shall look at a sort of independence proof that is justified by soundness and completeness proofs and shall examine, in some detail, Frege's reasons for rejecting that sort of proof. It will follow that Frege's reasons can be used to refute (HC), the claim that only complete logics should be part of our formalizations of mathematics, since if (HC) is true, the sort of independence proof in question is valid.

Let Γ be a set of axioms and ψ a sentence. Then, informally, we say that ψ is independent of Γ provided ψ does not follow from Γ. Given the definitions of the last chapter, we might put this as

(IND) ψ is independent of Γ=df there is structure satisfying every member of Γ and also satisfying $\sim\psi$.

It is worth pausing to notice that, if logic should be formalized using a Lindstrom logic, then any acceptable proof procedure (see definition 11 in chapter two) must be sound (see definition 15). Suppose we think that logic should be formalized using a Lindstrom logic, μ, and that we think a proof procedure, ρ, captures an informal notion of "logical proof". Then if ρ is not μ-sound, there is a finite set of axioms, Γ, such that Γ ρ-implies ψ but Γ does not μ-imply Ψ. That is, there is a structure satisfying every member of Γ and also satisfying $\sim\psi$. But, then, it seems (speaking informally now) that we can prove ψ from Γ, even though ψ is not true in every structure satisfying all members of Γ. So, by (IND) ψ is independent of Γ, even though we can prove ψ from Γ. This conclusion sufficiently conflicts with our presystemmatic notions of "proof" and "independence" to warrant our concluding that, if logics should be formalized using a Lindstrom logic, any accpetable proof procedure is sound.

The argument in the last paragraph is not meant to establish that acceptable proof procedures should be sound -- only that someone accepting that the logic of a portion of mathematics should be formalized using a Lindstrom logic seems committed to the claim that all acceptable proof procedures are sound. Any other claim would conflict with presystemmatic intuitions. It is by no means true, however, that every view about formalizations of mathematics is committed to the claim that all acceptable proof procedures are sound. Intuitionists, as already noticed, do not believe that the correct way to formalize logic is by means of a Lindstrom logic. We saw that every Lindstrom logic satisfies the law of excluded middle, and this law is not intuitionistically valid. It is not surprising, then, that intuitionists are not interested in (classical) soundness and completeness proofs, and are not committed to the claim that every acceptable proof procedure is sound.[13a]

In light of the definition of 'independence' ((IND)) it can be seen that whenever our logic is complete, there are (at least) two methods of showing that a sentence, ψ, is independent of a set of sentences, Γ. We can either (i) construct (exhibit, point out, or something similar) a model of Γ in which $\sim\psi$ is true of (ii) show that no contradiction can be proved from $\Gamma\cup\{\sim\psi\}$ and then appeal to completeness to infer that ψ is independent of Γ. We shall see that, at least so far as geometry is concerned (and probably arithmetic as well) Frege ruled out the second method and that, therefore, on Frege's view, geometry cannot be formalized using a complete logic.

One often hears that the parallel axiom is false, while the other axioms of Euclidean geometry are true. It would therefore be of grave logical consequence should it turn out that the parallel axiom is not independent of the other axioms of Euclidean geometry. (In the future, I shall simply refer to the other axioms of Euclidean geometry as the geometric axioms.) Yet this is precisely what Frege maintained[14]:

> If you [Hilbert] are concerned to demonstrate the mutual independence of axioms, you will have to show that the non-satisfaction of one of these axioms does not contradict the satisfaction of the others...But it will be impossible to give such an example in the domain of elementary geometry because all the axioms are true in this domain.

Frege believed that in order to show the parallel axioms independent of the geometric axioms we must find an example in the domain of elementary geometry in which the "non-satisfaction" of the parallel axiom does not contradict the "satisfaction" of the geometric axioms. For Frege, I think, this meant we must find an example of a space consisting of points, lines and distances in which the geometric axioms are all true, but in which the parallel axiom is false. The axioms of Euclidean geometry, the geometric axioms and the parallel axiom, are about points, lines and distances; we use them to say things about points lines and distances. Therefore, Frege concluded, in order to show that the parallel axiom is independent of the geometric axioms, we must show that what the geometric axioms <u>say</u> about points, lines and distances can be <u>true of</u> points, lines and distances, even though what the parallel axiom <u>says</u> about points, lines and distances is <u>false of</u> points, lines and distances. As Frege put it:

> You want to prove the mutual independence and lack of contradiction of certain premises (axioms), as well as the unprovability of propositions from certain premises (axioms)...What means have we of demonstrating that certain properties, requirements (or whatever else one wants to call them) do not contradict one another? The only means I know is this: to point to an object that has all those properties, to give a case where all those requirements are satisfied.

In light of these statements and considerations, I think we have to conclude that Frege could not have maintained (consistently) that geometry is formalizable using a complete logic. According to Frege, the <u>only</u> way to prove the independence of the parallel axiom is to construct a model satisfying the geometric axioms in which the negation of the parallel axiom is true. We cannot first demonstrate the consistency of the negation of the parallel axiom with the geometric axioms, and then appeal to completeness; the logic, then, used to formalize geometry cannot, on Frege's view, be complete. But Frege goes even further. Not only does he claim that we cannot demonstrate the independence of the parallel axiom by means of a consistency proof, he claims that we simply cannot demonstrate that it is independent. According to Frege, there is no model satisfying the geometric axioms in which the negation of the parallel axiom is true.

Typically the parallel axiom is shown independent of the geometric axioms by means of substitutions. New terms are uniformly substituted for each of the geometric terms occurring in the Euclidean axioms. We obtain new axioms by substituting, for example, 'point inside a fixed Euclidean cicle' for 'point' and 'open chord of a fixed Euclidean circle' for 'straightline'. The axioms so obtained can then be seen to have a natural model in which the translations of the geometric axioms are all true, while the translation of the parallel axiom is false. It is then usual to infer that the Euclidean axioms themselves have a similar model, a model in which the geometric axioms are true, but in which the parallel axiom is false. Indeed, this last inference is justified by clause (iia) of definition 5 (see chapter two). Frege was aware of such proofs. Nevertheless, he denied that the parallel axiom is independent. Should we then conclude that in addition to denying that geometry can be formalized using a complete logic, Frege also denied that it is formalizable using a logic satisfying clause (iia) of definition 5? and that, therefore, Frege denied geometry is formalizable using a Lindstrom logic? Although I think the answer to the second question is 'yes', the answer to the first question, as we shall see, is 'no'.

The only way to demonstrate that the parallel axiom is independent, we have seen Frege claim, is to construct (or exhibit) a structure satisfying the parallel axiom. There is, however, according to Frege, no such structure in the domain of elementary geometry "because all the axioms are true in this domain." If Frege is right, every structure exhibited that satisfies the geometric axioms will satisfy the parallel axiom because (i) only structures "in the domain of elementary geometry" satisfy the geometric axioms and (ii) every structure in the domain of elementary geometry satisfies the parallel axiom. Thus, according to Frege, there is no way to demonstrate that the parallel axiom is independent.

To a certain extent, Frege seems correct about this matter. If 'point' as it occurs in the Euclidean axioms refers to Euclidean points, and if 'line' refers to Euclidean lines, then it is impossible for the parallel axiom to be false. Given any Euclidean point and any Euclidean line it just always is the case that there is one and only one Euclidean line parallel to the given line and through the given point. Thus, if, say, adequate formalizations of the Euclidean axioms must capture what Frege called the "senses" of the terms occurring in those axioms, then it will be impossible to demonstrate the independence of the (formalization of) the parallel axiom.

We should at this point perhaps recall why Frege started formalizing mathematics in the first place. Frege thought that mathematical practice during his time was confused. It is not unfair to say that Frege though his contemporary mathematicians literally did not know what they were talking about. He noted that they were unable to define the most elementary concepts of their science and that when they tried to formulate such definitions the results were often far-fetched and contradictory. It looked to Frege as if mathematicians were not paying enough attention to the senses expressed by the words and symbols they used, and this, he thought, was inexcusable. "The sentence is of value to us," he said[16], "because of the sense that we grasp in it..."

> If you ask what constitutes the value of mathematical knowledge the answer must be: not so much what is known as how it is known, not so much its subject matter as the degree to which it is intellectually perspicuous and affords insight into its logical interrelations. And it is just this which is lacking. Authors explain the commonest expressions...in totally different ways and these discrepancies are not just trivial but concern the very heart of the matter.[17]

In order to avoid such confusions, Frege invented the concept-script, a formal logic in which the statements of ordinary mathematics, he hoped, could be expressed clearly and proved convincingly. The sense of a sentence of ordinary mathematics formalized as a formula of the concept-script, Frege thought, could be read and grasped without confusion. This was, for Frege, the primary goal of formalizing mathematics: to express the sense of ordinary mathematical sentences in as unconfused a manner as possible. "The effect," he said[18], "of the logical analysis...will then be precisely this -- to articulate the sense clearly." Formalizations, then, of ordinary sentences of mathematics must, according to Frege, express the senses of those sentences.

Frege's view is, I think, reminiscent of the so-called Skolem paradox. Frege supposed that 'is a point' has a sense that is independent of any structure in which formalizations of the Euclidean axioms are interpreted. He thought that formalizations of the axioms of Euclidean geometry must, therefore, in order to be adequate, capture this sense. Similarly, the Skolem paradox seems to presuppose that 'uncountable' has a sense that is independent of any structure in which formalizations of sentences containing that word are interpreted. Recall, for a moment, how the Skolem paradox goes. It is argued that

(1) There is an uncountable set

has no first-order formalization because any first-order formalization of (1) is true in a countable model even though what (1) says is that there is an uncountable set. Similarly, we saw Frege argue that the parallel axiom can never be false in a structure satisfying the geometric axioms because what the parallel axiom says about points is always true.

One way the Skolem paradox is often resolved is by claiming that there is no sense had by 'uncountable' that is independent of the structures in which formalizations of (1) are interpreted. We can avoid the Skolem paradox if, when formalizing (1), we eliminate the word 'uncountable' in favor of terminology that is interpretable relative to structures.[19] In a similar manner we might hope to remove the sting of Frege's comments. We might say that the axioms of Euclidean geometry have no sense that is independent of the structures in which formalizations of those axioms are interpreted. We might try eliminating 'is a point' in favor of terminology that is interpretable relative to structures. Our conclusion would be that sentences of geometry only have sense relative to structures and that, therefore, Frege's criticism of Hilbert's independence proof was misguided. We are not limited to structures "in the domain of elementary geometry" when we try to demonstrate that the parallel axiom is independent. We can, for example, substitute 'point inside a fixed Euclidean circle' for 'point' show the independence of the parallel axiom in the standard way described on page 98.

The problem with this sort of reply is that it begs the question. Frege would simply deny that the axioms of Euclidean geometry can be phrased using terminology that is interpretable relative to structures. The issue over which Frege and Hilbert differ just is whether, say, the parallel axiom has a sense that is independent of structures. According to Frege, it does; according to Hilbert, as we shall see in the next chapter, it does not. Frege could (and would) respond to the preceding paragraph by reiterating his claim that 'is a point' has a sense that is independent of structures in which formalizations of sentences using that phrase are interpreted.

In a similar manner, of course, it can be denied that (1) (see page 101) can be rephrased using terminology that is interpreted only relative to structures. For example, it can be claimed that 'uncountable' has a sense that is independent of structures and that formalizations of (1) should exhibit this fact. Some not uninteresting mathematical work has proceeded along these lines. (1), it might be claimed, should be formalized using the quantifier "there exists uncountably many x such that". Thus, when formalizing (1) we do not eliminate 'uncountable' in favor of terminology that is interpreted only relative to structures, rather we eliminate 'uncountable' in favor of logical terminology, in favor of Keisler's quantifier. I shall soon suggest that Frege had something along these lines in mind. He seemed to want to say that 'is a point' cannot be eliminated in favor of non-logical terminology. On Frege's view, it is a primitive expression on a par with the logical constants.

Frege, we have seen, thought that (at least some) mathematical sentences have sense independent of structures. He also, I argued, thought that formalizations of such sentences should express this sense. As he put it[20]:

> The natural way in which one arrives at a symbolism seems to me to be this: in conducting an investigation in words, one feels the broad imperspicuous and imprecise character of word language to be an obstacle, and to remedy this, one creates a sign language in which the investigation can be conducted in a more perspicuous way with more precision.

Formalizations, according to Frege, are ways of expressing the senses of ordinary mathematical sentences more precisely. Thus, if a formalization of an ordinary mathematical sentence can be interpreted in ways contrary to the sense of that ordinary sentence, the formalization in question, according to Frege, is inadequate. It is this, I think, that led Frege to reject formalizations, like Hilbert's, that permit the sort of independence proof described above.

Frege's claims can, I think, be made more forceful, if we look at them in the following light[20a]. In a number of places[20b] Quine suggests that a sentence is logically true if it stays true under all substitutions. Frege, I think, had a similar criterion in mind. Now, Quine's criterion, as it stands, needs amending. For instance, we do not want it to turn out that

(2) $x=y \ \& \ y=z. \rightarrow .x=z$

is not counted as a logical truth because

(3) $x \neq y \ \& \ y \neq z. \rightarrow .x \neq z$

is false and results from (2) by a substitution. What Quine's criterion must be amended to is: a sentence is logically true if it stays true under all substitutions for its non-logical constants. When we use Quine's criterion[20c], we must keep in mind that certain terms -- the logical constants -- remain fixed. Frege's criticism of Hilbert turned, I think, on his accepting Quine's criterion of logical truth and insisting that 'is a point' as it occurs in the Euclidean axioms is on a par with 'and' and 'is identical with'. We are, I think, justified in attributing to Frege the view that when formalizing the Euclidean axioms, 'is a point' should be treated as a logical predicate on a par with the identity sign. This interpretation of Frege is supported by the following statement of Dummett's:

> In the 1903 article on the foundations of geometry... [Frege] says that Hilbert's proof of the independence of his axioms for Euclidean geometry is a proof of the independence only of psuedo-axioms, obtained by varying the interpretations of the primitive expressions. In the actual axioms of Euclidean geometry, however, the primitive expressions have a fixed, determinate sense, and one cannot conclude from the independence of the psuedo-axioms to the independence of the genuine axioms.

