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NUMBERS AND EXPRESSIONS

by

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Abstract

NUMBERS AND EXPRESSIONS

by

Laura Jacobs Cunningham

Adviser: Professor Jerrold J. Katz

The objective of this dissertation is to determine whether a formalist interpretation of classical mathematics is tenable. We first argue that the best theories of linguistics and mathematics characterize both linguistic objects and mathematical objects as abstract. This eliminates one objection to a formalist construal of mathematics. These results are interesting in themselves, since they address and resolve a problem largely ignored by formalists: the ontological status of expressions.

A second objection to formalism stems from Godel's work. He demonstrated that truth could not be identified with derivability within a formal system. However, it has been suggested that formalism need not be abandoned but can still be defended on epistemological grounds. We argue, to the contrary, that there is no epistemological motivation for formalism.

Thus, while on the one hand we show that formalism is ontologically tenable, our demonstration that the objects of both mathematics and linguistics are abstract removes the epistemological motivation for formalism.

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CHAPTER ONE

INTRODUCTION

Three distinct ontologies can be provided for both linguistics and classical mathematics. Nominalism holds that the objects of mathematics and linguistics are concrete. They are, therefore, dependent on matter. Conceptualism depicts the objects of mathematics and linguistics as psychological objects. On this construal, numbers and expressions are mind-dependent. Platonism maintains that the objects of study in mathematics and linguistics are abstract objects. Hence, they are independent of both mind and matter.

Assessment of the ontological status of either mathematical or linguistic objects has proven to be a difficult problem (as is well known). Much of the difficulty and, of course, the philosophical interest of the problem, can be attributed to the interconnectedness of ontological questions with other philosophical concerns such as epistemology. For instance, nominalism has garnered much of its support in the philosophical community from the fact that it meshes with an empiricist epistemology. Platonism, on the other hand, is not currently a popular position because it leaves epistemological questions unanswered.

Steiner, for one, criticizes platonism for failing to adequately characterize the epistemic link between human cognitive faculties and the objects of knowledge. Empiricism characterizes this link as causal; empiricists hold that knowledge comes from the causal interaction of the objects of knowledge with the knowers. Obviously, knowledge of abstract objects cannot be accounted for in this manner. Platonists have traditionally cited the faculty of intuition as providing the link between human cognitive faculties and abstract objects. The problem has been to adequately characterize intuition. Steiner has even gone so far as to say that platonism would be more widely accepted if an adequate epistemology existed. He says:

> Platonism would be in much better shape, however, if the existence of mathematical intuition were itself a viable proposition. A PRIORI OBJECTIONS HAVE A HABIT OF PALING IN THE FACE OF FAITS ACCOMPLIS.[1]

Clearly, then, ontological questions do not occur in a void; they must be assessed within other contexts, such as epistemological ones.

It is still another question whether the ontologies of linguistics and mathematics are connected. In many, if not most, instances the ontological positions for both disciplines have been developed independently. The formalist construal of mathematics, which holds that the proper objects of mathematical investigation are expressions, would seem to be an instance where this would not be the case in so far as the ontological status of mathematical objects must be compatible with the ontological status of expressions.

We want to determine, in this dissertation, whether formalism is consistent with the ontologies of mathematics and linguistics. Although formalism has traditionally been construed au Hilbert, who simply assumed that expressions are concrete objects, it is possible to depict expressions as abstract objects, mental objects, or concrete objects. This is a viable suggestion because the heart of the formalist position simply holds that the quantifiers in mathematics range over the set of expressions. We can therefore strip formalism of all ontological presuppositions as to whether expressions are abstract, mental, or concrete objects at the start. Then, while one goal of this dissertation is to determine whether the discipline of mathematics requires concrete, psychological, or abstract objects, another goal is to determine whether the discipline of linguistics requires concrete, psychological, or abstract objects. We see an advantage in combining these objectives since any defense of a formalist construal of mathematics must be consistent with the outcome of these determinations.

Here is a description of what follows in the text. In his influential paper, "What numbers could not be", Benacerraf claims that he exposes "the difficulties involved in trying to determine which objects the numbers really are".[2] He concludes that numbers are not objects. But if numbers are not objects then a discussion as to whether numbers are concrete, psychological, or abstract objects is moot.

Chapter Two opens with a discussion of Benacerraf's paper. We demonstrate that neither of the arguments contained in his paper support his conclusion that numbers are not objects.

Chapter Three is concerned with determining whether mathematical objects are concrete objects. We want to see whether a nominalist framework can accomodate mathematics. We distinguish three attempts to construe mathematics nominalistically. In Section One we discuss Field's claim, in Science Without Numbers, that he has demonstrated that the mathematics necessary for the sciences need not include terms or quantifiers which refer to abstract entities. While he doesn't deny that some parts of mathematics contain terms and quantifiers which refer to abstract entities, he does deny that the mathematics necessary for the formulation of physical theories contains such reference.

Field's argument can be interpreted as either a criticism of Quine's acceptance of abstract entities or, more broadly, as an overall defense of nominalism against platonism. We claim that Field's argument against Quine fails because if he accepts the Quinean framework, then he must accept Quine's holistic view of the world. But if Field accepts Quine's view that there is no distinction between mathematics and physics, then he cannot maintain a distinction between mathematical theory and the set of nominalistically-statable premises and conclusions. Without this distinction, Field's argument can't get off the ground.

Nor does Field's argument succeed when viewed as a general defense of nominalism against platonism. Field's argument is directed against a platonism, au Quine, which holds that the postulation of abstract entities can only be justified if they are needed in order to "do science". We claim that his argument would not hold against a non-Quinean platonist.

In the second section of Chapter Three we argue that a nominalist interpretation of mathematics, along the lines of Goodman, fails.

Finally, in Section Three, we turn to Hilbert's formalist construal of mathematics. We characterize this as a nominalist position because Hilbert held that expressions are concrete objects. We will argue that Hilbert's construal of expressions as concrete objects is inconsistent with the requirements of classical mathematics. This does not, we note, require abandonment of a formalist construal of mathematics insofar as expressions can be construed as either psychological objects or abstract objects.

In Chapter Four we focus on the question of whether the conceptualist framework is adequate for mathematics. That is to say, we want to determine whether the objects of mathematics are psychological objects. It will be argued that Frege's criticisms of a conceptualist view of mathematics are still sound.

From Chapters Three and Four we conclude that the objects of mathematics are neither concrete nor psychological objects. It follows that if linguistic expressions are either concrete or psychological objects then all formulations of the the formalist construal of mathematics would have to be abandoned. In Chapter Five we argue that expressions are neither concrete nor psychological objects but are, rather, abstract objects.

Determination that expressions are abstract objects eliminates ontological objections to a formalist construal of mathematics which depicts the objects of mathematics as expressions. Thus, on the one hand, this dissertation removes one objection to a formalist construal of mathematics by providing an ontological characterization of mathematics and linguistics which shows that both disciplines require abstract objects. These results are interesting in themselves, since they address and resolve a problem largely ignored by formalists.

It would be natural for anyone familiar with Godel's results to be surprised that we would devote any philosophical effort towards this problem. After all, the original motivation behind formalism was the identification of truth with derivability within a formal system. However, although Godel demonstrated that there are limits on provability within a formal system, formalism might still be defended on epistemological grounds. That is to say, if expressions are concrete objects, Hilbert's formalism might be defended on the grounds that we have epistemic access to concrete objects but not abstract objects. It would seem, though, that our determination that expressions are abstract objects would eliminate the epistemological motivation for formalism.

However, Linda Wetzel, in On Numbers, characterizes a formalist position which distinguishes between the sorts of abstract objects which are directly epistemically accessible and those which are not directly epistemically accessible. According to this distinction, the former have concrete tokens while the latter do not. If it could be shown that, say, expressions have concrete tokens while numbers do not, this would restore the epistemological motivation behind the formalist construal of mathematics. This formalist position bases knowledge of abstract objects on interactions with their concrete tokens. We argue that this assumes that knowledge of abstract objects is based on interaction with concrete objects and that we have epistemic access to concrete objects but not abstract objects. We maintain that these are false assumptions, thereby blocking this move to restore the epistemological motivation for formalism.

Recognizing that much of the resistance to platonism has stemmed from its poor characterization of intuition, we provide an interpretation of Godel's concept of intuition which does not portray it as a case of perceptual acquaintance. In doing so we avoid the traditional objections to Godel's account of intuition. We also discuss

Katz's account of intuition which he maintains is contrary to Godel's views. We, on the other hand, argue that it is compatible with Godel's account of intuition. We also want to show that both Godel's and Katz's accounts of intuition point to the same fact: perceptual knowledge is not "better off" than intuition. Therefore, there are no grounds for holding that we have epistemic access to concrete objects but not abstract objects. We conclude that although a formalist construal of mathematics cannot be dismissed on ontological grounds, there is no epistemological basis for formalism.

NOTES

1 Steiner 1975: 120. Emphasis added.

2 Benacerraf 1965: 289.

CHAPTER TWO

BENACERRAF AND NUMBERS

Benacerraf concludes, in "What Numbers Could Not Be", that numbers are not objects. He first argues that since we can model Peano Arithmetic in any object-progression and since there are no grounds for identifying any particular number with any particular object, numbers cannot be objects. His second argument stems from his view that some identity statements are meaningless. We will argue that both of these arguments fail.

Benacerraf says:

> ... Any system of objects, whether sets or not, that forms a recursive progression must be adequate. But this is odd, for a recursive set can be arranged in a recursive progression. So what matters, really is not any condition of the objects (that is, on the set) but rather a condition on the relation under which they form a progression...that any recursive sequence whatever would do suggests that what is important is not the individuality of each element but the structure which they jointly exhibit. I therefore argue, extending the argument that led to the conclusion that numbers could not be sets, that numbers could not be objects at all; for there is no more reason to identify any individual number with any one particular object than with any other (not already known to be a number).[1]

Unfortunately, Benacerraf's remarks on this "extension" are limited. In fact, interpretation of this passage is problematic precisely because he does not provide the details of the extension he refers to. We can only turn to Benacerraf's initial argument in "What Numbers Could Not Be" as the basis for the extension that we're interested in. We recall that Benacerraf initially argued that numbers cannot be sets because, for instance, there is no reason to identify, say, the number two with the set containing only one member, 1, rather than with the set containing exactly two members, 0 and 1. Thus Benacerraf's opening strategy is as follows. He wants to see if numbers are, in fact, sets. He argues that we have no grounds for asserting an identity relation between a given number and some particular set rather than some other identity relation between that same number and a different set. He concludes that since Peano Arithmetic has infinitely many models in set theory and there exists no reason to pefer any one account, numbers cannot be sets.

Benacerraf then extends this argument. He maintains that since we can model Peano Arithmetic in any object-progression, it follows that numbers aren't objects. He claims that if numbers are objects, each and every number must be uniquely identified with some particular object in an object-progression. Benacerraf asserts that since there are no grounds for identifying "any individual number with any one particular object than with any other (not already known to be a number)", numbers are not objects.

This is essentially an extension of the strategy that Benacerraf adopted to show that numbers aren't sets. Given any progression of objects "not already known to be a number", Benacerraf can show that there are no grounds for asserting any one identity relation between a given number and a particular object in the progression rather than a different identity relation between that number and a different object. Consider a progression of stroke symbols. If asked whether numbers are stroke symbols, Benacerraf would reply negatively; we can provide no justification for asserting the identity relation between the number 3 and the stroke symbol //// rather than with the stroke symbol /////. Numbers cannot be stroke symbols because we cannot identify each number with a particular stroke symbol. And presumably, given any progression of objects not already known to be a number, Benacerraf can show that identity relations cannot be established between any given number and a particular object in the progression. This, in fact, is what Benacerraf claims when he says:

> I therefore argue, extending the argument that led to the conclusion that numbers could not be sets, that numbers could not be objects at all; for there is no more reason to identify any individual number with any one particular object than with any other (not already known to be a number).[2]

We have no quarrel with Benacerraf's claim that "there is no more reason to identify any individual number with any one particular object than with any other (not already known to be a number)." However, we agree with Wetzel that it doesn't follow that numbers are not objects. The objection focuses on the qualifier "not already known to be a number". Benacerraf claims that if the set of objects "not already known to be a number" is identical with the set A containing objects a, b, c, d, and e, then x is an object if and only if it is identical with some member of A. Benacerraf argues that since there are no grounds for asserting, say, "x = a" rather than "x = b" or "x = c" or "x = d" or "x = e" when x is a number, it follows that we cannot claim that x is identical with any particular object. Hence x is not an object.

The problem lies in Benacerraf's tacit identification of the set of all objects not already known to be numbers, A, with the set of all objects, O. To assume that O is coextensive with A begs the question. As Wetzel aptly points out, this presupposes that numbers are not objects (that no number belongs to O) for it cannot be shown that objects don't belong to the set of all objects nor that numbers are not numbers.[3] Therefore, Benacerraf has not shown that numbers aren't objects; at best he has shown that no identity relation can be asserted between any particular number and a particular object NOT ALREADY KNOWN TO BE A NUMBER. He cannot conclude that numbers are not objects. We note as a matter of general interest that this counter to Benacerraf is also developed by Michael Resnik.[4]

There is an even more fundamental objection which can be directed against the *form* of Benacerraf's argument. He maintains that since we can identify the progression of natural numbers with any recursive progression of objects, numbers cannot be objects. This argument takes the following form:

i) Since the progression of natural numbers can be identified with any recursive progression of objects, Peano Arithmetic has many extensionally distinct models.

ii) Either every one of these accounts is correct, exactly one is correct or none is correct. But if every one of these accounts is correct, then a given number would identical to distinct objects (for example: <0, <0>, <0, <0>>> = 3 = <<<0>>>). This is false. However, if only one account is correct, then we should be able to provide reasons for preferring one particular account rather than any other. Since we cannot provide any reasons for identifying any given number with any particular object than with any other, there are no grounds for identifying the natural numbers as any particular recursive progression of objects. Therefore, none of these accounts is correct.

Benacerraf concludes that since none of the accounts is correct, *numbers are not objects*. But if numbers are not objects, then the progression of natural numbers cannot be identified with *any* recursive progression of objects and (i) is false. And without (i), Benacerraf's argument cannot even get off the ground.

However, Benacerraf supplements this argument with other considerations. He claims that Frege argues as if all expressions of the form

n=s

where 'n' is replaced by any expression ordinarily believed to name a number and 's' is replaced by ANY expression, are either true or false. Benacerraf counters that some identity statements are neither true nor false, but are senseless and unsemantical.