There are, of course, alot of objections that can (and should) be raised against Frege's position. We should ask, for instance, whether it is the case that an ordinary mathematical sentence and a formalization of it have sense independent of any structures. Recently, H. Putnam has asked just this question and argued that, in fact, several results in model theory and mathematical logic suggest that sentences of mathematics and their formalizations do <u>not</u> have sense independent of structures[21]. This is a position first defended by Skolem[22]. Frege, however, as we have seen, is committed to the view that formalizations of mathematical sentences do have sense independent of structures, and it is precisely on this claim that his criticism of Hilbert's independence proof rested. But these are issues that I shall for the moment leave aside, touching on them only a bit in the next chapter.

Frege's criticism of the standard proof (and of Hilbert's proof) that the parallel axiom is independent, we have seen, centered around two claims:

(I) Ordinary mathematical sentences have sense,

and

(II) A formalization of an ordinary mathematical sentence has the same sense as that sentence.

Formalizations of the Euclidean axioms that allow the independence of the parallel axiom to be proved, according to Frege, are inadequate insofar as they violate (II). I suggested that, on Frege's view, the Euclidean axioms must be formalized using a logic containing a logical constant corresponding to 'is a point'[22a]; and, in general, it seems clear that one logic is more suited than another for expressing specified senses. For example, a logic with the Keisler quantifier, "there exist uncountable many x such that", it can be argued, is more suited for expressing the senses of sentences, like (1), in which 'uncountable' occurs; and, according to Frege, a logic with a logical expression corresponding to 'is a point' is more suited for expressing the senses of the Euclidean axioms. So, it seems that Frege's position -- conditions (I) and (II) -- conflicts with what, in chapter one, I called "Frege's thesis." (Frege's thesis, recall, is the position that there is one and only one formal logic in which the proofs of ordinary mathematics can be formalized.) Conditions (I) and (II) suggest that what formal logic is used is a function of what portion of mathematics is to be formalized.

This is not a view without proponents in the mathematical community. Flum and Ziegler claim that

> The formal language in the study of topological structures is Lt. This is a fragment of the (monadic second order language...obtained by allowing quantification of set variables of the form $\exists X(t \cdot X \& \psi)$...
>
> The reasons for the distinguished role that Lt plays in topological model theory are twofold. On the one hand, many topological notions are expressible in Lt...On the other hand, the expressive power of Lt is not too strong...[26]

Thus mathematicians sometimes adduce reasons for using formal logics other than first-order logic that depend upon the portion of mathematics that is to be formalized. According to Flum and Ziegler, for example, topology is illuminatingly formalized using the language L_t. According to Frege, Euclidean geometry is formalized using a logic containing a logical expression corresponding to 'is a point'. Frege's criticism of Hilbert, then, is not without analog in current mathematical practice.

Before ending this chapter we should ask: what do Frege's claims have to do with our general discussion of Hilbert's thesis? First, there is the obvious point that first-order logic is complete, and Frege's criticism of Hilbert, as we saw, entails that the logic used to formalize Euclidean geometry is not complete. Thus, what Dummett calls[22b] "Frege's Platonism" can be used as reason for denying that Hilbert's thesis is true. It might be thought, of course, that Frege's Platonism is more difficult to defend than the claim that logic is complete. This may well be so. Nevertheless, Frege's claims are an important example of philosophical reasons for denying Hilbert's thesis. But illustrative reasons are not the only reasons I had for discussing Frege's criticism of Hilbert. His criticism also sheds important light on a common confusion. In chapter 1, I discussed what was called "Morley's argument". Recall that Morley's argument is, roughly, that Hilbert's thesis is true because mathematics is reducible to set theory ((claim (i)) and set theory is a first-order theory (claim (ii)). We saw that the main source of evidence for claim (i) of Morley's argument (the claim that mathematics can be reduced to set theory) was the fact that a good many of the notions ordinarily used in mathematics can be defined using only the language of set theory. This last claim, however, is only a fact because ϵ is interpreted set theoretically (compare page 29). If the language of the set theory to which mathematics is reduced is first-order -- that is, if claim (ii) of Morley's argument is true -- the Löwenheim-Skolem theorem guarantees that if that set theory had a model, it has an arithmetical model; that is, there is an arithmetical relation, α, such that when ϵ

is interpreted as α, every truth of set theory becomes a truth of arithmetic. However, we do not -- and rightly so -- take this as showing that every mathematical notion is definable using only the language of arithmetic, even though, where ψ is a set theoretic definition of a mathematical notion we can obtain an arithmetical definition of that notion by substituting a term interpreted as α for every occurrence of ε in ψ.[22c] In some ways, when we talk about reducing mathematics to set theory we treat ε as if it were a logical constant, just as Frege treated 'is a point' as if it were a logical constant when he discussed geometry. Thus, in light of Frege's criticism of Hilbert we might want to say that the evidence for claim (i) of Morley's argument undermines the evidence for claim (ii).

I mention these points to stress that Frege's criticism of Hilbert is indeed relevant to our discussion of Hilbert's thesis. It provides us with an important example of a philosophical position that conflicts with Hilbert's thesis and suggests that the logic that should be used when formalizing mathematics is not complete, hence not first-order. In the next chapter I shall look in much detail at a contrary position, a position entailing that logic is complete and apparently entailing that Hilbert's thesis is true.

Footnotes for Chapter Three:

1. The reader who feels comfortable with alternative (2) need not worry. Nothing substantial turns on my ignoring it until the very end of chapter four.

1a. J. Barwise, "The Realm of First Order Logic" in _The Handbook of Mathematical Logic_.

1b. D. Hilbert, "Uber den Zahlbegriff"; quoted on page 84 of J.C. Webb, _Mechanism, Mentalism and Metamathematics_, D. Reidel Publishing Company, Boston (1980).

2. See W.V.O. Quine, "Existence and Quantification" in _Ontological Relativity and Other Essays_, op. cit.

3. Boolos makes essentially this point in "Second Order Logic", _op. cit._

4. R. Chisholm, _Theory of Knowledge_, Prentice Hall Press,

5. There are many objections to this argument. The reader is asked to ignore them for the moment.

7. There are, of course, well-known problems with the argument:

   (1)-(n) inductively establish P
   P deductively establishes Q
   ∴ (1)-(n) inductively establish Q

   I think in the contexts we are concerned with, we can ignore these problems without too much difficulty.

8. See, for instance, George Boolos, _The Unprovability of Consistency_, Cambridge University Press, Cambridge (1979).

9. G. Boolos pointed out to me the ideas contained in this paragraph.

9a. The example is G. Boolos's.

10. See M. Dummett, "The Justification of Deduction" in _Truths and Other Enigmas_, Harvard University Press (1978).

11. _Ibid._

12. *Ibid*.

13. Cf. Dummet, *op*. *cit*.

14. Brian McGuinness (ed.), *Gottlob Frege: The Philosophical and Mathematical Correspondence*, The University of Chicago Press, Chicago (1980), page 43.

15. *Ibid*., page 43.

16. Hermes, Kambertael and Kaulbach (eds.), *Gottlob Frege: Posthumous Writings*, The University of Chicago Press, Chicago (1979), page 206.

17. *Ibid*., page 157.

18. *Ibid*., page 211.

19. See, for example, H. Putnam, *Mathematics, Matter and Method*, *op*. *cit*., pages 15-16.

20. McGuinness (ed.), *op*. *cit*., page 33.

20a. G. Boolos suggested that this light be used.

20b. See Quine's *Philosophy of Logic*, page 46, for example.

20c. I am not proposing that Quine's criterion be accepted.

20d. M. Dummett "Frege and Consistency Proofs", in

21. Hilary Putnam, "Models and Reality", *Journal of Symbolic Logic* (1981).

22. See Skolem (1920) in van Heijenoort, *op*. *cit*.

22a. Incidentally, this logic would not be a Lindstrom logic; but that needn't concern us.

22b. Dummett, *op*. *cit*.

22c. Quine is, perhaps, an exception.

23. McGuinness (ed.), *op*. *cit*., page 44.

24. *Ibid*., page 46.

25. Mc Dummett, *Frege: Philosophy of Language*, Harvard University Press, Cambridge (1981), chapter 3.

26. Jörge Flum and Martin Ziegler, *Topological Model Theory*, Springer-Verlag, New York (1980), page vii.

Chapter 4

HILBERT AND HIS THESIS

In chapter three I did two things. First, I examined critically an argument that only complete logics should be used when formalizing mathematics. I looked especially closely at premise (C) of that argument, the claim that ordinary empirical procedures yield no knowledge of necessary truths. Second, I looked at a position Frege endorsed when he criticized Hilbert's formalization of and independence proof for the parallel axiom. In this chapter, I shall continue investigating these themes. First, I shall look at how Hilbert replied to Frege and shall reconstruct, using concepts ordinarily employed today, a perhaps anachronistic version of Hilbert's philosophy of mathematics. Using this philosophy I shall then construct an argument for Hilbert's thesis, the claim that only first-order logic should be used when formalizing mathematics. I shall conclude this chapter by looking at the status of this argument in the light of a (fairly standard) refutation of Hilbert's philosophy of mathematics.

(i)

According to Hilbert, one of the major differences between himself and Frege is their different opinions about the importance of consistency proofs and the relation of those proofs to conclusions about the truth-values of axioms. In a letter to Frege, Hilbert says[1]

> I was very much interested in your sentence: 'from the truth of the axioms it follows that they do not contradict one another', because for as long as I have

> been thinking about these things, I have been saying the exact reverse: If the arbitrarily given axioms do not contradict one another, then they are true, and the things defined by the axioms exist.

Frege believed that consistency proofs (at least in geometry) are superfluous because if a set of axioms contains only truths, then that set is consistent and the axioms of (Euclidean) geometry, Frege thought, can be seen to be truths by inspecting our "spatial" intuitions. Hilbert, however, held that in all areas of mathematics consistency proofs are essential. According to Hilbert's point of view, confronted with "arbitrarily given axioms," we can only see that those axioms are truths by demonstrating that they do not "contradict each other." If, according to Hilbert, a set of axioms is consistent, then it contains only truths; so the way to determine that the axioms we accept are truths, according to Hilbert, is not by inspecting intuitions, but by demonstrating that those axioms form a consistent set.

Hilbert and Frege, therefore had very different views about how we know mathematical truths. Hilbert thought, as shall be seen, that a mathematical truth is known only if we have shown how to deduce it from a demonstrably consistent set of axioms. So, in particular, on Hilbert's view, we know that a set of axioms contains only truths only if we have demonstrated that that set is consistent. Frege, on the other hand, was committed to no such claim. "It cannot be required," he said[2], "that we should prove everything, because that is impossible." In particular, according to Frege, we may know that a set of axioms contains only truths even if we have not demonstrated that that set is consistent. Rather, we can know that axioms are true independently of any deductions and proofs. For example, as I mentioned, Frege thought that we could know that the axioms of Euclidean geometry are true without any consistency proof, merely by inspecting our spatial intuitions. Furthermore, unlike Hilbert, Frege thought that axioms are not "arbitrarily given." He thought that axioms are truths upon which a system of mathematics rests, and which we can know to be true without consistency proofs. Thus, Frege and Hilbert had very different views about how we know mathematical truths. As I shall soon argue, Hilbert's view is committed to (HC), the claim that only complete logics should be used when formalizing mathematics; as I argued in the previous chapter, Frege's view is committed to no such claim.

At first, the view that I have associated with Hilbert seems wrong. Two sets of sentences can both be consistent even though they are jointly inconsistent. For example, A may contain ψ and B may contain the negation of ψ even though A is consistent and B is consistent. But then, on Hilbert's view, it looks as if A contains only truths and B contains only truths. So ψ is a truth and the negation of ψ is a truth. But then $\psi \& {\sim}\psi$ is a truth, and this, we know, is impossible.

This argument, however, is based on a confusion about the nature of Hilbert's position. According to Hilbert, sentences are purely syntactic items. To speak of a sentence being true or false, then, only makes sense if we have in mind a particular interpretation of that sentence. The argument made in the last paragraph misses this (important) point, as becomes clear if we examine it a little closer. That argument went as follows:

(1) There are two consistent sets, A and B, such that ψ is in A while $\sim\psi$ is in B.

Since, according to Hilbert, if a set of axioms is consistent, then it contains only truths, it follows that A contains only truths and B contains only truths. So

(2) ψ is a truth and $\sim\psi$ is a truth.

But it is plausible to hold that

(3) For any sentences, γ and δ, if γ is a truth and δ is a truth, then $\gamma\&\delta$ is a truth.

It follows from (3) and (2) that $\psi\&\sim\psi$ is a truth. This, however, is impossible. But this conclusion, I think, should not be interpreted so that it shows Hilbert's position false; rather, I think, it should be interpreted so that it shows (3) is wrong.

Strictly speaking, ψ is not an English sentence, but a formalization of a sentence of mathematical English; that is, both ψ and $\sim\psi$ are sentences of a formal logic. They are purely syntactic items and for all we know about them, they could be strings of numbers or sets of sets. It makes no sense to speak of such items being true or false without first giving them an interpretation. One way this can be done is relative to a structure or a class of structures. We can interprete the variables of, say, ψ to range over the universes of structures of a certain sort, and we can interprete the relation symbols of ψ using structures of that sort. If we agree that this is the way to go about interpreting ψ, a reasonable way to understand the statement that ψ is a truth is as the claim that there is a mathematically interesting class of structure in which we can interprete ψ and ψ is true in every member of that class. It is, of course, problemmatic what mathematically interesting classes of structures are; however, for our purposes, we can say that a class of structures is mathematically interesting if it is the class of models of some demonstrably consistent set of axioms. Now notice that, given this rough understanding, we can rewrite (2) as

(2') For some mathematically interesting class of structures, A, if $\mathfrak{M}$ is in A, then $\mathfrak{M} \models \psi$ and for some mathematically interesting class of structures, B, if $\mathfrak{N}$ is in B, then $\mathfrak{N} \models {\sim}\psi$.

(2') seems all right. However, if we similarly rewrite (3) as

(3') For any sentences, γ and δ, if for some mathematically interesting class of structures, A, and some mathematically interesting class of structures, B,

(i) if $\mathfrak{M}$ is in A, $\mathfrak{M} \models \gamma$
and (ii) if $\mathfrak{N}$ is in B, $\mathfrak{N} \models \delta$,

then for some mathematically interesting class of structures, C, if $\mathfrak{L}$ is in C, $\mathfrak{L} \models \gamma \& \delta$,

we obtain a false principle. Thus, if we accept Hilbert's view of mathematical truth (or at least my, perhaps anachronistic, reconstruction of it) we must reject (3), and so what seemed a refutation of Hilbert's position is not a refutation at all.

I should, however, mention that the view of mathematical truth presented above is foreign to Frege's views about mathematics and logic. Formal sentences, according to Frege, have sense independent of any structures in which they are interpreted. Indeed, according to

Frege which structures a formal sentence is interpreted in depends on its sense. This point should be clear after chapter three. Non-anomalous formal sentences (that is, formal sentences without what Frege would call "non-referring expressions") have truth-values independent of any structure in which they are interpreted. This is quite different from the view that motivated the rewriting of (2) as (2') and (3) as (3'). Indeed, I think, given what was said in chapter three about Frege's position, it is clear that Frege would have endorsed (3) and denied (2); exactly the opposite of what (my re-constructed version of) Hilbert would do. As we shall see in more detail, Frege and Hilbert had very different views about the nature of formal symbolisms. "The use of symbols," Frege said[3], "cannot be equated with a thoughtless mechanical procedure." Yet, as shall be seen, this is precisely what Hilbert, in some contexts, tried to do.

Let us end this section with the following (perhaps anachronistic) description of Hilbert's views. Hilbert thought that formal sentences had truth only relative to a (specified) sort of structure. A (formalization of a) mathematical theory, then, is a study of a sort of structure, namely, the sort of structure relative to which all the theorems of that theory are truths. So, all that is needed to show that a (formalization of a) mathematical theory is true is a demonstration that there are structures it studies. There are, as is well-known, at least two ways this can be done. We can either (1) show directly that some sentence, say '0=1', is not a theorem of the theory in question and then appeal to the completeness of its logic concluding that there are models of the theory; or we can (2) use a relative consistency proof, that is, we can translate the theorems of the theory into theorems of another theory that we already know has models and then conclude (using, by the way, clause (iia) of definition 5 in chapter two) that the former theory has models. These two methods, as shall be seen, play a central role in (my reconstruction of) Hilbert's philosophy of mathematics. In the next two sections, I shall take a detailed look at (this version of) Hilbert's philosophy of mathematics, to be followed in section four by an argument that goes from principles affirmed by it to the position that first-order logic is the only logic that should be used to formalize mathematics.

(ii)

Intuition is traditionally held to be the source of mathematical knowledge. Frege, for example, in order to explain how we are able to grasp geometric concepts, like _point_ and _line_, and to use them to formulate truths appealed to what he called "spatial intuition." It is, perhaps naively, thought that when a mathematician proves something he is appealing to intuitions that we all have. The mathematician who demonstrates that there is no largest prime is, on this view, examining concepts that we are all able to grasp -- like _is devisible by_ and _is the successor of_ -- and deriving facts using those concepts. This view has it that anyone, by appealing to properly trained intuitions, can "see" that any mathematical result is true. I shall call this rudimentary position "the naive view."

My characterization of the naive view is rather vague and imprecise. Nevertheless, I think, dressed up in different ways, the naive view represents the philosophy of mathematics of the nineteenth century. One reason the set theoretic paradoxes seemed so intellectually upsetting is that they demonstrated that methods that seem intuitively correct, in fact, lead to inconsistent results. The concept of a set that we intuitively arrive at, and the methods of inference consequently used to reason about sets, turn out to yield contradictory results. Hilbert's philosophy of mathematics, I think, is best seen as a reaction to this interpretation of the set theoretic paradoxes.

It should, however, be pointed out before we look at Hilbert's philosophy of mathematics that this interpretation of the set theoretic paradoxes is not uncontroversial. Kreisel argues that, as a matter of fact, the set theoretic paradoxes is not uncontroversial. Kreisel argues that, as a matter of fact, the set theoretic paradoxes confirmed the intuitions of working mathematicians, and that, therefore, the discovery of the set theoretic paradoxes vindicated the naive view. Although I think that Kreisel is wrong on this matter, it is worth outlining the reasons he gives for his view[4]. Kreisel suggests that mathematicians were bewildered by early research in set theory. Many thought that the theory of sets was not worth pursuing. Thus, when it turned out that that theory was inconsistent, Kreisel claims, the intuitions of working mathematicians turned out to be correct; the theory of sets, as it then stood, was inconsistent and not worth pursuing. Kreisel's view is, however, non-standard, and I shall not accept it here -- not because it is non-standard, but because it is easier to see the motivation for Hilbert's philosophy of mathematics if we keep in mind the more standard account of the set theoretic paradoxes.

In the beginning stages of set theoretic research, it seemed plausible that (as we might put it today) for every open sentence, $\psi(x)$, there is a set, Ξ, such that Ξ contains all and only those objects that satisfy $\psi(x)$. The intuitions of early set theorists, then, suggested that every instance of

$$(1)\quad (\exists x)\ (\forall y)\ [y \varepsilon x \leftrightarrow \psi(y)]$$

is a truth. Bertrand Russell and Zermelo, as is well-known, showed this to be untenable.[5] There are many instances of (1) that are false.

$$(2)\quad (\exists x)\ (\forall y)\ [y \varepsilon x \leftrightarrow y \not\varepsilon y]$$

is perhaps the most famous. Thus, appeals to intuitions, even well-trained ones, contrary to the naive view lead to contradictions.

Faced with the set theoretic paradoxes, philosophers of mathematics have a dilemma: either (1) deny that mathematicians uncover truths we know with certainty, while accepting the basic substance of the naive view; or (2) replace the naive view with something else, a position able to justify the view that the results a mathematician uncovers are certainly true, while at the same time explaining (away) the set theoretic paradoxes. Hilbert's philosophy of mathematics is one result of following the second strategy.

Hilbert hoped to establish the certitude of mathematical methods once and for all. "If mathematical thinking is defective," he plaintively asked[6], "where are we to find truth and certitude?" Hilbert accepted what was then the standard view of the set theoretic paradoxes, namely, that the set theoretic paradoxes resulted because transfinite methods were used in an illegitimate way[7]. In particular, Hilbert believed, early set theorists mistakenly generalized facts about bounded quantification to draw conclusions about unbounded quantification, although he probably would not have put the matter in this fashion.

This is not an unnatural position to be led to. Paradoxes do not result if we limit our attention to bounded formulas, that is, formulas in which no unbounded quantifiers occur. All instances of

$$(1') \quad (\exists x \varepsilon A)\ (\forall y \varepsilon b)\ [y \varepsilon x \leftrightarrow \psi(y)]$$

are not truths, but our intuitions do not suggest that they are. What our intuitions do suggest is true is that for every set, B, and open sentence, $\psi(y)$, we can find a set, A, such that the relevant instance of (1') is a truth; and it is not legitimate to infer that every instance of (1) is true from this fact about instances of (1')[7a]. Hilbert (on one interpretation) thought that by examining closely just what we can say using bounded quantifiers and investigating how far we can extend our use of bounded quantifers to a use of unbounded quantifiers without engendering contradiction, we might eliminate paradoxes and begin to establish the certitude of mathematical methods.

Hilbert's philosophy of mathematics, as I understand it, is a response to the set theoretic paradoxes[7b] and is based on three core ideas. First, Hilbert stressed that there are finitary reasonings that lead to conclusions we know are certainly true. As I understand Hilbert (and as I shall argue below), he thought that all finitary reasoning is a part of arithmetic. So this first idea of Hilbert's can be put as: there is a set of rules of inference in arithmetic (the rules of finitary inference) that lead to conclusions we know are certainly true. Such rules allow us to show that two plus two is four, or that 15077 is a prime number. The principles of finitary reasoning are simple arithmetic computations, involving only a finite set of integers, yielding a result after finitely many steps.

It should be mentioned, before going any further, that a good deal of controversy surrounds the question what exactly Hilbert thought finitary reasoning is[8]. In the next section, I shall give what appears to be a definition of finitary reasoning. However, it should be stressed now (it will be stressed again) and remembered when reading that section that the characterization of finitary reasoning there given is not meant to be definitive. I do not pretend to be able to give a definitive characterization of what Hilbert thought finitary reasoning is, nor do I want in this work to become embroiled in that controversy. All characterizations of finitary reasoning I put forward are very tentative speculations. How finitary reasoning is characterized does not, I think, affect the main arguments of this chapter (although it must be the case that finitary reasoning is a part of arithmetic. This, however, is not very controversial.)

The second core idea on which Hilbert's philosophy of mathematics is based is the claim that transfinite notions and methods are creations. We make them up. There is a sense, according to Hilbert, in which there are no infinite totalities. He claims that statements like

> For every natural number, n, there is a prime number, m, such that m>n,

which seems to imply that there is an infinite totality of prime numbers, are really ideal statements without sense or meaning that are only used in order to "tidy up" our theories. Hilbert says[8a] that "in general [a statement involving transfinite notions] has meaning only as a partial proposition, that is, as part of a proposition that is more precisely determined but whose exact content is unessential for many applications."

The third core idea of Hilbert's philosophy of mathematics is, perhaps, the most well-known. According to Hilbert, the only criterion that must be met by the ideal statements (such as the one displayed in the preceding paragraph) is that they are consistent with the set of true real statements (those we can show true using finitary reasoning). It is this last idea that has led to much of the discussion of Hilbert's philosophy. Hilbert claimed[9] that

> we must establish throughout mathematics the same certitude for our deductions as exist in ordinary number theory, which no one doubts, and where contradictions and paradoxes arise only through our own carelessness.

According to Hilbert, then, there is a core of arithmetic, "ordinary number theory" that contains truths we can know with certainty. He hoped to base all mathematics on arithmetic (as I shall soon argue) and to show that all arithmetic can be known with certainty by demonstrating that all arithmetic -- that is, ordinary number theory cum transfinite methods -- is consistent with that part of arithmetic "which no one doubts." Furthermore, he hoped to demonstrate this consistency using only principles "which no one doubts" -- that is, he wanted his consistency proof to be a part of ordinary number theory; thus establishing "throughout mathematics the same certitude...as exist[s] in ordinary number theory."

It is well-known how Gödel demonstrated that this last part of Hilbert's philosophy is, unfortunately, a pipe dream. The consistency of number theory cannot be established using only number theory; hence, it surely cannot be established using what Hilbert called "ordinary number theory" -- or so Gödel's results have been interpreted.

In the next section, I shall present a more rigorous treatment of Hilbert's philosophy, so that some of these issues can be discussed more clearly. In section four, I shall then present an argument that goes from principles of Hilbert's philosophy of mathematics to the conclusion that Hilbert's thesis is true.

(iii)

Let us start by considering a very simple language[9a], N, called "the language of arithmetic." N consists of 0, a constant symbol, s( ), a one-place function symbol, ( )+( ) and ( )·( ), two place function symbols, and <, a binary relation symbol. I am interested in generating a certain class of sentences from N, called "the ordinary sentences of arithmetic." To do this, take # to be the minimal syntax (in an obvious sense of minimal), *, such that where

(1) All variables of kind 0 are terms, and the constant 0 is a term

and

(2) If α and β are terms, then $s(\alpha)$, $(\alpha)+(\beta)$ and $(\alpha)\cdot(\beta)$ are terms

the following hold:

(3) If α and β are terms, then $\alpha=\beta$ and $\alpha<\beta$ are members of N^*[10]

(4) If γ and δ are in N^*, then $\gamma \& \delta$, $\gamma \vee \delta$, $\gamma \rightarrow \delta$, $\gamma \leftrightarrow \delta$ and $\sim\gamma$ are members of N^*

(5) If γ is in N^*, x is a variable of kind 0 and α is a term, then $(\exists x<\alpha)\gamma$ and $(\forall x<\alpha)\gamma$ are in N^*.