Benacerraf identifies three kinds of identity statements, distinguished by the kinds of expressions that can appear on the right of the identity sign:

> (a) with some arithmetical expression on the right as well as on the left...
> (b) with an expression designating a number, but not in a standard arithmetical way, as "the number of apples in the pot"...
> (c) with a referring expression on the right which is of neither of the above sorts, such as "Julius Caesar"...[5]

Benacerraf maintains that all identities of type (c) are "senseless and unsemantical".[6] He claims that those identity statements where the expression on one side of the identity sign designates a number while the expression on the other side does not, are meaningless. His reason for this claim is that "questions of identity contain the presupposition that the "entities" inquired about both belong to some general category."[7] He says:

> If an expression of the form 'x = y' is to

> have a sense, it can be only in contexts where it is clear that both x and y are of some kind or category C, and that it is the conditions which individuate things as the same C which are operative and determine its truth value.[8]

However, even given these rather strong assumptions about identity statements, we can still show that Benacerraf's argument fails. Benacerraf claims that the fact that '43 = Julius Caesar' is meaningless is evidence for the claim that numbers are not objects. He says:

> Throughout the first two sections, I have treated expressions of the form
>
> n=s
>
> where n is a number expression and s a set expression as if I thought that they made perfectly good sense, and that it was our job to sort out the true from the false.[9]

He says that, in reality, such statements are neither true nor false but meaningless. His point is that the problem, involved in determining which objects the numbers are, is that, contrary to Frege, not all identity statements are true or false; some are meaningless.

But why should the fact that some identity statements are meaningless have any bearing on the claim that numbers are objects? This would only be relevant if it could be shown that every statement of the form:

$$n = s$$

is meaningless where n designates a number and s designates an object. But this, in turn, presupposes that the set of objects is disjoint from the set of numbers - that they do not belong to the same general category. And, again, this is precisely what Benacerraf is trying to prove.

We note in conclusion that Benacerraf has not proved that numbers aren't objects. Both his initial argument and the subsidiary argument, mentioned above, fail to justify his conclusion.

NOTES

1 Benacerraf 1965: 290.

2 Benacerraf 1965: 290.

3 Wetzel 1984: 34-36.

4 Resnik 1980: 229-232.

5 Benacerraf 1965: 285-288.

6 Benacerraf 1965: 285-286.

7 Benacerraf 1965: 287.

8 Benacerraf 1965: 286-287.

9 Benacerraf 1965: 285.

CHAPTER THREE

ARE MATHEMATICAL OBJECTS CONCRETE OBJECTS?

A nominalist construal of mathematics depicts the objects of mathematics as concrete objects. We want to determine whether the nominalist framework can accomodate classical mathematics. We have identified three distinct arguments for a nominalist interpretation of mathematics. The nominalist might adopt a Quinean attitude towards the problem; Field is such a philosopher. While Field neither asserts nor denies that classical mathematics requires abstract objects, he holds that the part of mathematics adequate for science need not require abstract objects. Section One refutes Field's claims. On the other hand, a nominalist might assume that we can represent the objects of mathematics in the physical world au Goodman. In Section Two we argue that Goodman's nominalism cannot accomodate the fact that mathematics requires infinitely many numbers. Finally, the nominalist might argue that we should focus on the construction of the natural numbers as the place to explicate what it means to have infinitely many numbers. The idea is that the process of constructing the natural numbers can be carried out indefinitely. The nominalist then tries to provide an interpretation of constructions carried out indefinitely that is consistent with his view that the objects of mathematics are concrete, physical objects. Since Hilbert's formalism is representative of this strategy, we evaluate it in Section Three.

Section One

Field is a nominalist. He claims, in Science Without Numbers, that:

> ...the only non-question-begging arguments against the sort of nominalism sketched here (that is, the only non-question-begging arguments for the view that mathematics consists of truths) are all based on the applicability of mathematics to the physical world...there is one and only one serious argument for the existence of mathematical entities, and that is the Quinean argument that we need to postulate such entities in order to carry out ordinary inferences about the physical world and in order to do science.[1]

This passage is a good example of Field's commitment to the Quinean argument that the only viable justification for the existence of abstract objects is that they are needed to make inferences about the world and to do science. His strategy is then clear: if he can show that one need not refer to or quantify over abstract objects in physical theories, then he has shown that there is no justification for the existence of abstract objects. In other words, Field wants to show, on grounds that Quine himself would accept, that the assumption of abstract objects is not necessary. This can be seen as an internal criticism in so far as he accepts the Quinean framework but argues that Quine need not presuppose abstract entities.

We first want to show that Field's rejection of abstract objects, interpreted as an internal criticism of Quine, fails. Field's rejection of abstract objects can also be seen as an external criticism of platonism and, thus, as a overall defense of nominalism. We will argue that this, too, is ineffective.

Field argues that, for a Quinean, mathematical entities are theoretically dispensable because:

> ...if you take any body of nominalistically stated assertions N, and supplement it with a mathematical theory S, you don't get any nominalistically-statable conclusions that you wouldn't get from N alone.[2]

In other words, Field argues that since mathematics does not yield any new empirical knowledge, mathematical entities are theoretically dispensable. However, a fundamental assumption of Quinean theory is that mathematics and physics are continuous - they constitute one theory. This thesis holds that the evidence for both physics and mathematics is fundamentally the same: there is no line of demarcation between mathematical and physical evidence. One corollary of this thesis is that since there is no clear distinction between mathematics and physics, there can be no distinction between mathematical statements and nominalistically-statable, or empirical, statements. Central to Quine's holism is the idea that mathematical statements are not disconnected from observations in any way in which physics is not.[3] Both mathematics and physics face Quine's "tribunal of experience" and one cannot, in principle, distinguish mathematical theory from physical theory or mathematical statements from empirical statements.

This poses the following dilemma for Field. He claims that mathematical entities are dispensable because mathematical theory is not needed to derive nominalistically-stated conclusions from nominalistically-stated premises. This, however, presupposes that a distinction exists between mathematical theory and the set of nominalistically-stated premises and conclusions. If Field endorses Quine's holism, which denies this distinction, then he cannot make a principled distinction between mathematical theory and the set of nominalistically-stated premises and conclusions. Hence he cannot demonstrate that mathematical entities are theoretically dispensable. If Field rejects Quine's claim that mathematics and natural science constitute one continuous theory, then his argument can no longer be viewed as an internal criticism of Quine.

Field might escape this objection by agreeing, with Quine, that while there is no sharp distinction between empirical statements and mathematical statements, a distinction of degree exists.[4] Quine holds that mathematical statements are more centrally located in our conceptual framework than empirical statements and that while mathematical and empirical statements do not differ in kind, they do differ in degree. But on this account, both mathematical and nominalistic statements have empirical import, in so far as either type of statement can be revised in the face of recalcitrant empirical evidence. Thus, statements which are centrally located in our conceptual framework have empirical import, but because of their centrality are less likely to be revised in the face of recalcitrant evidence. Nominalistically-statable assertions, which are located nearer to the sensory periphery, are more likely to be revised than mathematical statements in light of recalcitrant evidence.

But if this view is correct and mathematical statements do have, in some sense, empirical import, then Field cannot maintain that the supplementation of N, a set of nominalistically stated assertions, with a mathematical theory S, doesn't yield any nominalistically-statable conclusions that you wouldn't get from N alone. This can be illustrated by the following example. Suppose that you supplement N with S_1, which contains the logical law of the excluded middle. This would result in empirical conclusions about the world that you wouldn't get if N was supplemented with S_2, in which the logical law of the excluded middle doesn't hold. Since N augmented by S_1 yields nominalistically-statable conclusions that you wouldn't get from N augmented by S_2, supplementing N with some S yields empirical statements that you wouldn't get from N alone. Therefore, even given the notion that mathematical statements differ from empirical statements in degree, Field's argument fails.

However, Field's argument could be viewed as a broader defense of nominalism, without accepting Quine's holism. As we saw above, Field claims that the only viable justification for the existence of abstract objects is that they are needed to make inferences about the world and to do science. He intends to show that since we do not need to refer to or quantify over abstract objects in the sciences, there is no justification for the existence of abstract objects. This could be seen as an external criticism of platonism. It is this part of Field's argument which is important to the question of whether a nominalist interpretation of mathematics is tenable.

The bulk of Science Without Numbers attempts to demonstrate that the existence of abstract objects is not required either for "ordinary inferences about the physical world" or "in order to do science"; Field concludes that mathematical entities are theoretically dispensable. His argument maintains that while we use mathematics in developing physical theories, the mathematics required need not contain either references to or quantification over abstract entities.

But this cannot be seen, on the broader view, as a defense of nominalism against platonism. Field's argument is directed against a platonism, au Quine, which holds that the postulation of abstract entities can only be justified if they are needed in order to "do science". But Field clearly begs the question by equating "doing science" with "doing physics". This assimilation of physical science with science glosses over the very point of controversy between nominalism and platonism. Field has not demonstrated that we do not need to postulate abstract entities to "do mathematics" or to "do linguistics". Since it is question-begging to assume that physics is co-extensive with science, he cannot conclude that numbers do not exist. A platonist would identify physics as only a proper subset of science.

We conclude that Field's rejection of abstract objects fails, regardless of whether his argument is viewed as an internal criticism of Quine or as an external criticism of the platonistic framework.

Section Two

The premise basic to Goodman's nominalism[5] is that it admits only individuals, which can be anything except classes. This last restriction is due to Goodman's denial that two different entities can be made out of the same basic elements, or constituents. Here is an example: let x be an atom which is ultimate, i.e., it has no constituents. The platonic system can distinguish between set A, whose only element is x and set B, whose only element is the set containing x. Goodman, on the other hand, asserts that sets A and B are not distinct because they both have exactly the same constituents.

The consequences of Goodman's premise are obvious. While the platonist can construct classes of classes by the power-set operator, and can thereby obtain classes of every cardinality, the cardinality of Goodman's "sum of individuals" is limited to 2^n, where n is the number of distinct basic constituents, or individuals. Thus the sum of individuals cannot provide collections of every cardinality.

Any nominalistically based construction of the natural numbers, au Goodman, must, therefore, assume that the world is sufficient to represent the numbers in. (We are not claiming that Goodman himself was trying to give a model for the natural numbers.) But, even if one places no restriction on what individuals are, except that they are not classes, the premise that there are infinitely many individuals requires strong empirical assumptions. We can put this another way. No reasons have been provided to show that the assumption, that the world is sufficient for the representation of the numbers, does not imply something false about the physical world. We conclude that since the structure of the world cannot guarantee infinitely many individuals, a nominalist interpretation of mathematics, au Goodman, cannot be sustained.

Section Three

Formalism holds that the terms and quantifiers of mathematics refer to linguistic expressions. The objects of mathematical study are, on this construal, expressions. Hilbert, as well as other formalists, has characterized these expressions as concrete objects. He says, "...in mathematics...what we consider is the concrete signs themselves, whose shape, according to the conception we have adopted is immediately clear and recognizable...".[6] For Hilbert, the objects of mathematics are physical.

Formalism is often dismissed by contemporary philosophers solely on the grounds of Godel's results; it is thought that formalism founders just because mathematics is not completely axiomatizable. We acknowledge, along with these philosphers, that the identification of truth with provability within a formal system was undermined by the publication of Godel's incompleteness theorems and that, as a result, post-Godelian formalism must acknowledge the inherent limits placed on the notion of provability. Nonetheless, the formalist might, while accepting these limitations, still argue that there are epistemological grounds for construing the objects of mathematics as concrete objects.

The demand that the objects of mathematics be epistemologically accessible has been articulated by Benacerraf in "Mathematical Truth". It is in this article that Benacerraf points to the need for a concept of mathematical truth which is consistent with an adequate epistemological account of how we have mathematical knowledge. He identifies two requirements for mathematical truth and knowledge. The first condition is that "mathematical truth be in conformity with a general theory of truth".[7] The second is that any account of mathematical truth must be consistent with the possibility of having mathematical knowledge. That is to say, the ontological status of mathematical objects must not preclude mathematical knowledge. He claims that the conditions for knowledge require a link between "our cognitive faculties and the objects known".[8] Benacerraf worries that no such account is possible when the purported objects of knowledge are abstract.

Epistemological considerations might, therefore, persuade us to view the formalist program more favorably because expressions are, on Hilbert's account, concrete objects. However, not only does such a construal presuppose that we have epistemic access to concrete objects rather than abstract objects, but it also assumes that mathematical expressions are concrete objects. Therefore, an argument which shows that these expressions are not concrete objects but must be abstract objects would, in effect, eliminate any obvious epistemological justification for formalism. We provide such an argument here.

Classical mathematics, even for the formalist, requires infinitely many expressions. Since anyone who holds that there are infinitely many concrete objects makes strong empirical assumptions, we will defend the view that if there are infinitely many expressions, then they cannot be concrete objects. It might be thought that Hilbert's distinction between potential and actual infinities accomodates the idea that mathematical expressions are both concrete and infinite in number. We will, however, argue that "potential infinity" cannot be characterized in a way that is consistent with the fact that mathematical expressions are concrete.

Hilbert claimed that the subject matter of number theory consisted of expressions, e.g., series of strokes /, //, ///, ////, etc. The concrete nature of mathematical objects was everywhere emphasized by Hilbert. We find him justifying his commitment to the concreteness of mathematical objects in the following representative passage:

> As we saw, abstract operations with general extensions of concepts and contents turned out to be inadequate and insecure. Rather something must already be given to our faculty of representation as a precondition for the use of logical inferences and the application of logical operations: certain extra-logically discrete objects which are intuitively present as immediately experienced prior to all thought...the objects of number theory are for me - to the exact contrary of Frege and Dedekind - the signs themselves...[9]

The extra-logical and discrete objects that Hilbert refers to are actual physical inscriptions. Hilbert suggested that mathematical statements were simply statements about physical inscriptions. As an example, consider a language consisting of only one primitive symbol, "/", and whose formulas are strings just containing this symbol, i.e., /, //, ///, ////,... We know that this series is isomorphic to the natural numbers; we map "/" to 0 and then call the operator which permits the iteration, or juxtaposition, of a "/" to the right of any formulas as the successor operator. We say that one numeral is larger than another just if the former extends beyond the latter. This is an example of a mathematical statement that refers to concrete objects; the mathematical relation "larger than" refers to differences in the length of physical inscriptions.