It should be obvious that there are syntaxes satisfying (1)-(5) and that # is, therefore, well-defined.

The members of $N^{\#}$ have their usual interpretations. So let $\not\models$ be the relation between structures and ordinary sentences of arithmetic that works out as we would expect. For instance, $\mathfrak{M} \not\models (\exists x<\alpha)\gamma$ if and only if $\mathfrak{M} \models_{fo} (\exists x)(x<\alpha \& \gamma)$, where $\models_{fo}$ is as in chapter two. (As it turns out $<\#, \not\models>$ is not a Barwise logic (see definition 5, chapter two), but this need not concern us.) We can now construct a formalization of a portion of mathematics[10a], Ξ_0, whose first component is N whose second component is a set of ordinary sentences of arithmetic and whose third component is $<\#, \not\models>$[10b] such that if ψ is a member of $N^{\#}$, then ψ is true in the standard model of arithmetic only if ψ is Ξ_0-valid.

For the purposes of exposition, <u>not as a definitive characterization</u>, I shall take Ξ_0 to formalize finitary arithmetic, that part of mathematics that, according to Hilbert, "no one doubts and where contradictions and paradoxes arise only through our own carelessness." As would be expected of a formalization of this fragment of arithmetic, there is a proof-system, $_0$, such that if ψ is Ξ_0-valid, then ψ is $_0$-provable.

According to my interpretation of Hilbert, Ξ_0, is a formalization of the contentful part of arithmetic (and hence, as shall be seen, of mathematics). A sentence of arithmetic has meaning, according to Hilbert, only if it can be formalized as a member of $N^{\#}$. These sentences are what Hilbert called "the real sentences of mathematics." Undoubtedly this was based on a particular view about what a theory of meaning for mathematical sentences should be like. It is not unusual to suppose that a theory of the meaning of mathematical sentences is a theory of computation, and this is what, I think, Hilbert had in mind. On this view, we describe the meaning of a mathematical sentence if we describe a type of computation that would show that sentence true or false. It is the existence of such a theory for the real sentences of mathematics (the sentences of $N^{\#}$) that, I think, makes it plausible to call those sentences the contentful part of arithmetic. However, according to Hilbert, Ξ_0 does not formalize adequately <u>all</u> arithmetic. Ξ_0 does not formalize those portions of arithmetic that use transfinite methods. There are many sentences ordinarily accepted by mathematicians that cannot be adequately formalized as ordinary sentences of arithmetic, and Hilbert therefore proposed that we extend Ξ_0 so that a larger portion of mathematics can be formalized.[10c]

Hilbert gave essentially two reasons for extending Ξ_0. It is not hard to see that

(A) $0<x\rightarrow[0<x+s(0)]$

is Ξ_0-valid; it is an ordinary sentence of arithmetic and it is true in all standard models of arithmetic. Thus if a structure does not satisfy

(A) it is not a standard model of arithmetic. That is to say: if a structure satisfies the negation of (A), it is not a standard model of arithmetic. But what sentence is the negation of (A)? In other words, what sentence is true in all and only those structures that do not satisfy (A)? It is not hard to see that there is no ordinary sentence of arithmetic that can be taken as the negation of (A). We know that (A) is satisfied in a structure, $\mathfrak{A}$, if and only if no matter what element of the universe of $\mathfrak{A}$ is assigned to x, (A) -- so interpreted -- is true in $\mathfrak{A}$. That is, $\mathfrak{A}$ satisfies (A) if and only if

$$(A'') \quad \mathfrak{A} \Vdash (0<x \rightarrow [0<x+s(0)]) \; [\sigma]$$

(in the sense defined on page 61) for all sequences, σ, over the universe of $\mathfrak{A}$. But there is no sentence, γ, in $N^{\#}$ such that $\mathfrak{A} \not\models \gamma$ if and only if there is a sequence, σ, over the universe of $\mathfrak{A}$ for which (A'') is false. There is, then, no way of formalizing the negation of (A) using Ξ_0; and so there is no way of capturing the classical rules of inference using only the logic $<\#, \not\models>$.

Hilbert therefore proposed that Ξ_0 be extended to a new system, Ξ_1, in which there is a negation of (A). This new system will consist of the language of arithmetic, a set of axioms extending the axioms of Ξ_0, and a new logic extending $<\#, \not\models>$. So long as every ordinary sentence of arithmetic that is Ξ_1-valid is Ξ_0-valid, that is, so long as Ξ_1 is a conservative extension of Ξ_0, there is, according to Hilbert, nothing objectionable about introducing a negation of (A) in this manner. Ξ_1' will be a tidier system than Ξ_0, although Ξ_0 is the "real" theory of arithmetic, while Ξ_1 contains ideal sentences that have no genuine content.

(The choice of (A), of course, was completely arbitrary; any open sentence in $N^{\#}$ could have been used to obtain a reason for introducing ideal sentences, like the negation of (A), into our formalization of arithmetic.)

There is another kind of reason Hilbert gives for extending "ordinary number theory, which no one doubts," that is, I think, more interesting. As I interprete Hilbert it is based on the idea that if we can recognize that all the members of a certain subset of $N^{\#}$ are true, then, in some cases, there should be a sentence that is true in all and only those structures in which all the members of this subset of $N^{\#}$ are true. Hilbert thought that the introduction of such a sentence gives us no new information and therefore should not be objectionable.

In order to illustrate this second reason for introducing ideal sentences, Hilbert used the following example. Let p be the greatest known prime number. Using only finitist methods, Hilbert claimed, we can show both that (i) p is a prime and that (ii) p is the greatest known prime -- since at any time there will be only finitely many known primes. However, also using finitist methods, we can show that there is a number greater than p that is also a prime number. If we multiply together all non-zero numbers less than or equal to p and then add one to this product, we obtain a number that we can show, using finitist methods, to be greater than p and prime. Letting n! have its usual meaning, we therefore have it that no matter what the value of p may be:

(6) If $\underline{p}$ is a prime, then there is an $x \leq \underline{p!+1}$ and x is a prime greater than $\underline{p}$

can be formalized as a Ξ_0-valid sentence. (Here, as usual $\underline{n}$ is the numeral for n.) So it looks as if no matter what prime is the greatest known prime, we can show that it is not the greatest prime -- and we can do so using methods that can be formalized using Ξ_0. It looks, then, as if we have established, using finitary methods, that there are infinitely many primes. Care, however, is necessary.

Although every <u>instance</u> of (6) can be formalized as a true, ordinary sentence of arithmetic, (6) itself cannot be so formalized. Although for any particular number, n, we have a method in finitary arithmetic for going from n to its factorial , n!, we do not have a method in finitary arithmetic of going from any number to its factorial[10d]. Thus, strictly speaking, (6) should be interpreted as a schema whose instances are formed by a rule -- not expressible in Ξ_0 -- with the peculiar property that all instances of (6) formed in accordance with that rule are formalizable as Ξ_0-valid sentences. So <u>every</u> (properly formed) instance of (6) is a true, ordinary sentence of arithmetic, that is, every instance of (6) is true in every standard model of arithmetic[10d']. There is, however, no ordinary sentence of arithmetic that is true in all and only those structures in which every (properly formed) instance of (6) is true. Hilbert proposed that Ξ_0 be extended, as before, so that there is such a sentence. As before, according to Hilbert, the only condition that our extension of Ξ_0 must satisfy is that it be conservative, for, then, Hilbert thought, no genuine content is added to arithmetic so that the content of arithmetic might be presented (and studied) using a tidier theory.

We might characterize Hilbert's thoughts on these matters as follows. We start with a formaiization of a portion of mathematics, Ξ_0, which formalizes the "real" sentences of arithmetic, those that have what Hilbert thought was content. For the sorts of reasons described above, Ξ_0 should be extended to another formalization, Ξ_1. By continuing in this manner we obtain a sequence of formalizations: $\Xi_0, \Xi_1, \Xi_2, \ldots$. The limit of this sequence is the (or a) correct formalization of arithmetic -- and, hence, as shall be seen, according to Hilbert can be used to formalize adequately all of mathematics.

It should be noticed that the particular system with which I started is irrelevant. There may be good reason for denying that Ξ_0 is an adequate formalization of finitist arithmetic. If that is the case, then we can simply replace Ξ_0 with an adequate formalization of finitist arithmetic without affecting the main arguments of this chapter.

(iv)

In this section I shall use the above (perhaps anachronistic) characterization of Hilbert's philosophy of mathematics to construct a plausible (but by no means conclusive) argument for Hilbert's thesis. The reader should consult chapter two, especially definition 16 and Lindstrom's theorem on page 72. What I shall suggest is that Hilbert's philosophy of mathematics is committed to the view that any logic used when formalizing mathematics contains first-order logic (and is a Lindstrom logic), is complete and satsifies the Löwenheim property. Hence, using Lindstrom's theorem, it can be deduced that first-order logic is the logic that should be used when formalizing mathematics. So, what I have to do in this section is show three things: (I) that Hilbert thought (or can be construed to have thought) that a Lindstrom logic should be used when formalizing mathematics; (II) that Hilbert thought (or can be construed to have thought) that any logic used when formalizing mathematics should be complete; and (III) that Hilbert thought (or can be construed to have thought) that any logic used when formalizing mathematics should have the Löwenheim property. (I)-(III) will show, by Lindstrom's theorem, that Hilbert was committed to Hilbert's thesis; that is, 'Hilbert's thesis' is not a misnomer.

It should be noticed that this is not an unsurprising result. Nothing that Hilbert says about mathematics, nor anything I said above when characterizing his philosophy of mathematics suggests that *he* thought that Hilbert's thesis is true. Indeed, on the fact of it, there is no reason why we should not expect that included among the ideal sentences of arithmetic is a sentence stating that every number has finitely many predecessors. Indeed, for each number, n, we can write a sentence that seems to state that n has only finitely many predecessors, for example:

$$x < n \rightarrow [x=o \vee x=1 \vee \ldots \vee x=n-1].$$

So why should we not expect that a sentence stating that every number is finite should not be a part of our formalization of arithmetic? Using infinite disjunctions, for example, we can suitably generalize the above sentence so that a sentence stating that every number is finite can be formed. Why can't we add on such a sentence as an ideal element?

Nothing in Hilbert's philosophy of mathematics directly says that we cannot. In fact, Hilbert himself seems to have thought that infinite conjunctions and disjunctions could be introduced as ideal elements. In "On Infinity" Hilbert[11] seems to have wanted to interprete $\exists x\psi(x)$ as

$$(7) \quad \psi(0)\vee\psi(1)\vee\psi(2)\vee\ldots \quad .$$

He suggests that just as $\exists x^{\leq n}\psi(x)$ is equivalent to $\psi(0)\vee\psi(1)\vee\ldots\vee\psi(n)$, so $\exists x\psi(x)$ is equivalent to (7). Thus, Hilbert's thesis is not an obvious consequence of Hilbert's philosophy of mathematics.

We already saw that Hilbert appeared to be committed to the claim that every logic used to formalize mathematics should be complete. We saw in chapter three that he claimed that statements of a mathematical theory "have validity only if one can derive them from...axioms by a finite number of logical inferences." If we agree, as in chapter two (page 71), that logical rules of inference can be effectively generated, we can interpret Hilbert here as straightforwardly endorsing (HC), the claim that only complete logics should be used when formalizing mathematics. But there are more profound reasons why, I think, Hilbert was committed to (HC).

On Hilbert's view, mathematics rests on arithmetic in a very important sense. The only part of mathematics that has "content" is a portion of arithmetic; the "real sentences" of mathematics are all sentences of finitist arithmetic. It is only this fragment of mathematics, according to Hilbert, that we know is certainly true independently of any consistency proofs. By then appealing to consistency proofs, Hilbert believed that all arithmetic (and all mathematics) could be secured as steadfastly as finitist arithmetic. By showing a mathematical theory consistent, Hilbert thought, we show that its theorems are truths. But what connection is there between the consistency of a theory and the truth of a theory's theorems? On pages 114-5, I argued that, according to Hilbert, a mathematical theory is true just in case it has models. Thus, if we can show that a theory has models, we can show that its theorems are truths. The gap between the consistency of a theory and the truth of its theorems is bridged by the completeness of that theory's logic. For if a theory's logic is complete, then the theory is consistent only if it has models. It seems natural to conclude, then, in light of the above considerations, that Hilbert would endorse (HC), the claim that only complete logics should be used to formalize mathematics.

Hilbert (or, at least, my reconstruction of him) was, then, committed to (HC). But what about the claim that every logic used when formalizing mathematics should satisfy the Löwenheim property? Might not it turn out, for instance, that one of the ideal sentences added onto arithmetic states that nothing has uncountably many predecessors, a statement most naturally made using Keisler's quantifier, "there exist uncountably many, x, such that"? The resulting logic would, as already noted be complete, but would not have the Löwenheim property. Why would Hilbert object to using such a logic when formalizing mathematics?

We can answer this question by noting, first, that, on Hilbert's view, there is no reason not to take 'something has uncountably many predecessors' as an axiom of some non-arithmetic mathematical theory, if 'nothing has uncopuntably many predecessors' is an axiom of arithmetic. According to Hilbert (see page 112), "if the *arbitrarily* given axioms do not contradict one another, then they are true." [emphasis added] So *any* axioms can be used -- so long as they do not contradict one another -- to construct a mathematical theory. But, according to Hilbert, as I understand him, only arithmetic can be proved to have a model by using consistency proofs. Other mathematical theories are shown to have models by reducing them to arithmetic, by translating their theorems into the language of arithmetic and showing that the translated theorems are consistent with the sentences of finitist arithmetic. The content of all mathematics is found in finitist arithmetic. So, on Hilbert's view, the way to show that any theory other than arithmetic has a model is to construct an arithmetic model (as described) for it. Thus, according to Hilbert, every non-arithmetic mathematical theory has an arithmetic -- hence denumerable -- model. The Keisler quantifier, then, as I interpret Hilbert, should not be used when formalizing a mathematical theory. According to (my version of Hilbert), every logic used when formalizing mathematics should satisfy the Löwenheim property.