While the notion that mathematical objects are concrete is well-developed by Hilbert, it is, nonetheless, problematic since classical mathematics requires infinities. Hilbert recognized as much when he said:

> Weierstrass's analysis did indeed eliminate the infinitely large and the infinitely small by reducing statements about them to statements about relations between finite magnitudes. Nevertheless the infinite still appears in the infinite numerical series which defines the real numbers and in the concept of the real number system which is thought of as a completed totality existing all at once.[1]

Acknowledging that classical mathematics requires infinities, Hilbert recommended that we take the nonfinitary statements to be ideal in the sense that irrational elements are ideal. Just as the introduction of irrational elements permits the closure of the real numbers under the square root operator, introduction of infinities permits closure under the laws of logic. Hibert said:

> ...just as ideal factors were introduced to preserve the simple laws of divisibility for algebraic whole numbers...; similarly, to preserve the simple formal rules of ordinary Aristotelian logic, we must supplement the finitary statements with ideal statements.[11]

This is the most commonly cited passage for the justification of Hilbert's use of ideal statements. However, since we are trying to justify the use of logical laws in nonfinitary situations we cannot assume that the "formally simple rules of ordinary Aristotelian logic" hold for nonfinitary situations. This is precisely what is at question here. Resnik suggests that since real (finitary) statements can be deduced from ideal statements, the introduction of ideal statements may be justified indirectly.[12] He cites textual evidence to support his view that Hilbert believed that the use of ideal statements to obtain further real ones provided indirect justification for the use of ideal propositions:

> The science of mathematics is by no means exhausted by numerical equations and it cannot be reduced to these alone. One can

> claim, however, that it is an apparatus that must always yield correct results when applied to integers.[13]

This suggests that the ideal statements are part of a mathematical apparatus the aim of which is to generate true finitary statements. And Hilbert seemed to reconcile the needs of mathematics with the formalist viewpoint by distinguishing between the potential infinite and the actual infinite with statements of potential infinity functioning as ideal statements. Hilbert distinguishes between potential and actual infinities as follows:

> ...in analysis we deal with the infinitely large and the infinitely small only as limiting concepts, as something becoming, happening, i.e., with the potential infinite. But this is not the true infinite. We meet the true infinite when we regard the totality of numbers 1, 2, 3, 4, ...itself as a completed unity, or when we regard the points of an interval as a totality of things which exists all at once. This kind of infinity is known as actual infinity.[14]

This distinction appealed to many philosophers because statements of potential infinity are weaker than statements of actual infinity; the latter requires the actual existence of infinitely many objects while the former only needs the possible existence of infinitely many objects. Some philosophers, reluctant to accept actual infinities, found potential infinities acceptable. An even further distinction was then made between "bad" and "good" infinities. Actual infinities were classified as "bad" because they involved the notion of a completed infinite set. Since potential infinity only involved the notion of a process which didn't end, it was considered a "good" infinity.

It is clear that Hilbert thought that he could distinguish between these two kinds of infinities and it is equally clear that he regarded this distinction as useful. We recall Cantor's theory of transfinite numbers and Hilbert's claim that "No one shall be able to drive us from the paradise that Cantor created for us."[15] The problems infesting this paradise were the paradoxes of set theory. Hilbert thought that the only acceptable way to avoid the paradoxes was to clarify the concept of infinity. His distinction was meant to do just this.

However, the usefulness of this distinction depends on whether the formalist can adequately characterize the notion of potential infinity. We will argue that it is the inability to do so which haunts Hilbert's program.

Hilbert thought that the construction of the natural numbers was an example of potential infinity. The notion of potential infinity, appealed to by Hilbert, is that of incompletability: no matter how long a string of strokes is, it can be extended by one more stroke symbol. The string is never complete: there is no upper bound to the string. This explanation is deficient, however, until an adequate account of "can be extended" is provided.

A natural move is to explicate "can be extended" in terms of possibility: it means just that it is possible to extend any string of strokes. But, then, what is the notion of possibility adopted here? If we are, as Hilbert assumed, dealing with concrete objects, then one might think that physical possibility, which holds that it is physically possible to extend any series of strokes, captures the notion. However, anyone who wants to hold that there are infinitely many concrete objects makes strong empirical assumptions. In addition, such assumptions are at odds with Hilbert's explicit assertion that we cannot find infinity (of any type) anywhere in reality. Hence the use of physical constructions to explain the concept of infinity is problematic: our corresponding notion of physical possibility simply cannot support this extension. The difficulty arises when we conceive of "physical possibility" in terms of, say, the ability to, using paper and pencil, carry out this construction indefinitely. We worry about such things as the upper limit on the speed of construction, the upper bound on man's life span, and the amount of physical space required for the construction.

Resnik has suggested that Hilbert may have thought of written numerals as Kantian constructions in the mind.[16] It would be difficult to reconcile this with Hilbert's claim that mathematical ojects are concrete: mental objects are not concrete objects. We will argue, however, that there is no need of such a reconciliation because the Kantian notion of possibility fails to explain potential infinity in a satisfactory manner.

Kantian theory holds that the objects of mathematics can "be given" to our faculty of representation - in the mind. They need not be constructed on paper, but can be represented in the mind. Since classical mathematics requires infinitely many objects, this presupposes that the mind is capable of infinitely many representations. Although Kant says suprisingly little to justify this presupposition, he does claim that we have a concept of the infinity of space and that we could not have this concept unless there was a "limitlessness in the progression of intuitions." Kant says:

> Space is represented as an infinite quantity. Now a general concept of space, which is found in a foot as well as an ell, could tell us nothing in respect to the quantity of space. IF THERE WERE NO LIMITLESSNESS IN THE PROGRESSION OF INTUITIONS, NO CONCEPT OF RELATIONS COULD YIELD A PRINCIPLE OF INFINITY.[17]

While we agree that an unending progression of intuitions could serve as a model of potential infinity, we wonder whether the Kantian theory of mind can accomodate a limitless progression of intuitions. We argue that, in fact, Kant's theory of mind doesn't support the premise that the mind is capable of infinitely many representations. In order to develop our argument fully, we will concentrate on those features of Kantian theory which are germane to our own argument.

Intuitions are, for Kant, representations in the mind. Kantian theory holds that the mind has certain capacities to be affected by objects. As a result of these capacities, our intuitions have certain qualities which Kant calls the forms of intuition. According to Kant, one of these forms of intuition is space and its properties. That is to say, we represent objects as existing in a space that is infinite - either infinitely divisible or infinitely expandable (and it is the nature of the mind which is responsible for this representation). Because it is the nature of the mind (or the forms of intuition) which determines the characteristics of space and some of the properties of objects existing in space, we have synthetic a priori knowledge of these characteristics. However, since Kant maintained that synthetic a priori knowledge is limited to objects of possible experience, this means that if we represent space as infinitely divisible, then objects in this space are objects of possible experience. Therefore, very, very small objects would have to be objects of possible experience. But we do not have unlimited perceptual abilities. The resolution of the human eye is restricted by finite limits such that objects smaller than a certain dimension cannot be perceived. Similarly, if we represent space as infinitely expandable, then objects very, very far from us would have to be objects of possible experience. Again, the resolution of the eye limits the range at which objects can be perceived.

And the finite limits imposed by our life span restricts our ability to get within visual range of the objects of perception.

The difficulty lies in trying to reconcile these two Kantian claims[18]:

(i) we have synthetic a priori knowledge that space is infinite; and

(ii) synthetic knowledge is limited to the objects of possible experience.

Reconciliation of (i) and (ii) requires a very liberal interpretation of "possible experience" such that the possibility of experience extends beyond what can actually be perceived or experienced by man. The limits imposed by either man's physical make-up (which puts a lower limit on the size of a perceptual object) or man's finite life span (which puts an upper limit on the distance that an object can be removed from man and still be perceived) must be ignored in the explication of "possible experience".

We want to argue that this generous construal of "possible experience" is not available to Kant. He held that synthetic knowledge only applies to objects of possible experience. But explication of "possible experience", independent of man's actual experiences, conflicts with Kant's explicit assertion that it is improper to attribute properties to intuitions that are not revealed to us by the way that we perceive, or experience objects (as is seen in the following passage). Kant says:

> ...all our intuitions is nothing but the representation of phenomena; that things which we see are not by themselves what we see, not their relations by themselves such as they appear to us, so that, if we drop our subject or the subjective form of our senses, all qualities, all relations of objects in space and time, nay space and time themselves, would vanish. They cannot, as phenomena, exist by themselves, but in us only. It remains completely unknown to us what objects may be by themselves and apart from the receptivity of our senses. WE KNOW NOTHING BUT OUR MANNER OF PERCEIVING THEM, THAT MANNER BEING PECULIAR TO US, AND NOT NECESSARILY SHARED IN BY EVERY BEING, THOUGH, NO DOUBT, BY EVERY HUMAN BEING.[19]

This is our point. Suppose that in determining the capacities of the mind we disregard any physical limitations. Such a move depends on a conception of the capacities of the mind in abstraction from man as a physical being. In effect, we are taking some ideal being, unhampered by physical constraints, as the basis for determining our forms of intuition. Here is the problem: we assume that the ideal being is representative of us and then we see what THIS mind brings to the epistemic act. It is, however, necessary to first determine independently whether this being is representative of us. Such an independent argument is nowhere provided by Kant and, indeed, it is difficult to see how he could provide such an argument. When we go beyond the limits of our own experience to determine the forms of intuition we find ourselves at a loss to justify why we should hold that these are the forms in which objects must be given to US.

In fact, Kant himself allowed for the possibility that different beings may have different forms of intuition. He says:

> It is therefore from the human standpoint only that we can speak of space, extended objects, etc. If we drop the subjective condition under which alone we can gain external intuition, that is, so far as we ourselves may be affected by objects, the representation of space means nothing. For this predicate is applied to objects only in so far as they appear to us, and are objects of our senses....As the peculiar conditions of our sensibility cannot be looked upon as conditions of the possibility of the objects themselves, but only by their appearance as phenomena to us, we may say indeed that space comprehends all things which may appear to us externally, but not all things by themselves, whether perceived by us or not, or by any subject whatsoever. WE CANNOT JUDGE WHETHER THE INTUITIONS OF OTHER THINKING BEINGS ARE SUBJECT TO THE SAME CONDITIONS WHICH DETERMINE OUR INTUITION, AND WHICH FOR US ARE GENERALLY BINDING.[20]

The possibility that different beings might have different forms of intuition suggests that our concerns about the legitimacy of construing "possible experience" in abstraction from man's actual experiences were not idle.

Here is a summary of our argument. Kant held that synthetic a priori knowledge is limited to the possibilities of experience. But if objects are perceived, by us, to exist in an infinite space, the theory has to hold that the possibility of experience extends beyond man's actual experiences and perceptions. Since Kant also held that we cannot attribute properties to intuitions that are not revealed to us in the way that objects are perceived by US - not beings with unlimited perceptual abilities - we must conclude that Kantian theory, as it stands, cannot account for the infinity of space as a form of intuition (for US).

We can now return to the original problem. Kant maintained that we have a concept of space being infinite and that we couldn't have this concept unless the "progression of intuitions was unlimited" (and this is the only argument we can find that the progression of intuitions is unlimited). Our conclusion that Kant was not justified in maintaining that the infinity of space is a form of intuition nullifies his argument for the progression of intuitions being unlimited.

Therefore, insofar as our intent was to clarify "potentially infinite" in terms of activities of the mind (that the mind is capable of indefinitely many representations) and insofar as this depended on the Kantian theory of forms of intuition, the conclusion that Kant's theory of the forms of intuition doesn't support the idea that the mind is capable of infinitely many representations prevents us from using Kantian theory to explicate the potential infinite in terms of the activites of the mind. We have no reason to believe, on the Kantian theory of mind, that the mind is capable of an unending progression of intuitions.

We originally attempted to justify the formalist identification of mathematical objects with expressions on epistemological grounds. This move depended on the characterization of mathematical expressions as concrete objects. The immediate objection to this was that classical mathematics requires an infinite number of expressions. In meeting this objection, Hilbert introduced the notion of potential infinity. However, the success of Hilbert's move depends on adequate characterization of potential infinity. We suggested, citing a passage from "On The Infinite", that Hilbert thought of mathematical objects as constructions in the mind au Kant. "Potential infinity" captures the notion that no matter how many steps have occurred in such a construction, it is possible to add still another step. The problem came with the realization that this notion makes assumptions about the unlimitedness of mental representations which cannot be substantiated by Kantian theory. We were forced to abandon the explication of Hilbert's notion of potential infinity in terms of Kantian theory.

Until some more satisfactory analysis of potential infinity is proposed, we conclude that Hilbert's distinction between potential and actual infinities is unable to reconcile the formalist construal of mathematical objects as physical inscriptions with the fact that mathematics requires infinitely many objects.

We need not abandon the formalist program inasmuch as expressions can be interpreted in either a conceptualist of platonistic framework. We consider these alternatives in a later chapter.

Section Four

In this chapter, we rejected three different nominalistic interpretations of mathematics. We argued that a Goodmanian interpretation of mathematics presupposes infinitely many individuals. This requires empirical assumptions which are gratuitous.

Field's nominalism has the advantage of avoiding these strong empirical assumptions. He argues, instead, that abstract entities only exist if they are needed to "do science". Interpreted broadly, this can be seen as an argument against platonism. We countered that since it is directed against a platonism au Quine, it fails to hold against platonism in general. Nor is Field's argument successful when viewed as an internal criticism of Quine's acceptance of abstract entities. We demonstrated that if Field accepts the Quinean framework, then he must also accept Quine's holistic view of the world. But then he is unable to distinguish between mathematical theory and the set of nominalistically-statable premises and conclusions. This blocks his argument at the start. We concluded that Field failed to establish his nominalistic position.

In the third section of the chapter, we turned to Hilbert's formalism. Since he held that the objects of mathematics were expressions, and expressions were concrete objects, Hilbert's formalism provided a nominalist interpretation of mathematics. We argued that Hilbert's theory was unable to reconcile the formalist's construal of mathematical objects as concrete physical inscriptions with the mathematical need for infinitely many expressions.

We conclude that we have no good reason to believe that a nominalist construal of mathematics is adequate. The need for a framework which accomodates infintely many mathematical objects has traditionally been the biggest barrier to a nominalist conception of mathematics. We saw this in our discussion of a Goodmanian interpretation of mathematics. Field seemed to side-step this problem: rather than reinterpret mathematics, he argued that abstract objects were not needed to make inferences about the world or for the sciences. Thus he never had to confront this traditional problem faced by nominalists. Nonetheless, his argument failed because of false or conflicting assumptions. Although Hilbert thought that mathematical objects were concrete physical objects (inscriptions), he recognized the need for infinitely many mathematical objects. Hilbert's introduction of the concept of potential infinity represented a way for the nominalist to reconcile the concreteness of mathematical objects with the fact that there are infinitely many of them. We contended, however, that this objective cannot be met until adequate characterization of "potential infinity" is provided.

NOTES

1 Field 1980: 4-5.

2 Field 1980: 9.

3 Quine 1961: 44-45.

4 See Goodman 1977, Goodman and Quine 1947, and Goodman 1956.

5 Quine 1961: 44.

6 Hilbert 1926: 192.

7 Benacerraf 1973: 408.

8 Benacerraf 1973: 415.

9 Resnik 1980: 81-82.

10 Hilbert 1926: 183.

11 Hilbert 1926: 195.

12 Resnik 1980: 85.

13 Hilbert, quoted in Resnik 1980: 85.

14 Hilbert 1926: 188.

15 Hilbert 1926: 191.

16 Resnik 1980: 99.

17 Kant 1787: A: 23-25; B: 38-41.

18 I owe much of the substance of this discussion, pointing to the tension within Kantian theory, to Charles Parsons: 1964.