This is brought out in the following passage from "On Infinity"[12]

> ...[T]he problem of proving consistency arises wherever the axiomatic method is used. After all, in selecting, interpreting, and manipulating the axioms and rules we do not want to have to rely on good faith and pure confidence alone. In geometry and the physical theories the consistency proof is successfully carried out by means of a reduction to the consistency of the arithmetic axioms. This method obviously fails in the case of arithmetic itself...[O]ur proof theory forms the necessary keystone in the edifice of the axiomatic theory.

It is natural to interpret Hilbert's claims here, as I have done, so that Hilbert is committed to the view that non-arithmetic theories are shown consistent by showing them to have denumerable models. We can conclude then that (one natural interpretation of) Hilbert's philosophy is committed to the view that every mathematical theory has an arithmetic model; and hence that the logic used when formalizing mathematics should have the Löwenheim property.

It should be mentioned before continuing that Hilbert sometimes used 'arithmetic axioms' to mean the axioms for the real number system.[12a] Thus, the above quotation from "On Infinity" might be interpreted to mean that every (acceptable) mathematical theory has a model in the real numbers. However, I do not think, given what has been said above, that this is the correct interpretation of the quoted passage. As I described it above, Hilbert's philosophy of mathematics does not seem able to accord the theory of real numbers so special a status. Hilbert's philosophy of mathematics was motivated by the desire to establish the certitude of mathematical methods. He tried to do this by showing, first, that there is a portion of mathematics (finitist arithmetic) that cannot be doubted, and, second, that methods of mathematics that are not a part of finitist arithmetic are "ideal creations" that can be shown consistent with the methods of finitist arithmetic. Under this description, there is no reason to accord real number theory a special position in Hilbert's philosophy of mathematics. Indeed, the methods of real number theory clearly outstrip the methods of finitist arithmetic. For instance, presumably using real number theory we can prove that there are uncountably many real numbers, and, thus, that there are uncountably many number theoretic functions (that is, functions whose arguments are natural numbers and whose values are natural numbers)[12b]. But, on any interpretation of Hilbert, the set of finitist number theoretic functions (that is, number theoretic functions whose existence can be proved using finitist arithmetic) is countable. For example, in a recent article[12c], Tait argues that the finitist number functions are just the primitive recursive functions. As we know, the set of all primitive recursive functions is countable. Similarly, if Kreisel is right[12d], and the finitist number theoretic functions are those that are first-order definable, then there are only countably many finitist number theoretic functions. Thus, the methods of real number theory are "ideal creations" in need of justification, like the methods of most other portions of mathematics. According it a special status (as we would have to do if 'arithmetic axioms' refers to the axioms for the real numbers as used in the above quote from "On Infinity") is, therefore, unjustified, given my description of Hilbert's philosophy of mathematics.[13]

So far it has been seen that (my reconstruction of) Hilbert's philosophy of mathematics is committed to the claims that logics used when formalizing mathematical theories should be complete and that they should have the Löwenheim property. So, two-thirds of what I set out to do in this section has been completed; (II) and (III) (on page 130), I think, have been established. What is needed now is an argument for (I), an argument that (my reconstruction of) Hilbert's philosophy of mathematics is committed to the claim that a Lindstrom logic should be used when formalizing mathematics. For, then, we can appeal to Lindstrom's theorem to conclude that (my reconstruction of) Hilbert's philosophy is committed tothe claim that nothing stronger than first-order logic should be used when formalizing mathematics.

Inspecting definition nine (page 63) shows that at least two things must be established if we are to conclude that (my reconstruction of) Hilbert's philosophy is committed to the claim that a Lindstrom logic should be used when formalizing mathematics. It must be shown, first, that a Barwise logic (see definition five) should be used, according to (my version of) Hilbert's views; and, second, it must be shown that logics used to formalize mathematics should contain first-order logic. Now, it should be clear that the question whether Hilbert thought only Barwise logics should be used when formalizing mathematics is moot. It was beyond Hilbert's means to formulate the notion of a Barwise logic; at the time he wrote, model theory (if it can be said to have existed then) did not have rich enough notions to characerize a Barwise logic. Nevertheless, I think it is reasonable to look at Hilbert's work as if he thought that a Barwise logic should be used when formalizing mathematics. Indeed, (on page 117) I parenthetically noted that one way Hilbert thought we could prove the consistency of a mathematical theory depends on something like clause (iia) of the definition of a Barwise logic (page 59). So, without much argument, I shall read Hilbert as if he thought that Barwise logics should be used when formalizing mathematics.

It should be stressed, however, that this reading is not uncontroversial. It might be claimed that Hilbert thought of logic in a purely syntactical way and that, since a Barwise logic is characterized using model theoretic notions, it is a gross distortion of Hilbert's views to read them as if he thought only Barwise logics should be used when formalizing mathematics. There may be something to be said for this view; I shall, however, disregard it for two reasons. The first is idiosyncratic. I said in chapter two that I was going to assume throughout this essay that only Lindstrom logics, and, hence, only Barwise logics, should be used when formalizing mathematics. The second reason is not so rooted in the assumptions made in this work. In fact, I do not think it distorts Hilbert's views to read them as if he used model theoretic notions. First, if, as argued above, Hilbert was committed to logic's completeness, then he did not have a purely syntactical view of logic. Completeness involves the notion of validity, and validity is a model theoretic notion. Also, if my arguments in the first section of this chapter are correct, then mathematical truth, as Hilbert thought of it, is a model theoretic notion. The use of model theoretic notions, when interpreting Hilbert, is not (necessarily) to distort his views. I shall, therefore, read Hilbert as if he thought that only Barwise logics should be used when formalizing mathematics.

This is not yet to claim that Hilbert thought only Lindstrom logics should be used when formalizing mathematics. We still must see whether we can read Hilbert as if he thought that first-order logic should be contained in logics used when formalizing mathematics. This question cannot be handled as easily as the question whether Hilbert can be read as if he thought only Barwise logics should be used. Detlefsen[14] claims, for instance, that the quantifiers of the logic Hilbert proposed we use when formalizing arithmetic are different from the first-order ones. In particular, Detlefsen claims, Hilbert was committed to an ω-rule being valid, and this, as we now know, is impossible, if the quantifiers used are standard. So, it looks as if to read Hilbert as if he thought first-order logic should be a part of the logic used when formalizing arithmetic is to distort Hilbert's views about what sorts of rules the quantifiers satisfy.

Detlefsen's point seems well taken. I noted above (page 131) that Hilbert seems to have wanted to interpret $\exists x\psi(x)$ as an infinite disjunction; so $\forall x\psi(x)$ would be an infinite conjunction, and the ω-rule would be satisfied. Thus, it seems that a good case can be made against reading Hilbert's existential (and universal) quantifiers as if they were quantifiers of first-order logic. Nevertheless, I shall do so for several reasons. One is the internal reason that doing so allows me to use Hilbert's philosophy of mathematics without violating the presuppositions of this essay, in particular the presupposition that only Lindstrom logics should be used when formalizing mathematics. But there is another reason as well. Hilbert, we saw, hoped to preserve the laws of classical logic and he thought that logic is complete. But logic cannot be complete and the laws of classical logic preserved if the quantifiers (used in arithmetic) satisfy an ω-rule (unless we think that an ω-rule is a rule of inference, a belief I shall not entertain). Something must be rejected. As I shall read Hilbert, it is the belief that the quantifiers satisfying an ω-rule. Thus especially in light of the presupposition of this essay, it is reasonable to read Hilbert _as if_ he thought first-order logic is a part of any logic used when formalizing mathematics. We can conclude then that (my reconstruction of) Hilbert's philosophy of mathematics is committed to the claim that only Lindstrom logics should be used when formalizing mathematics.

It is thus possible to construct, using Lindstrom's theorem, an Hilbertian argument for Hilbert's thesis. We have seen in this section that (my reconstruction of) Hilbert's philosophy of mathematics is committed to the claims that (I) only Lindstrom logics should be used when formalizing mathematics, (II) only complete logics should be used when formalizing mathematics, and (III) only logics having the Löwenheim property should be used when formalizing mathematics. Given these three claims Lindstrom's theorem entails that only first-order logic should be used when formalizing mathematics.[15]

(v)

The Hilbertian argument for Hilbert's thesis described in the last section makes use of two premises. First, in order to conclude that any logic used when formalizing mathematics should have the Löwenheim property, I attributed the following principle to Hilbert:

(Arithmetic's Priority)	It is possible to construct an arithmetic model for any mathematical theory.

I also attributed to Hilbert the belief that (HC) is true, that is that every logic used when formalizing mathematics should be complete. These two premises are not unrelated. One reason I gave for attributing (HC) to Hilbert is that he thought a demonstration of the consistency of a set of axioms showed that those axioms have a model, and, hence, (see section one) are true. Furthermore, Hilbert thought that arithmetic can be used to demonstrate its own consistency, hence, by (HC), that it has a model. So, Hilbert thought that arithmetic could be used to demonstrate that it is true (again, this follows from the discussion in section one). Hilbert also hoped to base mathematics on arithmetic by constructing arithmetic models for each (acceptable) mathematical theory; this is the reason I attributed (arithmetic's priority) to him. In this way, Hilbert hoped to establish the truth of all mathematics.

In the final sections of this chapter, I shall look once more at (HC). I shall look closely at two (fairly) standard objections to Hilbert's philosophy of mathematics, seeing now (HC) fares in their light. Both objections I consider are directed against (HC); however, as we shall see they have implications regarding (arithmetic's priority) as well.

Steiner and others (notably Putnam) have suggested that Euler's argument that the infinite sum of all numbers of the form $1/n^2$ is $\pi^2/6$ an example of a good, sound and acceptable argument of mathematics -- on equal footing with proofs -- that is not a proof and that is, therefore, not formalizable as a derivation (see chapter one for a discussion of the difference between proofs and derivations)[16]. Indeed, they claim that Euler's argument establishes that the value of the infinite sum in question is $\pi^2/6$, and that Euler, therefore, knew that its value is $\pi^2/6$, even though not one mathematician at the time Euler made his argument was able to give a proof of this fact. Thus, if we were to formalize mathematics at the time of Euler, it can be continued, we would have to formalize a statement,

$$(\alpha) \quad \sum_{n=1}^{n=\infty} [1/n^2] = \pi^2/6,$$

as a valid statement of mathematics, even though it has no derivation. Thus, our formalization of the theory of infinite summation at the time of Euler would violate (HC); not every valid sentence of that formalization would have a derivation. Let me quote Putnam extensively so that we may see the purported significance of Euler's argument.

> The use of quasi-empirical methods [that is arguments that are not proofs] in mathematics is not by any means confined to the testing of new axioms...Although it is rare that either mathematicians or philosophers discuss it in public, quasi-empirical methods are constantly used to discover truths or putative truths that one then tries to prove rigorously. Moreover, some of the quasi-empirical arguments by which one discovers a mathematical proposition to be true in the first place are totally convincing to mathematicians. Consider, for example, how Euler discovered that the sum of the series $1/n^2$ is $\pi^2/6$...Euler, of course, was perfectly aware that this was not a proof. But by the time he had calculated $1/n^2$ to thirty or so decimal places and it agreed with $\pi^2/6$, no mathematician doubted that the sum of $1/n^2$ was $\pi^2/6$, even though it was another twenty years before Euler had a proof.[17]
>
> [Quasi-empirical] methods are the source...of new theorems, that we often know to be true <u>before</u> we succeed in finding a new proof.[17a]

According to Putnam, then, quasi-empirical methods are part of the heart and soul of mathematics. They are used not only to discover new truths of mathematics, but (and this is important) to establish truths. It is a small step from this claim to the conclusion that (HC) is false. If proofs do not exhaust our methods of mathematical argumentation, formalizations should reflect this fact; one way to insure that they do, is to insist that (sometimes) the logic used when formalizing a fragment of mathematics not be complete.

It should be noted, however, that the use of quasi-empirical methods by mathematicians to establish truths is not enough to lead to the conclusion that (HC) is false, that logics used to formalize mathematics should not always be complete. We might hold, for instance, that Euler's argument establishes (α) and that every logic used to formalize mathematics should be complete ((HC)). There is a difference between being unable to prove (α) because we have not yet discovered a proof and being unable to prove (α) because (α) does not have a proof. If we think that quasi-empirical methods can be used to establish (α) before we are able to prove (α) (even though (α) does have a proof), then there does not seem to be any reason to deny (HC). However, if we think that quasi-empirical methods can be used to establish (α) and that (α) cannot be proved not because its proof has not yet been discovered, but because it does not have a proof, then it is possible to construct an argument against (HC). In the latter case, we might want (α) to be formalized as a valid sentence of Euler's theory of infinite summation, but we would not want (α) to be formalized as a sentence with a derivation (since, we agreed, it has no proof in that theory). But, then, in light of the definitions in chapter two, it is natural to suppose that the logic used when formalizing Euler's theory of infinite summation cannot be complete, that is, (HC) is false.

It should be noted that Putnam[17b] does not claim that Euler was unable to prove (α) because (α) had no proof. Putnam claims that Euler's argument does establish (α), but he does not claim that (α) has no proof. On the other hand, Steiner does seem willing to make the stronger claim. He suggests that Euler did not know a proof of (α), although he knew an argument for (α), because

> had Euler attempted to set down all the premises of his argument in mathematical detail and precision, he would undoubtedly have written falsehoods -- for the analogy between the finite and the infinite often breaks down...[17c]

Thus, according to Steiner, in Euler's theory of infinite summation there was no deduction of (α) from true axioms; hence there was no proof of (α) in Euler's theory. So Steiner, unlike Putnam, does seem wont to claim that (α) had no proof in Euler's theory of infinite summation; not merely that Euler was unable, at the time he made his argument, to prove (α).[17d] Steiner, then, makes claims that can be used as above to argue that (HC) is false.

There is also another way Euler's argument can be used to argue against (HC), the claim that all logics used in formalizations of mathematics should be complete. It is not only relevant that Euler's argument establishes (α), even though it is not a proof of (α); it is also relevant that (α) is a sentence of a certain sort. Since (α) can be spelled out so that it says that the limit of a certain sequence is $\pi^2/6$, (α) is equivalent to a sentence of the form

(*) For every m, there is an n such that $\varphi(m,n)$.

We know that when a complete logic is used in a formalization of mathematics, there will be a sentence, $\varphi(m,n)$, such that the relevant instance of (*) is not valid, even though for every m

(¢) There is an n such that $\varphi(\underline{m},n)$

is valid. We will see below that much of the evidence Euler used to establish (α) involved showing that for all values less than a large value of m, the relevant version of ($\not{c}$) holds. If such methods are in general applicable, then it seems to follow that if ($\not{c}$) holds for every m, (*) will hold. This, in turn, entails that the logics used in some acceptable formalizations of mathematics are not complete, that is, that (HC) is false.

What I shall call "Steiner's argument", then, contains six claims.

(1) Euler's argument establishes (α).

Therefore,

(2) Euler knew that (α) is true, and his contemporaries who were familiar with his argument knew that (α) is true.

But

(3) Neither Euler nor one of his contemporaries (for a while) could prove (α).

Therefore,

(4) It is possible to know (α) without there being a proof of (α).

Also,

(5) Formalizations of mathematical theories should reflect this fact (especially formalizations of Euler's and his contemporaries theory of infinite summation).

So,

(6) Some mathematical theories should be formalized using a logic that is not complete, that is, (HC) is false.

The details of why Steiner thinks that (4) follows from (3) were hinted at above and will be discussed in more detail below. Also, in all fairness to Steiner, it must be emphasized that he does not conclude (6), nor does he formulate (5). I have included (5) and (6) under the title "Steiner's argument" so that we may see how Steiner's claims are relevant to the subject of this essay.

Before looking closer at Steiner's argument, it will be helpful to examine Euler's argument for (α) in a little detail. Euler was able to prove that if an equation is of the form

$$(\beta)\quad b_0 - b_1 x^2 + b_2 x^4 - \ldots + (-1)^n b_n x^{2n} = 0,$$

where the b_i are real numbers, and if that equation has 2n different roots, $r_1, -r_1, r_2, -r_2, \ldots, r_n, -r_n$, then

$$(A)\quad b_1 = b_0 \,(1/r_1^2 + 1/r_2^2 + \ldots + 1/r_n^2).$$

(The details of this proof are irrelevant.) Furthermore, Euler was able to prove that if $\sin(x) = 0$, then

$$(B)\quad x - \frac{x^3}{3!} + \frac{x^5}{5!} - \frac{x^7}{7!} + \ldots = 0.$$

Euler then divided both sides of (B) by x, obtaining

$$(c)\quad 1 - \frac{x^2}{3!} + \frac{x^4}{5!} - \frac{x^6}{7!} + \ldots = 0.$$

Since it was assumed that $\sin(x) = 0$, Euler knew that (B) has the roots: 0, π, $-\pi$, 2π, -2π, ..., $n\pi$, $-n\pi$, ... So, since (C) resulted from (B) by dividing both sides of (B) by x, Euler was able to conclude that (C) has the roots: π, $-\pi$, 2π, -2π, ..., $n\pi$, $-n\pi$, ... But now Euler noticed that if we let $b_0 = 1$, $b_1 - 1/3!$, $b_2 = 1/5!$, ..., (C) can be seen as an infinite version of (β). So since (β) leads to (A), and (C) is an infinite version of (β), Euler concluded that (C), by analogy, leads to an infinite version of (A), namely,

$$\text{(D)}\quad \frac{1}{3!} = 1\cdot(1/\pi^2 + 1/4\pi^2 + 1/9\pi^2 + \ldots + 1/n^2\pi^2 + \ldots).$$

Multiplying both sides of (D) by π^2, we have

$$\text{(E)}\quad \frac{\pi^2}{6} = 1 + 1/4 + 1/9 + \ldots + 1/n^2 + \ldots ,$$

that is, (α).

Euler had other means of verifying (α). He had ways of estimating the value of π, and therefore the value of $\pi^2/6$. He also had ways of estimating the value of $\sum 1/n^2$. As his estimations got more and more precise, the two values, he noticed, converged. There was also other convincing evidence that (α) held.[18] Steiner and Putnam claim that in light of the above argument and evidence, Euler knew (α) (that is, (E)). In fact, they go further and claim that anyone who understands this argument knows that (α) is true. However, at the time Euler made this argument, neither he nor any of his contemporaries were able to prove (α); and Steiner, we have seen, goes even further, suggesting that there was no proof of (α) in Euler's theory of infinite summations.

Thus, it is claimed it is possible to know a statement of mathematics without there being a proof of that statement, and so it is possible for a mathematical statement to be true even though it has no proof.

Steiner's argument seems to rely heavily on two important claims. First there is the claim that Euler's argument establishes that (α) is true, even though it is not a proof. Second, there is the claim that mathematics at Euler's time is not essentially different from mathematics as it is now insofar as there are still arguments made today that establish statements as truths without being proofs and therefore without being presentable as derivations. (cf. p.17) The second claim is important; it allows us to conclude that at every stage of mathematical development there will be known truths without proofs, and that therefore formalizations of portions of mathematics should not always satisfy (HC). It might be argued, for example, that at Euler's time mathematics was in a state of ill repair in part because arguments like Euler's were taken as establishing truths, while today mathematics has entered the "age of rigor" in part because arguments like Euler's are no longer taken as establishing truths. Euler's argument, it might be argued, is a mere historical anomaly. Thus, it might be hoped, Steiner's argument can be explained away. However, if the second point is correct, Steiner's argument is not so easily dismissed. Euler's argument is not an historical anomaly, but an example of a method of argumentation that was in Euler's time, as it is now, an accepted and justified part of mathematical practice. According to Putnam and Steiner arguments that are not proofs are important parts of mathematics.

But why is Euler's argument not a proof? After all, if it is so convincing that "no mathematician doubted that the sum of $1/n^2$ was $\pi^2/6$," and if it establishes (α), why is it not formalizable as a proof? An argument, it is thought, is a proof only if it has a special sort of form. To be a proof an argument must be of a form such that any argument of that form with true premises has a true conclusion. Euler's argument is not of such a form. The inference from (C) to (D) is an inference based on analogy, not an inference based on form. Arguments of the same form as Euler's can be constructed that have true premises but a false conclusion. Thus, Euler's argument is not a proof, but an argument by analogy.

An interesting question to ask, but one which I shall not pursue, is whether the above argument that Euler's argument is not a proof works. It is true that Euler settled on the inference from (C) to (D) by analogy, that is, he infered (D) from (C) because he saw a similarity between (C) and (α) and (A) and (D). But granting that he settled on the particular inference he used by analogy, why can we not still hold that that inference is an inference justified by the forms of the statements in question, and thus that Euler's argument is, in fact, a proof. I think this question can be rephrased as: why pick one form over another when formalizing an argument? Why not formalize Euler's argument so that any argument of that form with true premises must have a true conclusion? Despite these (interesting) questions, I shall accept Steiner's and Putnam's claim that Euler's argument is not a proof. The questions raised in this paragraph call into question the entire project of formalizing mathematics and cannot be addressed in this essay. (See above, pages 25ff, for a discussion of related issues.) Furthermore, it does seem correct to suppose that Euler's argument is not a proof.

Even if we deny that Euler's argument is a proof, however, we are not committed to denying, as seen above, that there is a proof of (α) in Euler's theory of infinite summation; and it is this latter claim, as we have seen, that is needed to make what I called "Steiner's argument" against (HC). We need it to infer (4) from (3) (see page 144). Steiner gives some reasons for denying that in Euler's theory of infinite summation there is a proof of (α). He claims that Euler and <u>all</u> working mathematicians at that time did not know much about infinite summations. They had no consistent method of manipulating equations involving infinite sums. In fact, Steiner notes, actual mathematical practice at that time was so confused about equations involving infinite sums that inconsistent results were commonplaces. If the "rules" accepted by Euler and his contemporaries for manipulating equations involving infinite sums were formalized so that we had a proof system for their theory of infinite summation, Steiner suggests[18a], that system would be inconsistent. He claims that there is no consistent way to formalize the methods of proof Euler and his contemporaries used when dealing with infinite sums. Steiner therefore concludes that Euler's argument cannot be understood as a proof, and that there was no proof of (α) in Euler's theory about infinite sums.

Steiner's claims, I think, exploit a confusion we have about Euler's argument and Euler's knowledge at the time he made his argument. If we look closely at Euler's and his contemporaries concept of infinite summation, it is not hard to see why they were unable to manipulate consistently equations involving infinite sums and why they were therefore unable to demonstrate that (α) is true. Mathematicians, including Euler, at that time did not know what an infinite sum was. They thought that infinite sums are essentially the same sort of items as are finite sums -- only longer. It was not until later that a consistent notion of an infinite sum was available. We now know that (α) is true if and only if

$$\text{(F)} \quad \lim_{m\to\infty} \left(\sum_{n=1}^{n=m} 1/n^2\right) = \pi^2/6$$

is true. According to what we now know, infinite sums are limits of sequences of finite sums; they are not, as Euler thought along with his contemporaries, very long sums -- sums that are too long to be finite. Thus, Euler, at the time he made his argument, had a very different conception of infinite sums from the one we now have -- a conception of infinite sums that, as it turns out, is inconsistent. It is this fact, I think, that makes us ready to assent to Steiner's claim (3), the claim that Euler and his contemporaries were unable to prove (α). How could they prove (α) if they did not know that (α) is true if and only if (F) is true?

On the other hand, in the light of what we know about infinite sums, Euler's argument and the supporting inductive evidence provide good, convincing evidence for believing that (α) is true. Given what we know about infinite sums and how to manipulate equations that involve infinite sums, the reasons Euler gave overwhelmingly establish that (α) is true. But it is important to realize that part of what we know about infinite sums is that (α) is true if and only if (F) is true, and it is this fact in particular that leads to the belief that the convergence of Euler's increasingly precise estimations of the two sides of (α) is good reason for believing that (α) is true. The belief that Euler's argument is good reason for believing (α), that in light of the evidence given by Euler one can be said to know that (α) is true, is motivated by what we know about infinite sums, not by what Euler and his contemporaries knew about infinite sums; and, as it turns out, part of what we know about infinite sums is how to prove that (α) is true.

Try to imagine what we would think of Euler's argument if we did not have a proof that (α) is true. Would we believe that (α) has been established, that we know that (α) is true? Suppose we had a proof showing that (α) is independent of everything we can prove about infinite sums. Would we simply accept (α) without further ado? I do not think so. We would, I think, try to find a new axiom about infinite sums from which we could derive (α) and other similarly established equations. We would only be satisfied that we <u>know</u> that (α) is true, I believe, if we could find such an axiom. But once we have such an axiom, we have the means for proving that (α) is true. This suggests, I think, that we only take Euler's argument as good reason for believing (α) because we are able to prove (α). We only think that Euler's argument establishes (α) because we have a proof of (α).

Steiner says that Euler and his contemporaries had no proof of (α)

because they did not understand enough about infinite summations to prove (α). They thought that infinite sums were just very, very long finite sums, and they treated infinite sums as such, obtaining inconsistent results. We, on the other hand, now know that infinite sums are very different sorts of intems from finite sums. Infinite sums are limits of sequences of finite sums.[19] Since Euler and his contemporaries did not know what infinite sums are they had no proof of (α). But if they did not know what infinite sums are, how did they know that (α) is true? If they were so confused about infinite sums that they often obtained inconsistent results by manipulating equations involving infinite sums, why think that they knew enough about infinite sums to know that (α) is true? In fact, given the incredible difference between our understanding of infinite sums and the understanding had by Euler and his contemporaries, why even think that when they used the expression '$\sum_{n=1}^{\infty} 1/n^2$' they were referring to the same item we refer to when we use that expression? What evidence do we have beside typographic accidents that our use and their use of that expression are at all similar? Taking these thoughts one step further, we have the question: why think that, despite his argument, Euler knew that (α) is true? Our reasons for thinking that Euler could not prove (α) appear strong enough to warrant the claim that Euler did not understand (α). And it is a very small step from this claim to the claim that Euler did not that (α) is true. So it looks like we might want to deny that Steiner has provided an example of a known truth without a proof after all.