19 Kant 1787: A: 42-46; B: 59-63. Emphasis added.

20 Kant 1787: A: 26-29; B: 42-44. Emphasis added.

CHAPTER FOUR

Are mathematical objects psychological objects?

In the previous chapter we argued that the Kantian theory of mind could not account for infinity. We now want to offer a more general argument which would hold for all psychologically-based theories. Psychologistic theories hold that mathematical objects are neither abstract nor concrete, but are, rather, mental entities. We will argue that a psychologistic interpretation of mathematics is not defendable.

Section One

Frege's criticisms of psychologism, conceived nearly a century ago, are still regarded as sound. According to Frege, psychologism fails to provide an adequate ontology for mathematics because it holds that the objects of mathematics are mental entities or ideas. Frege says:

> ...the first question to ask is 'Whose idea?' We often speak as if one and the same idea occurred to different men, but that is false, at least if the word 'idea' is used in the psychological sense: each man has his idea.[1]

Frege's point is this: the objects of mathematics cannot be the product of mental processes because these products are peculiar to the producer or bearer of the ideas. The premise is that different men cannot have identical ideas because 'ideas' refer to mental contents. Ideas are distinct from thoughts in that the latter have no mental content and can be shared. This argument occurs throughout Frege's works. For instance, in "Logic" he says:

> ...the product of one person's mind is not that of another's, however similar they may seem to be, just as the hunger of one person is not that of another or the eye of one person is not that of another, however close the resemblance may be. We do not directly observe the processes in the mind of another, only the effects they have in the physical world. Strictly speaking, therefore, we can only form a superficial judgement of the similarity between mental processes, since we are unable to unite the inner states experienced by different people in one consciousness and so compare them.[2]

Post-Wittgensteinians will recognize this as the privacy issue. Frege claims that if, say, numbers are mental entities, then they "belong" to the individual who produces them. If the number ten were an idea, and ideas are private, then we would have many tens. There would be my ten and your ten and his ten and her ten... All of these tens would be distinct from one another. Frege argues that if numbers were ideas, mathematicians could not communicate because there would be no common grounds for communication. He says, in The Foundations Of Arithmetic:

> If number were an idea, then arithmetic would be psychology. but arithmetic is no more psychology than, say, astronomy is. Astronomy is concerned, not with ideas of the planets, but with the planets themselves, and by the same token the objects of arithmetic are not ideas either. If the number two were an idea, then it would have straight away to be private to me only. Another man's idea is, ex vi termini, another idea. We should then have it might be many millions of twos on our hands. We should have to speak of my two and your two, of one two and all twos.[3]

Not only would there be, on the ideational theory, no common grounds for agreement, but this theory even precludes disagreement! For Frege says:

> ...if, say, one man put 2x2=4 forward as true whilst another denied it, there would be no contradiction, because what was asserted by one would be different from what was rejected by the other. It would be quite impossible for the assertions of different people to contradict one another, for a contradiction occurs only when it is the very same thought that one person is asserting to be true and another to be false... There would simply be no common ground to fight on; each thought would be enclosed in its own private world and a contradiction between thoughts of different people would be like a war between ourselves and the inhabitants of Mars. Nor must we say that one person might communicate his thoughts to another and a conflict would then flare up in the latter's private world. It would be quite impossible for a thought to be so communicated that it would pass out of the private world of one person into that of another. The thought that entered the latter's mind as a result of the communication would be different from the thought in the former's mind; and the

> slightest alteration can transform a truth into a falsehood.[4]

The problem is not solely that psychologism precludes communication between mathematicians. Not only would different mathematicians associate different ideas with, say, the number two, but any given mathematician might associate $idea_1$ with the number two at T_1 and yet a different idea, $idea_2$, with the number two at T_2 . When putting forth 2x2=4 as true, is the mathematician associating $idea_1$ or $idea_2$ with the number two? Would it make a difference? It might. This points to the fact that communication problems can arise not only between individuals but also within individuals. Granted, it sounds peculiar to speak of communication within an individual. But it pinpoints the very real problem that any psychologistic interpretation of mathematics faces: the fact that any given individual can associate different ideas with a given number at different times. This is problematic because any construction of a proof, in which the number two occurs on successive lines, depends on the fact that the number two is used in a consistent manner throughout the proof.

Finally, Frege argues that evolution can alter the capacities of the mind. We know that the brain is subject to evolutionary changes: we can trace changes in our own evolutionary history. It is natural to wonder whether such changes might affect the production of mental entities. Frege suggests that the process of thinking may be different from what it was 5000 years ago[5]. If mathematical truths are products of human thought, then mathematical truths must be relativized to a given era. If this were not problematic enough, consider the following: it is possible that an evolutionary change might occur in females but not in males. Then, in a given era, a mathematical statement might be counted as true by the females but false by the males. Mathematical truths would have to again be relativized; they would, in this case, be sex-linked.

We have distinguished three Fregean arguments against psychologism. They all exploit the fact that psychologism, in precluding a common subject matter for mathematicians, deprives mathematical truth of objectivity. Frege's objections focus on the subjectivity of psychologism: psychologism holds that mathematical entities are mental entities (which are subjective). Psychologism is rejected as the correct interpretation of mathematics because it denies objectivity to mathematical truths.

Section Two

Frege's principle objection to a psychologistic interpretation of mathematics is that it makes mathematical truths subjective. This is based on the fact that, on this interpretation, mathematical objects are mental objects and unique to the individual who constructs them. Intuitionism is the most fully developed conceptualist theory of mathematics. Given the intuitionistic notion of truth-values, we might wonder whether intuitionism is vulnerable to these same criticisms.

In intuitionistic mathematics a given mathematical statement, say, Z, is true only if we can construct a proof of Z and Z is false only if we can demonstrate that a proof of Z yields an absurdity. The truth-value of a mathematical statement is proof-dependent. It might be natural to conclude that intuitionistic truths are implicitly indexed since proofs are, for the intuitionist, mental constructions and mental constructions are unique to the constructor. This relativization of truth-values is the basis for the charges of subjectivity against intuitionism.

The fact that intuitionism denies the objectivity of mathematical truths is one of the most common, and obvious, charges against intuitionism. However, in The Elements of Intuitionism, Dummett has rejected this on the grounds that the indexing, or relativization, of truths is not implicit in intuitionism. He holds that intuitionists are concerned with possible rather than actual constructions; "possible to construct" is viewed, by Dummett, as not having the force of an existence claim and, therefore, not indexed to a particular mental activity. Dummett says:

> 'We can prove A' ('A is provable') is not interpreted to mean either, at one extreme, that, independently of our knowledge, there exists something which, if we became aware of it, we should recognize as a proof of A, nor, at the other, that as a matter of fact we either have proved A or shall at some time prove it. In the former case, we should be appealing to a platonistically conceived objective realm of proofs; in the latter, we should be entitled to deny that A was provable on non-mathematical grounds (e.g. if the obliteration of the human race were imminent).[6]

Dummett claims that this interpretation is consistent with actual intuitionistic practice. 'We can prove A' is taken to mean: <u>in principle</u> we can prove A. In other words, the assertion of a mathematical statement does not entail that a proof of that statement exists but that there is an effective means for obtaining that proof. (Compare this to finitistic mathematics which interprets 'we can prove A' as '<u>in practice</u> we can construct a proof of A'.) Dummett supports the idea that intuitionists are concerned with possible, rather than actual, constructions with the following argument:

> We have explained disjunction by saying that a proof of AvB is a proof either of A or B, and existential quantification by saying that a proof of Ex A(x) is a proof

> of a statement A(n) for some n. In practice, however, intuitionistic mathematicians do not confine their assertions of disjunctive and existential statements, even in the course of giving a demonstration of the truth of some other theorem, to those for which they actually have a proof, as thus specified; it is considered sufficient that we have a means, at least in principle, for obtaining a proof. The most striking case of this is an instance Av-A of the law of excluded middle, when A is an effectively decidable statement, e.g. a statement that some very large number is prime. It is perfectly in order intuitionistically to demonstrate a theorem by showing that it follows equally from the supposition that some large N is prime and from the supposition that N is composite: it is not required that we should actually decide the matter before regarding the theorem as established. In general, it is licit to assert AvB provided that we have an effective means of which we can recognize that it would yield a proof either of A or of B, and to assert Ex A(x) if we have an effective means of which we can recognize that it would yield a particular number n and a proof of A(n).[7]

Dummett points to the fact that the intuitionist need not adopt the radical view that mathematical truths are relativized to individual minds in order to reject the law of excluded middle. This is based on his observation that when intuitionists hold that mathematical proofs must be constructive they do not mean that these proofs have actually been constructed, but that they could, in principle, be constructed. Understood in this way, mathematical truths are not indexed to individual minds.

In treating possible, rather than actual, proofs as the subject matter of mathematics, Dummett is moving from a performance model of mathematical theory to a competence model. Although mathematical truths are tied to proofs on the intuitionistic interpretation, these proofs include, Dummett claims, not only those which mathematicians have constructed but also those which mathematicians are capable of constructing. However, this move makes mathematics a function of the principles of mental operations which underlie mathematical competence in the following sense. Since there is no reason to think that only humans are capable of mathematical reasoning, we must assume that mathematical knowledge can by exercised by beings with mental capacities that are different from those found in humans. Suppose there were two such mentally distinct mathematicians: a human and a Martian, both of whom are ideal in the sense that they each exhibit all of the mental capacities of their particular species. Given that the human and the Martian have different mental capacities, it is conceivable that the Martian mathematician could prove a certain result that the human mathematician could not prove. The outcome would be such that the same mathematical statement would be provable in Martian mathematics and not provable in human mathematics. As a result, the truth-value of any mathematical statement would have to be species-indexed.

And since evolution affects the neurological and physiological mechanisms of mental operations, the intuitionist need not expect mathematical truths to hold across the evolutionary lifetime of even a single species. In other words, a mathematical statement that is not provable early in the evolutionary history of a given species might be provable later in its development.

We can see that, on Dummett's account of intuitionism, mathematical truths need not hold for all species of beings nor even across the evolutionary span of a single species. As long as the intuitionist fails to distinguish between the truth of mathematical statements and the mental processes involved in proving them, the truth-value of any mathematical statement must be indexed to the set of mental operations which underlie mathematical reasoning. Thus, while on Dummett's account mathematical truths are not subjective in the sense that they are indexed to particular individuals, they are, nonetheless, subjective as a consequence of the fact that they are indexed to the principles of mental operations.

It follows that since intuitionistic truths are subjective, intuitionism cannot provide a satisfactory account of mathematics. When we restrict our interest to classical mathematics, we can see that intuitionist mathematics differs from classical mathematics not only in principle but also in practice. For instance, intuitionistic theory does not yield the classical continuum. Thus, intuitionism, the most well-developed conceptualist theory of mathematics, fails for two reasons. It fails in principle because its truths are subjective, rather than objective. And it fails in practice, to provide an interpretation of classical mathematics, because the set of intuitionistic true statements is disjoint from the set of statements held to be true by classical mathematics.

Section Three

Psychologistic interpretations of mathematics deny that mathematical truths are objective. We argued that, contrary to Dummett's beliefs, intuitionism can not escape this objection. We argued earlier that mathematical objects cannot be construed as concrete objects; we've argued in this chapter that mathematical objects, such as numbers, can only be construed as abstract objects. This permits us to conclude that if expressions are either concrete objects or psychological objects then all formulations of the formalist program would have to be abandoned. We will argue in the next chapter that linguistic expressions are, in fact, abstract objects.

NOTES

1 Frege 1897: 130.

2 Frege 1879-1891: 3-4.

3 Frege 1980: 37.

4 Frege 1897: 133-134.

5 See, for instance, Frege 1879-1891: 5.

6 Dummett 1977: 19.

7 Dummett 1977: 19.

CHAPTER FIVE

The Ontological Status Of Expressions

Section One

We have argued that mathematical objects, such as numbers, cannot be construed as either psychological or concrete objects. Our second objective is to determine the ontological status of linguistic objects. Three distinct ontological positions have been defended by linguists: nominalism, conceptualism, and platonism. A nominalist interpretation identifies expressions with physical objects while the conceptualist identifies expressions with a psychological reality. Only the platonist takes expressions to be abstract objects.

Chomsky discredited nominalism in the early 1960's. Under the nominalist conception, linguistics is no more than a description of the physical features of language. Bloomfield defended the nominalist framework for linguistics. He said:

> Non-linguists (unless they happen to be physicalists) constantly forget that a speaker is making noise, and credit him, instead, with the possession of impalpable 'ideas'. It remains for the linguist to show, in detail, that the speaker has no 'ideas', and that the noise is sufficient - for the speaker's words act with a trigger-effect upon the nervous systems of

his speech-fellows.[1]

Chomsky argued that taxonomic theories were, in principle, unable to account for many features of language. For instance, they could not account for the creativity of language. Chomsky says:

> ...one of the qualities that all languages have in common is their 'creative' aspect. Thus an essential property of language is that it provides the means for expressing indefinitely many thoughts and for reacting appropriately in an indefinite range of new situations.[2]

Chomsky points out, correctly, that the taxonomic theories could not account for the fact that language-users can both express and understand indefinitely many sentences. In order to account for the creativity of language, the taxonomist would have to be able to mechanically produce a grammar given an adequate finite corpus of the language. However, there simply was no means of characterizing the inductive step which permits the linguist to syntactically describe infinitely many sentences on the basis of a finite corpus. Chomsky said that:

> ...syntax is an infinite system, but the inductive step that would lead to a description of syntactic structure was plainly lacking <in taxonomic theories>... The failure of inductive, data-processing procedures at the syntactic level became more obvious the more I worked on the

problem.[3]

Chomsky also found, in the course of his own attempts to construct a grammar of Hebrew, that the structuralist conception of grammar was not adequate for a description of the grammatical properties and relations of this language: he charged that it was not abstract enough. He had to introduce grammatical concepts which were not reducible to acoustic signals. He says:

> ...the elements that I was led to postulate in studying the generative grammar of Hebrew were plainly not within the range of such procedures. However refined, these were essentially procedures of segmentation and classification. They were designed to isolate classes of phones, sequences of these classes, classes of these sequences, etc., until, ultimately, sentences are characterized in terms of their constituents. But the elements that were needed in the optimal generative grammar simply did not have this character. They were not classes, sequences of classes, or anything of the sort, but were simply abstract elements forming strings that could be mapped into phonetic representation by deeply ordered rules of considerable generality.[4]

These objections, to a nominalist interpretation of linguistics, are still regarded as definitive today. A taxonomic description of language, which could only describe distributional patterns of sound waves, was too restrictive.