We can now see clearly the confusion I earlier claimed Steiner's argument exploits. On the one hand, Euler had what we consider overwhelming evidence that (α) is true. His argument and his approximations, we would say, are good reasons for believing that (α) is true. On the other hand, Euler could not prove (α) because, at the time he made his argument, Euler was confused about what infinite sums are. Indeed, he and his contemporaries were so confused about what infinite sums are that, we might easily say, he did not understand (α), and hence he did not know that (α) is true. It seems that Steiner's (and Putnam's) conclusion is based on confusing what we would say about the evidence provided by Euler's argument with what Euler and his contemporaries would (and should) say about that evidence.

It is important to distinguish between what we would take as overwhelming evidence and what Euler would take as overwhelming evidence. The distinction is important because what is taken as overwhelming evidence is a function of what is known, and what we know about infinite sums is very different from what Euler and his contemporaries knew about them. If this distinction and my argument is accepted, then we have good reason for denying that Steiner has given an example of a known truth of mathematics without a proof.

It begins to look as if Euler's argument cannot be taken as establishing (α). This becomes even more plausible if we think about arguments similar to Euler's but whose conclusion has been refuted. $\pi(x)$ is the number of primes less than x. For large values of x, $\pi(x)$ is approximately

$$\int_0^x \frac{dt}{\log(t)} \quad .$$

Hardly claimed that an extremely natural conjecture is

$$\text{(Con)} \quad \pi(x) < \int_0^x \frac{dt}{\log\,(t)}$$

and he pointed out that "Gauss and other mathematicians commented on the high probability of this conjecture."[20]

> The conjecture is not only plausible but it is supported by all the evidence of the facts. The primes are known up to 10,000,000 and their number at intervals up to 1,000,000,000, and [(Con)] is true for every value for which data exists.

Thus, there is extremely compelling evidence for (Con), evidence that seems to establish (Con) just as strongly as Euler's argument establishes (α). But (Con), unlike (α), is false. It can be shown that for some x less than

$$10^{10^{10^{34}}}$$

the inequality in (Con) is reversed. (Hardy claimed that this number is the largest to have "ever served any definite purpose in mathematics.") Such examples as this suggest, I think, that arguments like Euler's do not establish their conclusion.

What then of the claim that quasi-empirical methods are used commonly in mathematics? Are Steiner and Putnam wrong? No. It is true that quasi-empirical methods are used in mathematics; however, the proper interpretation of their role is, I think, different from that given them by Steiner. As already mentioned, it is possible to endorse the view that quasi-empirical methods play an important part in mathematical research without denying that their conclusions have no proofs. Indeed, I believe the correct view is along such lines.

What Euler's argument shows is not that (α) is true, but that there is reason to believe

(G) (α) has a proof.

Euler's argument, I think, confirms the claim that there is a way to use expressions like '$\sum_{n=0}^{\infty} [1/n^2]$' consistently so that they behave, in certain respects like expressions designating finite sums and so that (α) is true. At most, then, after seeing Euler's argument we believe (α) because that argument suggests that (G) is true. But this is by no means to claim that a mathematical statement can be known to be true even though it does not have a proof. For it is still imperative that we find a proof of (α) before we can be said to know it.

I think it is fair to say, then, that Steiner's argument cannot be used to refute (HC). So far, then, the Hilbertian argument for Hilbert's thesis seems to work; we have not yet undermined its premises. In the next section, however, I shall look at a standard criticism of Hilbert's philosophy of mathematics, and we shall see that criticism undermine the Hilbertian argument for Hilbert's thesis.

(v)

In the thirties and forties a series of results were obtained showing that if T is any reasonably strong recursively enumerable set of true sentences of arithmetic, and if the predicate '___is-provable-in-T' can be formalized as a Σ_1-formula, then a (maybe, the only) reasonable formalization of the claim that T is consistent cannot be proved in T. These results seem to demolish the motivation we had for thinking that every logic used to formalize mathematics should be complete and should have the Löwenheim property, two essential premises of the Hilbertian argument for Hilbert's thesis. Hilbert, recall, was led to claim that every logic used to formalize mathematics should have the Löwenheim property for (basically) two reasons. Since he thought that a mathematical theory is true if it is consistent (see section one), he believed that in order to show that a mathematical theory is true all we have to do is show that it is consistent. He also believed that finitistically acceptable arithmetic was certainly true and could be used to demonstrate the consistency of arithmetic. Thus, according to Hilbert, arithmetic contains a certainly true subtheory that can be used to demonstrate that arithmetic itself is consistent, and hence, according to Hilbert, true. Arithmetic, he thought, therefore, in a sense, secures itself. By then accepting that every logic used to formalize arithmetic has the Löwenheim property, Hilbert thought any part of mathematics could be shown consistent by constructing for it an arithmetic model. In sections three and four we saw how, in order for this view to begin to work, it must be supposed that logic used to formalize mathematics be complete. But then it follows from our definitions in chapter two and the supposition at the beginning of chapter three that every set of axioms be effectively generated that '___-is-provable-in-arithmetic' is formalizable as a Σ_1-formula. So by the results of the thirties and forties, arithmetic cannot prove its own consistency; and so surely finitistically acceptable arithmetic cannot prove the consistency of arithmetic. It looks as if the Hilbertian argument for Hilbert's thesis, therefore, fails for the same reasons that Hilbert's philosophy of mathematics does. However, in the final, concluding chapter of this essay, we shall see that there is a way of looking at the role of logic so that the Hilbertian argument, despite the refutation of Hilbert's philosophy of mathematics, does establish that, for some purposes, no logic stronger than first-order logic should be used when formalizing mathematics.

Footnotes for chapter four:

1. McGuinness (ed.), Gottlob Frege, The Philosophical and Mathematical Correspondence, op. cit., page 42.

2. P. Geach and M. Black (eds.), Translations from the Philosophical Writings of Gottlob Frege 3rd Edition, Rowman & Littlefield, Totowa, New Jersey (1980), page 117.

3. McGuinness (ed.), op. cit., page 33.

4. See George Kreisel, "Informal Rigor and Completeness Proofs" in Lakatos (ed.) Philosophy of Mathematics,

5. See Russell's communication to Frege in McGuinness, op. cit.

6. D. Hilbert, "On Infinity" in van Heijenoor, op. cit.

7. G. Boolos pointed out to me the following, plausible reason why Hilbert (and others) thought that the set theoretic paradoxes were the result of using transfinite notions and techniques illegitimately. Inspection of, say, Russell's paradox shows that no (genuinely) transfinite notions are involved. The axiom of infinity, for example, is not needed to derive Russell's contradiction. It, therefore, seems puzzling why Hilbert et. al. blamed the set theoretic paradoxes on the transfinite. Boolos suggests that they were still suffering from the paradoxes of analysis surrounding infinite series and summations and that they, therefore, concluded that the set theoretic paradoxes, similarly, resulted from an illegitimate use of transfinite methods.

7a. Note that this inference would be legitimate if there were a set containing every set. In that case we might let B be that universal set. Then $\forall y \varepsilon B$ can be rewritten as $\forall y$. So we know that for every ψ, we can find a set, A, such that

$$(\exists x \varepsilon A)\ (\forall y)\ [y \varepsilon x \leftrightarrow \psi(y)]$$

But then every instance of (1) is a truth.

7b. And the paradoxes of analysis; see note 7a.

8. See W.W. Tait, "Finitism", Journal of Philosophy (1981).

8a. See Hilbert's "On Infinity", op. cit., page 378.

9. Ibid.

9a. In a suitably generalized sense; i.e., unlike in Chapter 2, languages may contain operation symbols.

10. As in chapters one and two, I am supposing that = is a logical constant.

10a. In a sense suitably generalizing definition 10 of chapter two.

10b. For purposes of definitiveness we might take Rohit Parikh's system PB (see "Existence and Feasibility in Arithmetic" Journal of Symbolic Logic (1971)). PB is the subsystem of PA with the following axioms:

(1) $0 \neq s(x)$
(2) $s(x) = s(y) \rightarrow x=y$
(3) $x=0 \vee (\exists y)\ (x=s(y))$
(4) $x + 0 = x$
(5) $x + s(y) = s(x+y)$
(6) $x \cdot 0 = 0$
(7) $x \cdot s(y) = (x \cdot y) + x$
($\mathcal{E}n$) $A(0)\ \&\ (\forall x)\ (A(x) \rightarrow A(s(x))) \rightarrow (\forall x)\ A(x)$

where A(x) contains only bounded quantifiers.

Of course, PB is not quite the system discussed in the text, since (8n) and (3) are not ordinary sentences of arithmetic.

10c. But this leads to the following question: if the Ξ_0-valid sentences are the real sentences of arithmetic, if they capture the contentful part of arithmetic, what reason can there be for going beyond the Ξ_0-valid sentences? Why not simply reject those statements ordinarily accepted by mathematicians that are not expressible as Ξ_0-sentences? This is, in a sense, the line taken by the intuitionists, although they deny that the Ξ_0-valid sentences are all the true contentful sentences of arithmetic.

10d. Parikh proves that exponentiation cannot be represented in PB. He takes a non-standard model, N*, of Peano arithmetic, lets α be an infinite integer in the universe of N* and then considers the submodel, S, of N* whose universe is

$$A = \{x \text{ in the university of } N^* \mid \text{there is a standard } k \text{ such that } x < \alpha^k\}$$

S is a model of PB, but α^α is not in A, so exponentiation is not represented in PB. Similarly, if we notice that for each standard k, there is a standard r such that for all $\alpha > r, \alpha^k \leq \alpha!$, we can see that the factorial function cannot be represented in PB. (cf. note 10b)

10d'. G. Boolos pointed out to me the analogy between this sort of treatment of non-finitist methods in finitary arithmetic and the set theorist's treatment of classes.

11. D. Hilbert "On Infinity" _op. cit._, page 378.

12. D. Hilbert "On Infinity", _op. cit._, page 383.

12a. _Ibid._

12b. That Hilbert knew this and accepted it can be seen by examining "On Infinity" pages 384-5.

12c. W.W. Tait, _op. cit._

12d. George Kreisel, "Informal Concepts of Proof" in _Proceedings of the Internation Congress of Mathematicians_ (1958).

13. Of course this is not a definitive argument. My inpterpretation of Hilbert's philosophy of mathematics may be incorrect. Indeed, as noted above, it is a bit anachronistic. However, for our purposes in this essay, we can ignore the subtleties of Hilbert exegesis.

14. Michael Detlefsen, "The Significance of Gödel's Theorem", _Notre Dame Journal of Formal Logic_.

15. See Leslie H. Tharp, "Which Logic is the Right Logic" in _Synthese_, XXXI (1975), pages 1-21 for a very similar argument that Hilbert's thesis is true.

16. See Steiner _Mathematical Knowledge_, _op. cit._ and Putnam, "What is Mathematical Truth?" in _Mathematics, Matter and Method_.

17. Putnam, _op. cit._, page 68.

17a. _Ibid._, page 76.

17b. _Ibid._

17c. Steiner, _op. cit._, page 106.

17d. Steiner does not go on to conclude that the logic used to formalize Euler's theory of infinite summation should therefore not be complete. He does not consider this issue. Thus, I am taking certain liberties with the phrase 'Steiner's argument'.

18a. Steiner, _op. cit._, page 106.

19. Of course, using non-standard analysis, we might claim, infinite sums can (consistently) be looked at as very, very long finite sums; thus vindicating Euler's view. I shall, however, side-step this issue by ignoring it; not because it is uninteresting or false, but because I do not have the room to discuss it.

20. See G.H. Hardy, Ramanujan, Chelsea Publishing Company, New York (1940), pages 17ff. All references to Hardy are from this book. I should mention that G. Boolos called my attention to this example.

Chapter 5

CONCLUSION

The results of this essay seem negative. I have scrutinized several arguments purporting to show that formalizations of logic must meet specified conditions, and I have, for the most part, rejected those arguments as inconclusive. It is, however, traditional to try to end a work on a positive note, and that is what I shall try to do in this final (short) chapter. In the course of criticizing the arguments of Chisholm, Morley, Steiner, Hilbert and Dummett, several themes have emerged -- themes that can be used as the beginning of an account of the conditions formal logics used when formalizing mathematics must meet. In this last chapter, I shall present these positive themes, although, I should stress, the arguments I make and the conclusions I draw must not be treated as if they are conclusive.

When I discussed the Frege-Hilbert controversy (see the end of chapter three and the first two sections of chapter four), I noted that Frege's and Hilbert's disagreement resulted from their different opinions about whether all formal sentences have (or can have) sense independent of structures in which they are interpreted. Frege, I noted, was committed to the claim that all formal sentences used to formalize ordinary mathematical sentences and arguments have sense independent of the structures in which they are interpreted. In fact, it seems that, according to Frege, which structures are used to interprete a given formal sentence depends on the sense of that formal sentence. Recall that Frege criticized Hilbert's proof of the independence of the parallel axiom because it presupposed that the parallel axiom can be interpreted in a structure in which there are no Euclidean points, and such an interpretation, Frege thought, is contrary to the sense of the parallel axiom.) Hilbert, on the other hand, denied that all formal sentences used to formalize ordinary mathematical sentences have sense independent of the structures in which they are interpreted. I argued that (a reconstruction of) Hilbert denied that some quantified formal sentences have any sense independent of the structures in which they are interpreted.

Hilbert's and Frege's view on this matter can be contrasted by comparing the following two quotations. One is from Frege[1]:

> The sentence is of value to us because of the sense that we grasp in it.