Chomsky's conclusions were further supported by experiments carried out in the Haskin's laboratory.[5]

Taxonomic theory holds that grammars are theories of sounds, the primary task being the segmentation and classification of acoustic signals. They assume that there is a correspondance between the phonetic analysis of speech and the acoustic analysis of speech - that each phonetic unit can be associated with a distinguishable and observable acoustic unit. Since acoustic signals are no more than patterns of sound waves they can be characterized by spectographic analysis. However, spectographic analysis of the acoustic signals revealed that different signals were perceived as identical phones and a given acoustic signal was perceived as different phones. We can conclude from this that there is a gap between the analysis of acoustic signals and the phonetic characterization of the sounds. Analysis of acoustic signals does not permit phonetic characterization of utterances.

However, taxonomic theories assume that all grammatical information is ultimately reducible to acoustic signals. Since analysis of the acoustic signals was not sufficient to identity the phonetic characteristics of utterances, taxonomic theory is unable to provide a phonetic characterization of the speech sounds.

These considerations motivated Chomsky's rejection of the taxonomic interpretation of grammars, the most well-developed nominalist interpretation of language. Grammatical concepts, adequate for linguistic theory, could not be reducible to acoustic signals but must be abstract.

Furthermore, taxonomic theory could not phonetically characterize speech sounds.

Chomsky has since been a strong defender of a conceptualist interpretation of grammars. He conceived of grammars as psychological theories about the ideal speaker's knowledge of the language. This knowledge was expressed in terms of the ideal speaker's set of internalized rules which reflected his linguistic competence. Chomsky says:

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance... The problem for the linguist, as well as for the child learning the language, is to determine from the data of performance the underlying system of rules that has been mastered by the speaker-hearer and that he puts to use in actual performance. Hence, in the technical sense, linguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying actual behavior.[6]

Although conceptualism has dominated linguistic theory for the past two decades, platonism has been increasingly defended as the correct ontological interpretation of linguistics (it can easily be shown that Chomsky's refutation of the nominalist framework is ineffective against platonism). Jerry Katz, a former conceptualist himself, has led the attack on conceptualism. He charges that conceptualism, just like nominalism, imposes constraints on linguistic theories which are deleterious. He says:

> Although the psychological constraints that conceptualism imposes on theories in linguistics are tame by comparison with the physical constraints that nominalism imposes, the conceptualist's constraints are not negligible. In requiring conformity to a concrete reality, psychological reality conditions impose constraints of a kind different from the requirement that grammars correctly describe the sound pattern, well-formedness, ambiguity, and other structural features of sentences. Psychological reality conditions in linguistics do not concern the grammatical structure of sentences but concern particulars of subjective experience or human biology. Since conceptualism imposes constraints requiring grammars to reflect some concrete reality, it could, in principle, prevent grammars from achieving the degree of abstraction necessary for satisfying traditional descriptive and explanatory standards.[7]

Certainly Katz is correct in his claim that conceptualism imposes psychological constraints on linguistic theories. Chomsky himself says:

> I have argued that the grammar represented in the mind is a "real object", indeed that a person's language should be defined in terms of this grammar, and that the vague everyday notion of language, if one wants to try to reconstruct it for some purpose, should be explained in terms of the real systems represented in the minds of individuals and similarities among these.[8]

Chomsky also says:

> It may turn out that in the domains where we speak of "knowledge of X" (knowledge of language, of music, of mathematics, of the behavior of objects, of social structure, of human characteristics, etc.), with the consequences of such knowledge in the form of expectations or knowing that such-and-such, there is a mentally-represented system of this nature which can be taken to be an object of knowledge, just as there is good reason, I believe, to think of what we know as a grammar when we speak loosely of "knowing a language."[9]

Our concern, then, is not whether conceptualism imposes psychological constraints on linguistic theory (clearly it does) but whether these constraints preclude, as Katz claims, adequate linguistic theories. We will try to determine, in the next section, whether psychological constraints are, indeed, too restrictive to provide an adequate theory of language.

Section Two

While Chomsky defends a conceptualist interpretation of linguistic theory, his definition of grammars is ontologically neutral. He says:

> A grammar of L is a system of rules that specifies the set of sentences of L and assigns to each sentence a structural description. The structural description of a sentence S constitutes, in principle, a full account of the elements of S and their organization. By the "structure of L," then, we mean the set of structural descriptions of sentences in L.[10]

According to Chomsky, the grammar must not only specify the set of sentences of L, but it must account for all grammatical properties and relations. Finally, the grammar must accomodate the creative aspect of language and account for projection which is reflected in the language-users ability to express and understand indefinitely many propositions based on limited exposure to the language.[11] Since this account of grammars is ontologically neutral, it is compatible with a platonistic interpretation of linguistic theory. Needing ontologically neutral guidelines for evaluating ontological characterizations of language, we identify the following as standards for linguistic theory:

> (i) A grammar must specify the set of sentences which belong to the language; it must mark as grammatical all sentences which belong to the language.

(ii) A grammar must account for all of the grammatical properties and relations.

(iii) A grammar must be able to account for the creativity of language and projection.

Our argument against conceptualism is twofold. First, we hold that the conceptualist requirement that grammars be psychological objects, taken with the fact that grammars are supposed to generate all and only those sentences which are grammatical, violates (i): some sentences which are grammatical will not be marked grammatical. Second, we will argue that since conceptualism cannot explain all grammatical properties and relations, condition (ii) is compromised. In short, we claim that the conceptualist grammar fails to specify the set of sentences which are grammatical and to account for all grammatical properties and relations.

As a matter of historical interest, Chomsky's criticisms of nominalism parallel our own criticisms of conceptualism.[12] Taxonomic grammars could not, Chomsky charged, accomodate the fact that natural languages are creative since they couldn't account for the grammaticality of indeterminitely many sentences; we hold that conceptualist grammars cannot account for the grammaticality of nondenumerably many sentences. We also claim that, just as the nominalist could not account for all of the grammatical properties and relations, neither can the conceptualist.

We first argue that conceptualism violates (i). On Chomsky's conceptualist view, while there is no finite limit on sentence length, there is an upper limit, N_0. Thus, every sentence in the language has fewer than N_0 components; each sentence is finite in length. Under this account, sentences of transfinite length would not be considered grammatical. Chomsky's commitment to this is explicit:

> A language is a collection of sentences of finite length all constructed from a finite alphabet (or, where our concern is limited to syntax, a finite vocabulary) of symbols. Since any language L in which we are likely to be interested is an infinite set, we can investigate the structure of L only through the study of the finite devices (grammars) which are capable of enumerating its sentences.[13]

It follows from Chomsky's characterization of sentences as finite in length, but without a finite bound, that there are denumerably many sentences. Thus there is not only an upper size limit on sentence length but there is also an upper limit on the set of sentences which are grammatical.

We want to show that this characterization is incorrect and that any linguist, such as Chomsky, who rejects a finite bound on sentence length must also reject any size limit on sentence length and any size limit on the set of grammatical sentences.

We take sentence length to be a direct correlate of the number of components or words, N, in the sentence. Someone might argue that N is imposed by performability and understandability considerations. We can see that this would impose transfinite limitations on sentence length because of limitations imposed by man's mortality, his memory capacities, and his perceptual apparatus. However, any move to make sentence size bounds a function of performability and understandability would result in limitations which would preclude the characterization of MOST sentences that are of finite length (say all those strings longer than 10^{100}) as grammatical. But this contradicts the supposition that there is no finite limit on sentence length.

We conclude that anyone, such as Chomsky, who rejects finite bounds on sentence length cannot impose transfinite bounds on sentence length on the grounds that such sentences could not be produced or understood. Such performance considerations would also impose finite restrictions on sentences. In other words, the conceptualist has already agreed to ignore the actual bounds of human capacities when he rejects finite bounds on sentence length.

However, if we ignore performance limitations, what would be the justification for imposing any size bound? In The Vastness of Natural Languages, Langendoen and Postal argue that once we determine that sentence length cannot be limited by whether or not they can be produced or understood, there is no linguistic justification for any restrictions on sentence length. They say:

> ...no speaker of any NL has ever even been claimed to have any intuitions about sentence length per se. There is no case where an intuition of ill-formedness is attributable to mere length. All that is ever observed is that as sentences become longer, they become harder to understand, perform, etc. But these are performance properties independent of any NL and dependent on the properties of the creatures attempting to understand, perform, etc. An intuition of ill-formedness truly due to length would have to involve structures which are not in conflict with performance limitations but still intuited to be ill-formed.[14]

Langendoen's and Postal's point is this: at best, speakers find that sentence length affects their ability to understand or produce sentences. Speakers' intuitions do not mark sentence length as a syntactic property. These facts could not be ignored by Chomsky. After all, he adopts as a methodological principle the fact that a grammar must be tested for adequacy by seeing whether it conforms to the linguistic intuitions of the native speakers of the language.[15] In "Methodological Preliminaries", Chomsky says:

> A grammar can be regarded as a theory of a language; it is descriptively adequate to the extent that it correctly describes the intrinsic competence of the idealized native speaker. The structural descriptions assigned to sentences by the grammar, the distinctions that it makes between well-formed and deviant, and so on, must, for descriptive adequacy, correspond to the linguistic intuition of the native speaker (whether or not he may be immediately aware of this) in a substantial and significant class of crucial cases.[16]

Langendoen and Postal pointed to the fact that if sentence length were a syntactic property, then an intuition of ill-formedness due to sentence length would have to involve structures which were intuited to be ill-formed for reasons other than performability or understandability. The fact that grammars must correspond to speakers' intuitions taken with the lack of evidence that speakers identify sentence length as a syntactic property, provides even more reason to exclude sentence length from the set of syntactic properties.

However, we don't want to rest our rejection of sentence size limitations on "lack of evidence". In <u>The Philosophy of Language</u>, Katz observes that:

> Since the infinite set of strings that is considered too long is in no way structurally different from those that are granted the status of sentencehood, the length property that differentiates such strings from those that are accepted as sentences has nothing whatever to do with the structural property of syntactic well-formedness.[17]

Katz's point is that the imposition of sentence size limits entails the following objectionable outcome:

> Outcome$_K$: If sentence length is a syntactic property, then there will be at least two sentences differing in sentence length, S_1 and S_2, such that S_1 and S_2 conform structurally but S_1 is grammatical and S_2 is ungrammatical.

We want to argue that any linguistic theory which is committed towards resolving the projection problem and which rejects finite bounds on sentence length must reject Outcome_K, and, therefore, reject the imposition of any limits on the size of sentences.

The projection problem holds that while the PLD (the primary linguistic data, or the set of utterances, that the speaker is exposed to) consists of a relatively small number of sentences, the set of grammatical sentences that can be projected from the PLD is infinitely large. The goal of the linguist is to describe the structure of the language in order to account for projection; the linguist tries to describe the patterns to which the grammatical sentences conform. Syntactic descriptions make explicit the structural regularities, or patterns, which occur in language. Chomsky says:

> Syntax is the study of linguistic form. Its fundamental notion is "grammatical," and its primary concern is to determine the grammatical sentences of any given language and to bring to light their underlying formal structure. The goal of syntactic study is to show that the complexity of natural languages, which appears superficially to be so formidable, can be analyzed into simple components; that is, that this complexity is the result of repeated application of principles of sentence construction that are in themselves quite simple.[18]

Thus, projection is accounted for by describing the structural regularities, or patterns, which occur in the language. Chomsky has observed that the projection problem is central to linguistic analysis, holding that a fundamental goal of linguistic theory is to "explain the general process of projection by which speakers extend their limited linguistic experience to new and immediately acceptable forms".[19] Chomsky says:

> A speaker of a language has observed a certain limited set of utterances in his language. On the basis of this finite linguistic experience he can produce an indefinite number of new utterances which are immediately acceptable to other members of his speech community. He can also distinguish a certain set of "grammatical" utterances, among utterances that he has never heard and might never produce. He thus projects his past linguistic experience to include certain new strings while excluding others...[20]

Resolving the projection problem involves, Chomsky claims, abstracting, from the PLD, a structural pattern which, in turn, accounts for grammatical decisions about infinitely many new sentences. Any syntactic framework which cannot account for this projection is inadequate (taxonomic theories, for instance, were rejected because they could not, in principle, account for the grammaticality of infinitely many sentences).

As a first step towards reconstructing this process of projection, the notion of "syntactic category" is introduced. Suppose that we have a PLD consisting of:

a. Kate left.
b. Sal slept.
c. Sam ate the leftovers.

We can assign these words to syntactic categories:

aa. N > Kate
bb. N > Sal
cc. N > Sam
dd. N > leftovers
ee. V > ate
ff. V > left
gg. V > slept
hh. Det > the

We can then associate a sequence of categories with each sequence of words:

A. S > N V
B. S > N V Det N

Each sequence of categories is called the syntactic form of the corresponding sentence. The set of grammatical sentences consists of those sentences which conform to one of the sentence forms. In this example, 'Kate slept' and 'Sal ate the leftovers' would be grammatical sentences because they conform, respectively, to the sentence forms: (A.) N V and (B.) N V Det N.

To summarize, we first assign words to categories such as: noun, adjective, verb, adverb, etc. We then rewrite each sentence in the PLD as a sequence of categories, replacing each word with the category to which it belongs. This produces a set of sentence forms and gives us a means of describing the structure of the language. It also is a first step towards reconstructing the notion of projection: any sentence conforming to one of these forms is itself grammatical. Thus, the set of grammatical sentences includes not only the set of sentences contained in the

PLD, but also all of the sentences of the same form as the attested grammatical sentences.

We can see that the use of conformation of structural form as a mark of grammaticality is critical. Without it, the notion of grammaticality is restricted to the set of sentences which is contained in the PLD. Under such conditions, only finitely many sentences (just those contained in the PLD) would be marked grammatical. Such a framework must be rejected because it could not, in principle, resolve the projection problem; it could not account for the grammaticality of infinitely many sentences.

Chomsky himself explicity accepts this when he says:

> A primary motivation for this study is the remarkable ability of any speaker of a language to produce utterances which are both new to him and to other speakers, but which are immediately recognizable as sentences of the language. We would like to reconstruct this ability within linguistic theory by developing a method of analysis that will enable us to abstract from a corpus of sentences a certain structural pattern, and to construct, from the old materials, new sentences conforming to this pattern, just as the speaker does.[21]

Chomsky points to the fact that whether or not "new" sentences are grammatical depends on their conformation or non-conformation to structural patterns abstracted from the PLD. It follows that any linguist, such as Chomsky, who is commited to resolving the projection problem must accept the following principle:

P_1 Any sentence which conforms to the structural pattern of some well-formed sentence is itself well-formed and any sentence which conforms structurally to some ill-formed sentence is itself ill-formed.

Corollary C_1 follows from Principle P_1:

C_1 There cannot be two sentences which conform structurally but which differ in grammaticality.

In the grammar developed above, we assumed that projection is accounted for by structural patterns which describe the combination of words. Call this a Word Level Grammar, or a description of the language at Level W. It is fairly simple to concretize our notion of P_1 at this level of description by explicating "conformation of structural form" as follows:

$CONF_W$: S_2 conforms structurally to S_1 when S_2 consists of exactly the same sequence of word categories as S_1.

We can see that this proposed description of language, at Level W, fails to mark many grammatical sentences as grammatical (i.e., 'John slept').[22] At first glance, this might appear to be a problem that could be resolved by listing more words and more sentence formulae. However, any grammar constructed within a syntactic framework which describes sentences as sequences of <u>words</u> would place a finite bound on sentence length. Thus the problem lies, not in any particular grammar formulated in this framework, but in the framework itself which describes sentences as sequences of words. The technique of projection described at the level W places a finite limit on sentence length and is unable to account for the unboundedness of sentences; no grammar constructed within this framework could account for the grammaticality of even denumerably many sentences.

In order to account for the grammaticality of infinitely many sentences, grammars must describe sentences as not just sequences of words, but also as sequences of phrases (groups of words). Thus the grammar consisting of rules (aa.) - (hh.) and (A.) - (B.) can be expanded to include:

ii. N > I
jj. V > know
kk. VP > V that S
ll. NP > Det N

C. S > N VP
D. S > S and S

The introduction of phrase structure rules represents a new level of description called the Phrase Structure Level, or Level P. The technique of projection at Level P accomodates the fact that while each sentence in the PLD has relatively few components, the grammar can mark sentences with a greater number of components (than the largest sentence in the PLD) as grammatical.

This represents a new level of description which extends our notion of structural conformity. This is to be expected since Level W represents utterances as strings of words while Level P represents utterances as strings of phrases. At Level W, S_1 and S_2 conform structurally only when they consist of exactly the same sequences of word categories. But the intuition about language structure that motivated phrase structure grammars was that complex sentences can be represented as the result of repeated applications of principles of sentence construction. This, taken with the fact that these complex sentences are grammatical because, as Chomsky says, they conform to a certain structural pattern, permits an extension of our notion of structural conformity as follows:

> $CONF_P$: S_2 conforms structurally to S_1 if S_2 is obtained from S_1 by the iteration of grammatical rules (i.e., Rules (aa.) - (ll.) and (A.) - (D.)) or if S_2 consists of exactly the same sequence of phrase categories as S_1.

This notion captures the intuition behind the construction of grammars at the Phrase Structure Level: that the grammar is capable of marking sentences longer than the longest sentence in the PLD as grammatical and that this is based on conformation of structural patterns. It is important to see, for what follows, that our notions of $CONF_W$ and $CONF_P$ are consistent with Chomskian theory which holds that one goal of linguistic theory is to account for projection.

We saw that a description of language at Level W could only partially account for projection - it could not account for the grammaticality of infinitely many sentences. Description of the language at Level P provides the means of accounting for the grammaticality of infinitely many sentences. At both levels the grammaticality of "new" sentences depends on whether they conform to the structural patterns abstracted from the PLD, just as Chomsky required. But in order to account for the grammaticality of sentences with no finite bound, our notion of structural conformity must be able to accomodate the fact that sentences of different lengths can be described as "conforming structurally". Our notion of $CONF_P$ not only allows sentences of different lengths to conform structurally, but it also captures the notion that the grammaticality of "new" sentences depends on whether they conform to certain structural patterns.

We can see that the technique of projection constructed at Level P is capable of accounting for the grammaticality of infinitely many sentences. The interesting question which remains is whether this notion of projection imposes a transfinite limit on sentence length or whether sentences have no limit, finite or transfinite. We want to argue that the technique of projection described at Level P is capable of accounting for the grammaticality of not only denumerably many sentences but also nondenumerably many sentences and that anyone who rejects finite limits on sentence length must reject any limit.

In The Vastness of Natural Languages, Langendoen and Postal demonstrate that there are sentences of every length. They argue as follows[23]:

Let S_0 be the denumerably infinite set of grammatical sentences (these sentences are grammatical by $CONF_p$ and P_1):

> <I know that Kate ate the leftovers; I know that I know that Kate ate the leftovers; I know that I know that I know that Kate ate the leftovers...>

Now, consider the set, S_1, which consists of all of the sentences of S_0 plus all of the co-ordinate projections of every subset of S_0 with at least two elements.

To see what a co-ordinate projection is, let U be a set of grammatical sentences, with the cardinality of U > 1. Let T be some co-ordinate compound of members of U. If U = S_0, then one co-ordinate compound would be:

> I know that Kate ate the leftovers and I know that I know that Kate ate the leftovers.

Assume that:

> (a) each conjunct of T has an element of U as a subconjunct; and
>
> (b) each element of U is a subconjunct of some conjunt of T; and
>
> (c) no element of U appears more than once as a subconjunct of any conjunct of T; and
>
> (d) if two elements of U occur as subconjuncts of conjuncts C_i and C_j of T, then C_i and C_j occur in a fixed order. Where C_i and C_j are of distinct lengths,

assume the shorter precedes; where C_i and C_j are the same length, assume some arbitrary order.

In this case, we say both that T is a co-ordinate projection of U and that U is the projection set of T.

(e) Example: Let U = <I know that Kate ate the leftovers; I know that I know that Kate ate the leftovers>. Then one may choose T = I know that Kate ate the leftovers and I know that I know that Kate ate the leftovers.

Then if S_1 is the set made up of all of the sentences of S_0 together with all and only the co-ordinate projections of every subset of S_0 with at least two elements, S_1 is:

<I know that Kate ate the leftovers; I know that I know that Kate ate the leftovers;...; I know that Kate ate the leftovers and I know that I know that Kate ate the leftovers; I know that Kate ate the leftovers and I know that I know that I know that Kate ate the leftovers;...; I know that Kate ate the leftovers, and I know that I know that Kate ate the leftovers and I know that I know that I know that Kate ate the leftovers;...>

Since the cardinality of S_0 is N_0, the cardinality of S_1 is N_1 and exceeds that of S_0. And since we can form greater and greater sets of sentences like S_1, by the method of co-ordinate compounding, there are sentences of every length and sets of sentences of every size.

We therefore have nondenumerably many sentences which conform structurally to some well-formed sentence and by P_1 these are grammatical.

Suppose that someone, like Chomsky, wanted to impose a transfinite length limit on sentences, holding that not every member of S_1 is grammatical. However, since every member of S_1 is a sentence which is obtained by the iteration of grammatical rules and thus conforms structurally to a grammatical sentence, this would entail $Outcome_K$: there would be two sentences differing in length, S_1 and S_2, which conform structurally but S_1 is grammatical and S_2 is ungrammatical. This, in turn, entails the rejection of P_1.

However, rejection of P_1, leaves no grounds for marking sentences with, say 10^{10} conjuncts as grammatical. In other words, rejection of this principle would not only deny grammaticality to infinitely long sentences, but also to large finite sentences. In short, any linguistic theory which rejects P_1 and marks a sentence, S_1, as grammatical and another sentence, S_2 as ungrammatical, when their structural properties conform, is not only unable to account for the grammaticality of nondenumerably many sentences but is also unable to account for the grammaticality of denumerably many sentences.

Such a theory could not, in principle, account for the grammaticality of sentences longer than the longest sentence in the PLD. Since the PLD consists of a finite number of finite sentences, such a grammar could not resolve the projection problem and must be rejected. We conclude that any linguistic theory which resolves the projection problem must reject Outcome_K and accept P_1, thereby placing no restrictions on sentence length.

We can conclude that the conceptualist theory, which holds that sentences of transfinite length are not grammatical, violates (i): there are some sentences, the transfinite sentences, which are grammatical but which are not recognized as grammatical by conceptualist theory.

Section Three

We will discuss one other objection to conceptualism. According to condition (ii), p. 60, grammars are supposed to account for all of the grammatical properties and relations. Katz argues that conceptualism cannot account for all grammatical properties: analyticity is unexplainable. Analytic sentences are those sentences which express propositions which are secured against falsehood. A standard example of an analytic sentence is: Nightmares are dreams. Katz argues:

> First, analyticity is a semantic property, since it is determined by meaning, and hence it must be accounted for at the semantic level of grammars. Second, analyticity is a species of necessary truth... Theories of natural languages ought not preclude explanation of the grammatical properties of their sentences. At the very least, a theory of natural language ought not rule out the possibility of accounting for necessary truths...which owe their necessity to the language...[24]

Katz claims, in this passage, that since analyticity is determined by meaning, it is a semantic property. He argues that since analyticity is a semantic property, it must be accounted for by the grammar. His argument depends on the fact that analyticity is determined by meaning. This fact has been challenged by Quine who asserts that there is no such thing as meaning. However, Katz has demonstrated that Quine's criticisms of meaning fail. Because Katz's critique of Quine is central to our argument, we will summarize it here. Quine's argument, found in "Two Dogmas of Empiricism", depends on the assumption that it is impossible to explicate notions such as analyticity and synonomy. He argues for this by showing that there is no noncircular way to use substitution criteria to distinguish synonymous and non-synonymous words and analytic and non-analytic statements.

Katz counters that although Quine has demonstrated that there are no substitution criteria for analyticity and synonymy, it doesn't follow that there are no means of clarifying these notions. He points out that the taxonomic method of clarification (which relies on substitution criteria) was discredited by the Chomskian program which replaced substitution criteria with a conceptualist conception.[25] Katz says:

> Chomsky's new theory replaces distributional analysis with the construction of formal characterizations of highly abstract systems of rules that themselves define linguistic notions and their indirect confirmation... The use of substitution tests disappears entirely, replaced by methodological principles for evaluating systems of formal rules as the best means of predicting and explaining grammatical facts. Thus, the circularity Quine discovered in attempts to clarify semantic notions on the basis of a substitution test no longer counts against their acceptability: the circularity is a product of a data-cataloguing approach to scientific theorizing about language, not of an inherent confusion in semantic notions.[26]

Katz is correct in his assessment that Quine inappropriately relied on a linguistic framework that was, in the mid-60's rejected by Chomsky (and for good reason, as we saw earlier in the chapter). Katz's conclusion, however, depends on whether semantic notions can be given theoretical definitions in linguistic theory. This, in turn, presupposes that there is a level of semantic representation in grammar. Chomsky has often expressed doubts about whether grammars include semantic representation. For instance, he says:

> Katz has perhaps been the clearest advocate of the view that linguistic theory provides a system for representation of meaning, and that the rules of grammar literally map formal structure into representations of meaning. My own view is more skeptical. In ATS it is argued that the line of demarcation (if there is one) between syntax and semantics is most unclear, and that it is also questionable whether the theory of meaning can be divorced from the study of other cognitive structures... I suspect that Katz's approach is legitimate over an interesting range, but that much of what is often regarded as central to the study of meaning cannot be dissociated from systems of belief in any natural way.[27]

However, Chomsky provides little justification for his skepticism of semantics. He says that he adopts a skeptical attitude towards representing semantic facts in a grammar because IT IS DIFFICULT TO DISTINGUISH BETWEEN SYNTAX AND SEMANTICS. Why, then, doesn't he adopt a skeptical attitude towards representing syntactic facts in a grammar - after all, it is difficult to distinguish between syntax and semantics! Furthermore, theoretical distinctions HAVE been made between the domains of syntax and semantics. Katz, for instance, distinguishes them as follows:

> Semantics does not lack an initial theoretical specification. It is specified as the domain whose objects are senses of sentences, compositionally determined from the senses of their constituents, and whose distinctive properties and relations include synonymy, meaningfulness, semantic anomaly, ambiguity, redundancy, analyticity, and contradiction. Such an initial specification of semantics seems no different in kind or less adequate than an initial specification of...syntax as the domain whose objects are sentences and whose distinctive properties and relations include well-formedness, sentence type..., ellipsis, and agreement. Semantics seems to be on a par with phonology and sytax: in each case, there is considerable prethoretic plausibility in the choice of objects and the properties and relations seem to be the ones that are determined by the objects in question.[28]

Katz undermines this particular objection by showing that there is no special problem with specifying the domain of semantics: both the domains of sytax and semantics are specified, theoretically, at the outset.

Chomksy also objects that NOT ONLY IS IT DIFFICULT TO DISTINGUISH BETWEEN SYNTAX AND SEMANTICS BUT THAT IT IS ALSO DIFFICULT TO DISTINGUISH BETWEEN SEMANTIC FACTS AND NON-GRAMMATICAL FACTS, which he refers to as "systems of belief", or intensions. Katz argues that this is not a problem unique to semantic representation in so far as a parallel problem exists for both syntactical and phonological representations: one must distinguish between syntactic and phonological phenomena and non-grammatical phenomena. Katz says:

> The problem of interpenetration for the phonological component, the other interpretive component of the grammar, is the same as that for the semantic component. The pronunciation of sentences is permeated with articulatory noises of all kinds, such as burps, pause fillers, language mixtures, artificial pronunciations like foreign accents, voice and sound imitation, drunken slurrings, and so on. Phonological interpenetration is no different in principle from semantic interpenetration, but it is not taken by Chomsky to raise parallel serious questions about whether phonology can be regarded as a legitimate part of grammar.[29]

The claim here is that in order to be consistent, Chomsky should treat the interpenetration of, say, beliefs with semantic representation in the same way that he treats phonological interpenetration. Both semantic and phonological interpenetration are matters of performance while semantic and phonological representations in the grammar reflect competence. The performance/competence distinction accomodates semantic interpenetration in the same way that it accomodates phonological interpenetration.

Chomsky also appeals to Quine's and Putnam's arguments against the distinction between meaning and extra-gramamtical belief. For instance, Chomsky says:

> ...a full dictionary cannot be distinguished in a principled way from an encyclopedia, while a grammar can be distinguished as a separate idealized structure, with certain aspects of dictionary entries as parameters. We might accept the legitimacy of the parametrized idealization, while agreeing with Hilary Putnam, for example, that "natural kind" terms such as "lemon," "water," "run," and so on, cannot be provided with "dictionary entries" that ignore matters of fact and belief. Then as Quine has argued, analyticity will not always be distinguished from shared belief.[30]

In "It Ain't Necessarily So", Putnam argues that since the animals referred to as cats could turn out to be robot spy-devices that only look and act like animals, 'cats are animals' is not an analytic statement.[31] If it were analytically true, then the animals referred to as cats couldn't turn out to be robots. Putnam's argument is intended to demonstrate that meaning is inseperable from factual belief: whether we mark 'cats are animals' as an analytically true statement depends on how things are in the world. Putnam holds that the answer will change according to the way that the world is consituted - we get one answer if all things referred to as cats are really robots, possibly a different answer if some of them are animals and some are robots, and yet another if at one time they were animals but now they are all robots.