The other is from Claude Chevalley[2]:

> ...[O]bjectivity is attained only in a pure symbolism, in emptying symbols completely of all meaning.

According to Frege, I think it is fair to say, mathematical knowledge is possible only if we are able to grasp the senses of relevant sentences. The more clearly we express these senses, the less chance there is of error and the more sure we can be of our results. For Frege, we can say metaphorically, the sources of objectivity in mathematics are the senses of the sentences of mathematics. For Hilbert, however, the sources of objectivity are not the senses expressed, but the symbols used to express those senses. Hilbert's approach to formalizations, as we have seen, therefore, stressed the syntactic aspects of a formalization, while Frege was more concerned with semantical issues. Hilbert tried to show how ideal sentences -- senseless sentences --

could be added on to what he thought was the contentful part of mathematics -- finitistically acceptable number theory -- without it being possible (syntactically) to derive a contradiction. Frege, on the other hand, tried to construct formal systems that exhibit clearly, precisely and unambiguously the senses of ordinary sentences of mathematics. Both hoped ultimately to show that mathematical truths can be known with certainty; but for Frege this involved expressing senses as precisely as possible, while for Hilbert it involved studying syntactic items with no regard to their senses.

These two different approaches towards formalizing mathematics, I have argued, lead to different conclusions regarding Hilbert's thesis. In section two of chapter three I interpreted Frege to be denying that first-order logic should be used to formalize geometry. Expressions like 'is a point', I interpreted Frege to claim, should have a counterpart among the constants of the logic used to formalize geometry. In a similar way, if we thought that the purpose of a formalization is to express specified senses clearly and unambiguously, we might deny that first-order logic should be used when formalizing other portions of mathematics. In chapter one we saw several examples of sentences and expressions -- used ordinarily by mathematicians -- whose senses cannot be captured by formulas of first-order logic. Thus, a Fregean view of formalizations, a view that formal sentences express senses, seems committed to the view that Hilbert's thesis is false.

This point can be made more strongly as follows. According to Frege[3], the sense of a sentence is that part of its meaning that we grasp that allows us to calculate it[illegible]th-value. So, if the purpose of formalizing, say, arithmetic i[illegible]ess the senses of the sentences of arithmetic as clearly as possib[illegible]n formalizations of arithmetic should display as clearly as possible a method (understood in a very loose way) for calculating the truth-value of arithmetic sentences. An axiom system is one way of doing this. If we specify a set of axioms and a logic and say that an arithmetic sentence is true if and only if it follows, using that logic, from those axioms, we have given the outlines of a method for calculating the truth-values of the sentences of arithmetic. There may be reasons for thinking that some logics used in this way cannot be complete (although, as stressed at the beginning of chapter three, we have every reason for hoping that the logic used will be complete). For instance, it might be argued that when formalizing arithmetic, when giving the outlines of a method for calculating the truth-values of the sentences of arithmetic, the logic used must be able to formalize the notion of finiteness; such a logic, we know, is not complete. A Fregean view of formalizations, then, can lead to the denial of Hilbert's thesis. The senses of ordinary mathematical sentences cannot always be expressed using first-order logic; to express those senses, then, a logic stronger than first-order logic must be used.

Despite the fact that the senses of ordinary mathematical sentences cannot always be expressed using first-order formulas, first-order logic retains a unique status. Even though Frege counted 'is a point' among the primitive expressions of geometric discourse and treated it on a par with logical constants, like the conjunction and identity signs, he did not believe that 'is a point' is a logical constant. We saw that, on one interpretation, Frege believed that when geometric proofs are formalized, are presented as sequences of sentences of a formal logic, the logic used should contain a constant corresponding to 'is a point'; however, Frege did not believe that geometric proofs are logical proofs nor did he believe that theorems of geometry can be logically proved. On the contrary, according to Frege, a major difference between geometry and arithmetic is that the latter can be reduced to logic, while the former cannot be. Thus, on Frege's view, geometry should be formalized using a formal logic containing a constant corresponding to 'is a point'; but this formal logic is not a formalization of logic, for 'is a point' is not a logical constant.

In a similar vein we might say that some statements about uncountable sets cannot be expressed using only first-order logic (in light of the Skolem-Löwenheim theorem)[3a], and that when formalizing some sentences of mathematics Keisler's quantifier should therefore, be used, although Keisler's quantifier is not a logical constant. We might also say that some arguments of arithmetic can only be formalized using weak second-order logic, even though some constants of weak second-order logic are not logical constants. If we find this view at all plausible, we cannot help but ask the following question: which, if any, formal logic formalizes logic? That is, which, if any, formal logic contains only logical constants as its primitives?

To pose this question is not, I think, to make merely a terminological query. There are two distinct uses to which formal logics are put. On the one hand, we use them to express clearly the arguments and statements made in ordinary mathematics. This is the Fregean view of formalizations. However, there is another use to which formal logics are put. Sometimes we use formal logics to analyze the arguments and statements made in ordinary mathematics. If the question "what, if any, formal logic formalizes logic?" is used to ask what if any formal logic can be used to express all and only logical notions, I think it is a terminological query. It is a matter of terminology whether or not we call 'is a point' a logical expression when we are only concerned with constructing a formal logic capable of expressing geometric statements. However, if the question is used to ask which formal logic is best used to analyze the arguments and statements of ordinary mathematics, it is no longer about a terminological point. Rather, it is a methodological query. It is a question about how fine we want the details of an analysis of ordinary mathematical notions to be.

Let me call the first use of logic discussed above "logic's expressive use" and the second use "logic's analytic use." Frege, I think, was primarily concerned with the expressive use of formal logics. When we are concerned with the expressive use of a formal logic, it is an important criticism to point to a statement (ordinarily expressed in mathematics) that cannot be formalized using only formulas of that logic. However, when we are concerned with the analytic use of a logic, it is not always a criticism to point to a statement (ordinarily expressed in mathematics) that cannot be formalized using only formulas of that logic. It might turn out that the best way to study and analyze a notion is by means of a formal logic that does not have the machinery needed to formalize that notion. For example, first-order logic, as is well-known and as has been mentioned repeatedly in this essay, contains no sentence true in all and only the finite models of arithmetic. This, I have said, might be used as a reason for denying that the arguments arithmeticians ordinarily employ can always be formalized as sequences of sentences of first-order logic. And so, if we are concerned with the expressive use of first-order logic, we might deny that it should be used when formalizing arithmetic. However, the fact that first-order logic contains no sentence true in all and only the finite models of arithmetic is not a reason for denying that first-order logic is the correct formal logic to use when analyzing the notion _finite integer_. A study of first-order consequences of statements made using the notion _finite integer_ might be precisely what sheds the most light on that notion.

The first-order Peano axioms figure centrally in the analysis of arithmetic notions, I think, for precisely this reason. We learn more about our notion of a finite integer by studying what can and cannot be derived from the first-order Peano axioms than we do by studying what does and does not follow from the second-order Peano axioms. Furthermore, that the Gödel consistency sentence does not follow from (using first-order logic) the first-order Peano axioms tells us more about the strength of the notion _finite integer_ than that the Gödel consistency sentence does follow from (using weak second-order logic) those axioms along with a sentence stating that the set of predecessors of every integer is finite[4]; and this suggests that when analyzing the notion of a finite integer, first-order logic should be used. However, it does not suggest that when using the notion of a finite integer, that is, when doing arithmetic, first-order logic is the strongest logic that should be employed.

When analyzing notions, there may be reasons for excluding those notions from the logic used; when employing those notions, there is no reason for such measures. This distinction, I think, is often overlooked. Often it is thought that because the former is true, the latter is as well -- that because it is expedient to exclude a notion from logic for purposes of analysis, the notion should be excluded from logic when it is employed. Thus, for example, Hilbert (correctly) studied and analyzed transfinite methods using only finitary methods, but he (incorrectly) thought that the content of all mathematics -- including those parts using transfinite methods -- is found in that part of mathematics that employs only finitary methods. However, there is no reason to conclude that because (one of) the best way(s) to study transfinite methods is to use only finitary methods, transfinite methods are only an "ideal" part of out logical machinery.

If we distinguish the two uses to which formal logics can be put -- if we distinguish between the analytic and the expressive use of logics -- we can hold that although one formal logic may be adequate for one task, it is inadequate for the other. A plausible claim, I think, is that first-order logic is the most suited for analyzing and scrutinizing the statements of ordinary mathematics, although it is not always suited for expressing clearly and presenting precisely the statements and arguments of ordinary mathematics. Indeed, I think the Hilbertian argument for Hilbert's thesis shows that no logic stronger than first-order logic should be used to analyze the statements of mathematics.

Two premises figured prominently in the Hilbertian argument for Hilbert's thesis: the claim that every logic used to formalize a portion of mathematics should be complete ((HC)), and the claim that every formalization of a mathematical theory should have an arithmetic model ((Arithmetic's Priority)). As stressed in the beginning of chapter three, completeness of a logic is always desirable; every thing being equal, we would like to be able to generate effectively the valid sentences of a logic. This is especially true if we are primarily interested in the analytic use of a logic; the only reasons given above for denying (HC) were motivated, I think, by concentrating on the expressive use of logic. Frege, we saw, would deny that logics used to formalize mathematics should always be complete because some notions cannot be _expressed_ using a complete logic, not because some notions cannot be _analyzed_ using complete logics. Steiner's argument against (HC) also turned on an emphasis of the expressive use of logics. That argument, recall, was that (HC) is false because there are truths with no proofs, and if we use a complete logic to formalize a mathematical theory, every valid sentence of that theory will have a derivation. But if we emphasize the analytic use of a logic, there is no reason why we should think that _every_ truth must be expressible as a valid sentence of the formalization in question, and so, in this light, Steiner's argument seems superfluous. If we emphasize the analytic use of formal logics, there is no reason for denying (HC); and since it is always desirable to use complete logics -- for any purpose -- if possible, all logics used to analyze mathematical notions should be complete.

Emphasizing the analytic use of formal logics also makes (Arithmetic's Priority) seem plausible. When analyzing notions, it can be argued, the objects presupposed should be as clearly understood as possible. The natural numbers are without doubt the most clearly understood mathematical objects.[5] It seems reasonable to insist, then, that formalizations of mathematical theories, <u>for the purposes of analysis</u>, should have arithmetic models.[6] Thus, if we emphasize the analytic use of formal logics, the Hilbertian argument for Hilbert's thesis is cogent.

Perhaps a good way to conclude this essay is by claiming that whether we accept Hilbert's thesis or not depends on the use to which we intend to put a formal logic. If we are interested in an analytic use, no logic stronger than first-order logic should be used; if we are interested in expressive uses, logics stronger than first-order logic may (and, if Frege is right, should) be used. Such a conclusion is supported by the following considerations.

There is no more doubt that the Gödel consistency sentence is true than that the first-order Peano axioms are true. Yet the former, as is well-known, does not follow from the latter (using first-order logic). There is a tendency to conclude that what we have to do is add the Gödel sentence to the first-order Peano axioms to obtain a better formalization of arithmetic.[7] But this, I think, is the wrong way to look at the situation. Our reasons for believing that the Gödel sentence is true are not independent of our reasons for thinking that the first-order Peano axioms are all true. Rather, the grounds for one are the grounds for the other. We know that the Gödel sentence is true, because the first-order Peano axioms are truths and from truths only truths can be derived (using first-order logic).[8] Once we accept the first-order Peano axioms, we must accept the Gödel consistency sentence (although, as it turns out, not on pain of (first-order) inconsistency). Our grounds for believing the Gödel consistency sentence just are our grounds for believing the first-order Peano axioms, namely, our notion of a finite integer and our techniques for using that notion to obtain truths. The fact that those grounds yield, on the one hand, the first-order Peano axioms and, on the other hand, the Gödel consistency sentence and that using first-order logic we cannot derive the one from the other tells us something interesting and important about our notion of a finite integer; it shows us just how strong that notion is. But it does not call into question the coherence of our notion of a natural number, nor does it cast doubt over our techniques for using that notion to obtain truths.

The conclusion of this essay can be painted in broad strokes and points the way for further research. The Hilbertian argument for Hilbert's thesis, I think, shows that first-order logic is the maximal logic that should be used when <u>analyzing</u> mathematical notions, but it does not show that first-order logic is the maximal logic that should be used when expressing statements of mathematics. On the other hand, the Fregean considerations of chapter three suggest that first-order logic should not always be used when expressing mathematical statements, but they do not show that a logic stronger than first-order logic should be used for analyzing those statements. In this way, I think, a niche can be found for Hilbert's thesis in the philosophy of mathematics.

Footnotes for chapter five:

1. See above in chapter three.

2. Claude Chevalley, On Herbrand's Thought in W. Goldfarb (ed.) Herbrand's Logical Writings.

3. See Dummett's Frege: Philosophy of Language for an excellent discussion.

3a. Here I am using 'express' in a very strong sense so that for example, a sentence expresses that statement that every number is finite if and only if it is true in all arithmetic models whose universes contain no infinite integers.

4. G. Boolos put the matter this way.

5. This is not to say that there are not problems with understanding what the natural numbers are, only that they are the best understood of the objects of mathematics. See L. Wetzel's forthcoming dissertation.

6. In fact, this accords with much mathematical practice. The rationals are often analyzed as ordered pairs of naturals, and the reals are analyzed using rational approximations.

7. I know there is this tendency because I exhibit it.

8. Compare Frege's criticism of Hilbert discussed above in section two of chapter three. Also see J. Myhill, "Remarks on the Notion of Proof" Journal of Philosophy, July 7, 1960.

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