Katz maintains that Putnam's argument is irrelevant:

> The reason is that meaning relations are not relations to empirical fact but relations in language... Putnam's claim that it is possible for the objects to which people actually refer in uses of "cat" to be non-animals, being about empirical fact, is irrelevant to the thesis about language that the concept of cat contains the concept animal as a part. The latter thesis does not claim that people cannot use words to refer to objects that do not fall under the meanings of those words.[32]

Katz charges that Putnam errs in assuming that empirical evidence can refute the intensionalist position. Since this is precisely what the intensionalist challenges, the fact that the objects referred to by 'cats' are actually robot spy-devices, is not a refutation of intensionalism. Whether or not this type of evidence is relevant is precisely the issue between intensionalists and non-intensionalists. The intensionalist holds that meaning is determined independent of facts in the world while the non-intensionalist maintains that such determination is tied to facts and beliefs about the world.

The intensionalist can still account for the fact that people can use words to refer to objects that don't fall under the meaning of those words (cases of reference under a false description) by utilizing Chomsky's performance/competence distinction. Katz accounts for this by distinguishing two notions of reference. He says:

> There must be two notions, one that functions as the criterion for saying that

> the description is false, and one in accord with which robots...are the object(s) referred to on the uses of the language in question. The former notion, which I call the "type reference", is one in which only the meaning of an expression in the language provides the information for determining the referent; the latter notion, which I will call the "token reference", is one in which, in addition to meaning, various beliefs of the speaker, etc. provide the information for determining the referent. Cases of reference under a false description can thus be explained as cases in which the type referent, projected from the meaning of an expression is not the token referent on the particular use in question because contextual factors, such as shared belief, true or false,...and so on, enable actual reference to succeed in spite of the false projection from semantic competence...[33]

In the case of Putnam's example, 'cat' lacks a type referent because there are no cats which are animals in his imaginations. 'Cat' does have a token referent which is explained by the contributions made by the speaker's extra-grammatical knowledge. However, since analytic truths are independent of the speaker's extra-grammatical knowledge, Putnam's example, at best, only shows that 'cat' lacks a type referent. The fact that its token referent is different from its type referent is not relevant to the question whether 'cats are animals' is analytic. Therefore, Putnam cannot conclude from the fact that all references of 'cat' are robot spy-devices that 'cat' does not mean "feline animal" and that 'cats are animals' is not analytic.

Quine also argues that the distinction between meaning and extra-grammatical belief cannot be sustained. He claims that the question of the meaning of an expression is indeterminate because there is no "fact of the matter" to appeal to.[34] According to Quine, semantics is distinct from the physical sciences in that there is a "fact of the matter" for physics to be right about. Then, although the physical sciences are vulnerable to ordinary inductive risks (that is, the theory is underdetermined by the evidence), they are not indeterminate. He holds, however, that semantics is, in principle, indeterminate because there is no "fact of the matter" which the theory can capture. Quine maintains that there is no "fact of the matter" in semantics because there are, say, no meanings for the theory to be right about. Katz counters that if meaning, and other semantic notions, can be explicated in linguistic theory, then Quine's indeterminancy thesis is not sustainable. Katz says:

> Since now there is no structuralist requirement that we have to define grammatical concepts in terms of inductive generalizations from a corpus, the concepts of analyticity and synonymy themselves may be used in constructing a theory of translation and in confirming its predictions on the basis of the judgements of speakers, particularly bilinguals, about synonymy relations, analyticities, etc.[35]

We can summarize Katz's objections. Quine's indeterminancy thesis rests on his earlier arguments that semantic notions cannot be explicated within linguistic theory. However, the fact that taxonomic theory couldn't explain the semantic notions of analyticity and synonymy does not mean that semantic notions cannot be explained within linguistic theory - they just cannot be explained within a taxonomic interpretation of linguistic theory (which is incorrect anyway).

Katz has argued for a theoretical definition of semantic notions with the definitions of synonymy and analyticity as part of linguistic theory. This provides the "fact of the matter" that Quine held was missing. We can utilize evidence about the semantic structure of the language to determine if two words have the same meaning or if a sentence is analytic.

However, even given a level of semantic representation in grammars, is analyticity a semantic property and is conceptualism able to account for analyticity? Since analyticity depends on the meaning of the words and the syntactic arrangement of these words, analyticity is a semantic property. In Semantic Theory[36], Katz characterizes analyticity as the inclusion of representations at the semantic level. And while it is true that conceptualism can also accomodate a level of semantic representation in grammars (Katz's semantic theory is ontologically neutral in that respect), conceptualism is unable to account for analyticity. Katz points out that the only notion of necessity allowed by conceptualism is what humans conceive, on the basis of their mental of physical makeup, to be true no matter what.[37] The conceptualist pictures necessity as relative to the human cognitive process. Since platonism treats analytic truths as linguistic facts, it can explain how analytic truths ARE true no matter what; it alone can account for the grammatical property of analyticity.

Section Four

This chapter examined and compared three distinct ontologies for expressions: nominalism, conceptualism, and platonism. Nominalism was rejected because it could not account for the creativity of language. Nominalistic interpretations also precluded adequate theoretical characterization of language because the grammatical concepts were insufficiently abstract; nominalist theories could not account for all of the linguistic properties and relations.

Conceptualism was rejected for similar reasons. For one, it could not account for the creativity of the language because it excluded from the set of grammatical sentences all sentences of infinite length. Secondly, the conceptualist theory could not account for all of the grammatical properties and relations: it was unable to account for analyticity.

Since platonism conceives of grammars as theories about abstract objects, it could accomodate the creativity of language: there need not be, for the platonist, any limit on sentence length. The platonist can also provide a theory of language which is capable of accounting for, and explaining, analyticity. We conclude that while both nominalism and conceptualism provide insufficient frameworks for a theory of language, a platonist interpretation accomodates the degree of abstraction necessary for an adequate theory of language.

The reader may have noticed that we relied on the competence/performance distinction at crucial points in our argument. We relied on it to show that sentence length is unbounded (by showing that length restrictions reflect performance limitations) and that there is a level of semantic representation in grammars (by accounting for semantic interpenetration). Thus, our conclusion that linguistic expressions are abstract objects, rather than psychological objects, relied, in part, on a distinction championed by Chomsky. We defend our use of this distinction on the grounds that one need not accept the conceptualist claim that grammars are theories of the ideal speaker's internalization of rules in order to recognize the distinction between competence and performance.

We argued earlier that mathematical objects can be neither concrete nor psychological objects but must be abstract objects. In this chapter we argued that linguistic objects are neither concrete nor psychological objects but are, instead, abstract objects. This permits several conclusions regarding the formalist position. Formalists claim that expressions are sufficient for mathematics. On our account, linguistic theory can accomodate the fact that formalism requires infinitely many expressions. Thus the formalist program need not be rejected on ontological grounds. However, we initially motivated the formalist position epistemologically. We said: if expresions are concrete objects, then there are epistemological grounds for the formalist position. That is to say, if numbers are abstract objects while expressions are concrete objects, and concrete objects are epistemically accessible while abstract objects are not, then there may be epistemological grounds for a formalist interpretation of mathematics. Since expressions are not concrete objects, the epistemological motivation seems to vanish. We turn, in the next chapter, to the question of whether the epistemological motivation for the identification of mathematical objects as linguistic expressions can be restored.

NOTES

1 Bloomfield 1936: 23.

2 Chomsky 1965b: 83.

3 Chomsky 1975c: 30-31.

4 Chomsky 1975c: 29.

5 Fodor, Bever, and Garrett 1974: 291-299.

6 Chomsky 1965b: 80-81.

7 Katz 1985b: 195.

8 Chomsky 1980: 120.

9 Chomsky 1980: 180.

10 Chomsky 1975c: 5.

11 Chomsky 1965b: 83.

12 This is, essentially, the strategy adopted by Katz 1981.

13 Chomsky 1959: 137.

14 Langendoen and Postal 1984: 37-38.

15 Chomsky 1965b: 102.

16 Chomsky 1965b: 102.

17 Katz 1966: 122.

18 Chomsky 1975c: 57.

19 Chomsky 1975c: 519.

20 Chomsky 1975c: 61.

21 Chomsky 1975c: 131.

22 Selectional restrictions would give an account of grammaticalness that is more detailed and specific. However, for our purposes, this very general account suffices.

23 Langendoen and Postal 1984: 49-59. It should also be mentioned that mathematicians have provided formal systems for languages with formulas of infinite length; see, for instance, Carol Karp's <u>Languages With Expressions Of</u>

Infinite Length or The Syntax and Semantics of Infinitary Languages, ed. by Jon Barwise.

24 Katz 1985b: 199.

25 See, for instance, Katz 1981:35-39 and Katz 1985a: 7-8.

26 Katz 1981: 150.

27 Chomsky 1975c: 23.

28 Katz 1981: 119.

29 Katz 1981: 118.

30 Chomsky 1977: 36-37.

31 Putnam 1962: 52-62.

32 Katz 1981: 146.

33 Katz 1981: 147.

34 Quine 1960: 73-79.

35 Katz 1981: 152.

36 Katz 1972: 171-177.

37 Katz 1981: 6.

CHAPTER SIX

FORMALISM RECONSIDERED

Section One

We have provided general arguments which show that mathematical objects cannot be construed as either concrete or psychological objects; they must be abstract objects. We have also argued that expressions are neither concrete nor psychological objects but abstract objects. These findings are ontologically compatible with a formalist construal of mathematics in so far as both expressions and mathematical objects are abstract objects.

We had earlier motivated formalism on epistemological grounds: if expressions are concrete objects and numbers are abstract objects, then expressions, but not numbers, are epistemically accessible. Accepting, for the sake of argument, the claim that concrete objects are epistemically accessible while abstract objects are not, we still found this "motivation" to be unjustified because expressions are NOT concrete objects. We now want to see whether we can defend formalism on other epistemic grounds.

In her doctoral dissertation[1], Linda Wetzel considers a reformulation of the formalist position which both accomodates the fact that expressions are abstract objects and restores the epistemological motivation for a formalist construal of mathematics. The idea behind this move is that even if formalists concede that expressions are abstract objects, they might still maintain that there is an epistemological advantage in having the mathematical quantifiers range over expressions. We call this reformulation: $formalism_2$. Wetzel says that the central idea behind $formalism_2$ is that:

> Even though expressions (types, that is) are abstract objects, there is still a mighty disanalogy between expressions and numbers. For expressions (again, types) have tokens - splotches of ink, air disturbances, and so forth - that are readily perceptible. We can come to know about expression types by means of interaction with their tokens, and thereby circumvent the epistemological problems chronicled by Benacerraf. No such explanation is available in the case of numbers, however, for numbers have no tokens. So even were they to exist, we would be unable to know anything about them.[2]

We can see that $formalism_2$ restores the epistemological motivation. It claims that while there is some acceptable explanation of how we can come to know expressions, no such explanation is available for how we can come to know numbers. In effect, while conceding that expressions are abstract objects, $formalism_2$ makes a distinction between abstract objects which are epistemically accessible, i.e., expressions, and abstract objects which are not epistemically accessible, i.e., numbers.

Wetzel rejects $formalism_2$. She argues that if we can come to know expression types through interaction with concrete objects, then interaction with concrete objects yields knowledge of number types. She says:

> ...I want to claim that to the extent that the formalist is justified in claiming that "we can come to know about expression types" -- whatever this amounts to -- "by means of interaction with their tokens" we can also, and on a similar basis, "come to know about" numbers... That is, I will argue that there are readily perceptible phenomena that function as numerical analogs of actual utterances and inscriptions.[3]

Wetzel maintains that if we can provide an account of how our knowledge of expression types is based on interaction with concrete objects, we can also provide an account of how our knowledge of number types is based on interaction with concrete objects. It is important to distinguish this position from Penelope Maddy's claim that abstract objects can exist in space and time and, hence, are accessible to causal contact (see, for instance, "Perception And Mathematical Intuition", 1980). Wetzel doesn't abandon the standard notion that abstract objects exist apart from space and time; she argues, instead, that if interaction with concrete objects yields knowledge of expression types then interaction with concrete objects yields knowledge of number types. If substantiated, this would undermine the epistemological motivation behind the $formalist_2$ reduction. That is to say, if numbers are abstract objects which can come to be known through interaction with certain concrete objects then there is no epistemological benefit in restricting the set of mathematical objects to expressions. Our explanation of how we can come to know, say, numbers is on a par with our explanation of how we can come to know expressions.

We, on the other hand, reject formalism$_2$ on the grounds that interaction with expression tokens cannot account for knowledge of expression types - we argue for this below. That is to say, we will provide a general argument for the claim that knowledge of types is not based on interaction with tokens. This makes Wetzel's claim (that if interaction with concrete objects yields knowledge of expression types then interaction with concrete objects yields knowledge of number types) vacuously true. It also nullifies the epistemic motivation for formalism$_2$. It doesn't, however, nullify epistemic motivations for all formalist construals of mathematics. Forsaking the presupposition that knowledge of abstract objects is based on interaction with concrete tokens, one might still argue that we only have epistemic access to a proper subset of abstract objects. Later in the chapter we address the question of whether we can defend formalism on such epistemological grounds - this will turn on whether an account of the intuition of expression types, but not of number types, can be provided.

Formalism$_2$ bases knowledge of abstract objects on interaction with concrete objects. We are familiar with the supposed epistemological advantage of such a construal; epistemic access to concrete objects is thought to be less problematic than access to abstract objects. We challenge formalism$_2$'s assumptions that knowledge of abstract objects is derived from interaction with concrete tokens and that we have epistemic access to concrete objects but not abstract objects.

Formalism$_2$ takes our experience with tokens to be basic, with knowledge of types derived. This epistemological claim is of the following sort: from interaction with tokens A, B, and C we can infer P about type Z. However, this depends on the presupposition that A, B, and C are tokens of Z; knowledge of, say, type Z is only derived from interaction with tokens of a particular type, namely tokens of Z. Thus the initial interaction with which the derivation of knowledge of, say, type Z begins with is identified as interaction with tokens of THAT type - of type Z. Therefore, one problem faced by formalism$_2$ is that it must provide an account of 'tokens of THAT type' which doesn't presuppose knowledge of the type for it is precisely knowledge of the type that they're trying to account for.

There is no obvious way to avoid this difficulty. One might propose the following: take any particular token and declare that that token and all similar tokens give us information about the relevant type, whatever that might be. In effect, the particular functions as a standard. This doesn't appear to require prior knowledge about the type. But this move isn't really very helpful because it requires an account of similarity. In order to identify any two tokens as similar, one must have a prior notion of what it is that the tokens share in common; this requires an explanation of the common element shared by the tokens or concrete objects.

The problem of explaining how it is that objects can resemble each other is very old: it was at least partly responsible for the development of Plato's theory of Forms. We often identify one object as resembling other objects. Plato explained this resemblance by saying that the resemblance is due to a shared property. However, once we admit to a shared property, then we face a new problem: the type of existence that this property has. Plato, of course, held that this shared property was a universal which existed independent of mind and matter.

The problem, for formalism$_2$, is to provide a theory which explains when two tokens are similar without the universal of resemblance. Quine attempts to reconstruct the notion of resemblance, in Word & Object, in terms of the capacities of the mind. If successful, this would be adquate for the needs of formalism$_2$. We want to argue, however, that Quine's attempt fails.

Quine held that language is acquired through conditioned responses: sentences are associated with each other and with external stimuli.[4] The appropriate model for Quine's theory of language acquisition is based on reinforcement theory. However, this conceptualization must be able to account for the fact that the same, or similar, stimuli can recur at different times. Thus, in order to explain language acquisition, Quine had to provide an account of when two stimuli are similar. Quine explicitly recognizes this when he says:

> There could be no induction, no habit formation, no condition, without prior dispositions on the subject's part to treat one stimulation as more nearly similar to a second than to a third.[5]

Quine concedes that there must be some mental structure which explains how, say, one can generalize from tokens. He says:

> If the child is to be amenable to such training, however, what he must have is a prior tendency to weigh qualitative differences unequally. He must, so to speak, sense more resemblance between some stimulations than between others. Otherwise a dozen reinforcements of his response 'Red', on occasions where red things were presented, would no more encourage the same response to a thirteenth red thing than to a blue one; and a dozen reinforcements of his response 'Mama', on occasions dominated by the mother's face at various angles, would be just as inconsequential. In effect therefore we must credit the child with a sort of prelinguistic quality space.[6]

The prelinguistic quality space, postulate by Quine, has a built-in distance measure. Quine maintains that we obtain information about an individual's distance measure through empirical observations. He says:

> If we reinforce his response of 'Red' in the presence of crimson and discourage it in the presence of yellow, and then find that he makes the response to pink and not to orange, we can infer that the shades of crimson and pink used are nearer each other, in his quality space, than the crimson and orange. Supplementary clues to spacing are available in the child's hesitation, or reaction time.[7]

The crucial assumption is that the inductions (which are essential to his account of learning the sentences of L) depend on similarities, which, in turn, are based on innate properties of the mind. According to Quine, the postulation of innate mechanisms is permissible provided that they are related to external observations. But what exactly are the restrictions on the innate mechanisms? How does one characterize the dimensions of quality space? In Word & Object it appeared that Quine confined himself to dimensions with physical correlations: his examples[8] included the dimensions of hue and brightness. He says:

> ...the denizens of what we have been calling the child's quality space are, we can as well say, the stimulations; what needs to have been peculiarly "within" the child is just the spacing of them.[9]

However, in Words & Objections, Quine asserts that although similarity is defined in terms of the distance measure of the quality space, the dimensions need not have physical correlates. He states this explicitly in his response to Chomsky in Words & Objections:

> In fact the denizens of the quality spaces are expressly stimulations...any and all, with no prior imposition of dimensions. Any irrelevant features of the stimulations will in principle disappear of themselves in the course of the experimental determination of the quality space.[10]

However, once there are no physical constraints imposed, it is natural to wonder whether we shouldn't postulate an even richer system of innate ideas to account for language acquisition. Chomsky suggests that once Quine admits innate mechanisms of the generality that he describes in his response to Chomsky in Words & Objections, there is no reason not to postulate a very rich system of innate structures (especially if it can provide a more adequate account of language acquisition). Chomsky says:

> If innate mechanisms of arbitrary complexity are permissible, so long as conjectures are eventually made sense of in terms of external observations, then there is no reason to assign any special place to dimensional structures such as "quality space," nor to structures determined by differential conditioning and extinction tests (as distinct, say, from recall or recognition tests).[11]

Chomsky also points out that a very strong version of a theory of innate ideas is compatible with Quine's framework:

> ...one could easily construct a theory of innate ideas of a rather classical sort in terms of a prelinguistic quality space with a built-in distance measure. Quine would, apparently, accept a very strong version of

> a theory of innate ideas as compatible with his framework. Thus he considers the possibility that "a red ball, a yellow ball, and a green ball are less distant from one another in...the child's...quality space than from a red kerchief". It is difficiult to see how this differs from the assumption that "ball" is an innate idea, if we admit the same possibilities along other 'dimensions' (particularly, if we allow these dimensions to be fairly abstract).[12]

Not only is a very rich system of innate structures compatible with Quine's theory, but there is good reason to postulate such a rich system of innate structures. Katz has argued extensively that it is impossible to account for language learning on the basis of inductive generalizations and associations.[13] For our purposes it is sufficient to look at two of Katz's objections to the reinforcement model of language acquisition. The empiricist premise behind this is that we acquire language through inductive generalizations which associate sentences with each other and with stimuli. Katz argues that this is an incorrect assumption. Katz points out that only the observable features of utterances can be associated with each other and with stimuli, on the empiricist model. Katz says:

> One fundamental assumption of any associative theory of learning is that what is learned can be broken down into elements which have been each associated with observable and distinguishable constituents of the input in the following sense. The elements of the input which can have something associated with them must be distinguishable in terms of the discriminative and analytic capacities of

> the perceptual mechanism that codes the input into discrete parts and analyzes those parts as units within one or another category... Therefore, if the associationist theory is to successfully explain the case of language learning, the physical speech sounds, or utterances, from which the child acquires his knowledge of the rules of the linguistic description must contain, or be analyzable into, observable and distinguishable elements such that for each constituent of the meaning of an utterance whose meaning has been acquired there is an observable and distinguishable component of its phonetic shape with which that semantic constituent can be associated.[14]

However, as we pointed out in Chapter Five, the Haskins laboratory findings indicate that the physical sounds do not provide a complete basis for identifying the phonetic components of language. Since physical sounds are the observable features of utterances, the empiricist account of language acquisition is compromised from the start. Furthermore, many semantic facts, such as synonymy, cannot be accounted for by the empiricist. The empiricist can only explicate different conceptual relations in terms of the strengths of associations. Katz says:

> However, such different conceptual relations as sameness of meaning, meaning inclusion, difference in meaning, incompatibility of meaning, and so on are wholly unexpressable in these terms. Given that two ideas are associated, each with a certain strength of association, we cannot decide whether one has the same meaning as the other, whether they are different in meaning, whether they are incompatible in meaning, etc. Strongly associated expressions like 'ham' and 'eggs'...are not for that reason strongly related in

> meaning, and weakly associated expressions like 'eye-doctor' and 'oculist'...can be equivalent in meaning. Similarly, no invariant feature of association separates synonyms from antonyms or pairs of sentences exhibiting a logical entailment from pairs exhibiting a causal relation. The fact that such conceptual relations are unrepresentable in terms of associative connections means that the empiricist hypothesis provides no way to explain how the speaker's acquisition of language equips him to determine such relations. What is required is a wide variety of relations. What is required is a wide variety of different kinds of connections, not one type of connection with possible variations in the degree of its strength.[15]

Katz's arguments point to the fact that Quine's assumption about the mechanism responsible for language acquisition (namely, that stimulus-response connections are adequate) is unwarranted. But if we cannot account for language learning on the basis of inductive generalizations and associations, then we need to postulate an even richer concept of innate structures than the empiricist envisions.

Quine originally atempted to reconstruct the notion of resemblance psychologically: he postulated an innate quality space with a built-in distance measure to account for similarity. The postulation was justified on the grounds that this assumption was required by the reinforcement model of language acquisition. Therefore, since the reinforcement model cannot account for language acquisition, Quine's attempt to reconstruct the notion of resemblance psychologically fails.

If successful, Quine's attempt to reconstruct the notion of resemblance, in terms of the capacities of the mind, might have been used to explain how two tokens are similar - without postulating resemblance as a universal. This could, in turn, be utilized by $formalism_2$ to account for the similarity of tokens. The reader will recall that the $formalist_2$ thesis (that the knowledge of types is derived from the knowledge of tokens) was handicapped by its inability to account for the similarity of tokens. Since Quine's reconstruction of resemblance fails, the epistemic program of $formalism_2$ lacks credibility until some other reconstruction of the notion of resemblance is provided.

Section Two

We have provided two arguments which undermine the epistemic program of $formalism_2$. We argued that any account which bases knowledge of types on interaction with tokens commits one to either (i) accepting the uneliminable role of at least one type (resemblance) in the account of how we know types or (ii) an epistemological "explanation" wherein the derivation of the knowledge of the type from tokens presupposes knowledge of the type.

At first sight, (i) doesn't appear devestating to the $formalist_2$ program. After all, it didn't claim that all types are known through interaction with tokens. However, the objection raised by (i) is not that our knowledge of some abstract objects cannot be derived solely from interaction with concrete objects but that our knowledge of no abstract objects can be derived solely from interaction with concrete objects. All such knowledge requires prior knowledge of the universal resemblance which itself cannot be derived from interaction with concrete objects: it must be an innate idea. Thus (i) undercuts the $formalist_2$ program. We have also demonstrated that (ii) is equally destructive of $formalism_2$: it presupposes knowledge of the very thing that it is trying to provide an epistmeological account of!

Our objections to $formalism_2$ exploited the fact that it is notoriously difficult to explain how perception of tokens can provide knowledge of truths of types. Up to this point our complaints centered on the problems of basing knowledge of types on interaction of tokens with that type. We emphasized the difficulties involved in beginning the epistemic route with tokens of a given type and ending with the type itself. Such explanations, we argued, are inherently circular.

The motivation behind the formulation of $formalism_2$ was epistemological. Knowledge of abstract objects is explained by citing causal interactions with tokens of the relevant types. We found that our inability to describe how knowledge of types is "yielded" by causal interaction with tokens has been a limiting feature of such attempts. Now we want to argue that $formalism_2$ implicitly accepts the empiricist assumption that the causal theory of knowledge is the appropriate model for all knowledge (acceptance of this assumption motivates the claim that knowledge of types is derived from knowledge of tokens, which are capable of causal interaction). We will argue that this assumption is incorrect.

The causal theory of knowledge has dominated recent epistemological theory. The basic condition that this theory places on accounts of knowledge is that X knows that Y only if X is in the right sort of relationship with the fact that Y. The relationship is characterized as causal. (It should be noted that the exact characterization of the causal relationship varies in subtle ways: see Skyrms and

Harman, for instance.[16] Our argument does not depend on a decision about this point.) It follows that a necessary condition for 'X knows Y' to be true, is that what is known (Y) causes X's knowledge. Such a requirement is necessary to avoid the Gettier problem.[17] I have construed the causal condition as giving (at the very least) a necessary condition for knowledge; this seems consistent with Goldman's use of it.[18] And, in fact, acceptance of the causal condition as a necessary condition for knowledge is supported by examples of the sort which occur in the famous statement of Gettier's problem. That is to say, the causal condition was provided to resolve the sorts of difficulties which Gettier points out. Consider Gettier's Case II:

> Let us suppose that Smith has strong evidence for the following proposition:
> (f) Jones owns a Ford.
> Smith's evidence might be that Jones has at all times in the past within Smith's memory owned a car, and always a Ford, and that Jones has just offered Smith a ride while driving a Ford. Let us imagine, now, that Smith has another friend, Brown, of whose whereabouts he is totally ignorant. Smith selects three place-names quite at random, and constructs the following three propositions:
> (g) Either Jones owns a Ford, or Brown is in Boston;
> (h) Either Jones owns a Ford, or Brown is in Barcelona;
> (i) Either Jones owns a Ford, or Brown is in Brest-Litovsk.[19]

Each of propositions (g), (h), and (i) is entailed by (f). Seeing that (f) entails (h), and having strong evidence for f, Smith concludes that h is true. Gettier then invites us to imagine that (1) Jones does not at present own a Ford, but is driving a rented car and (2) by coincidence (and unknown to Smith), Brown is in Barcelona. This means that h is true, that Smith believes h and, furthermore, that Smith has adequate evidence for h. However, Smith does not know h because what makes h true is the fact that Brown is in Barcelona but this fact has nothing, whatsoever, to do with Smith's belief of h. Goldman says:

> If Smith had come to believe h by reading a letter from Brown postmarked in Barcelona, then we might say that Smith knew h. Alternatively, if Jones did own a Ford, and his owning the Ford was manifested by his offer of a ride to Smith, and this in turn resulted in Smith's believing h, then we would say that Smith knew h. Thus, one thing that seems to be missing in this example is a causal connection between the fact that makes h true...and Smith's belief of h. The requirement of such a causal connection is what I wish to add to the traditional analysis.[20]

Thus, according to Goldman, if the relevant causal connection is absent, we would object to the assertion that, say, Smith knows that h is true. This is compared, by Goldman, to the fact that knowledge acquired by sight includes a causal requirement and that absence of this causal relationship is grounds for withholding the "assertion that so-and-so saw such-and-such."[21] He says:

> Suppose that, although a vase is directly in front of S, a laser photograph is interposed between it and S, thereby blocking it from S's view. The photograph, however, is one of a vase (a different vase), and when it is illuminated by light waves from a laser, it looks to S exactly like a real vase. When the photograph is illuminated, S forms the belief that there is a vase in front of him. Here we would deny that S sees that there is a vase in front of him, for his view of the real vase is completely blocked, so that it has no causal role in the formation of his belief.[22]

Goldman argues in this article, in agreement with Gettier, that in the case of empirical propositions, justified true belief is not sufficient to guarantee knowledge. Whether or not this holds for non-empirical propositions is another question. Goldman says that it does not:

> Since Edmund L. Gettier reminded us recently of a certain important inadequacy of the traditional analysis of "S knows that p," several attempts have been made to correct that analysis. In this paper I shall offer still another analysis (or a sketch of an analysis) of "S knows that p," one which will avert Gettier's problem. MY CONCERN WILL BE WITH KNOWLEDGE OF EMPIRICAL PROPOSITIONS ONLY, SINCE I THINK THAT THE TRADITIONAL ANALYSIS IS ADEQUATE FOR KNOWLEDGE OF NONEMPIRICAL TRUTHS.[23]

Why is the traditional analysis of knowledge of non-empirical propositions not vulnerable to Gettier's objections? The causal factor was necessary to distinguish between cases of perceptual experiences which are numerically distinct yet have the same qualities. The perceptual experience of S when he really is seeing the vase and the perceptual experience of S when he is really seeing a photograph, are indistinguishable in terms of S's sensations. What distinguishes these two cases is the cause of these stimulations (whether the vase or the photograph is causally related to S). However, this situation cannot arise when the objects of knowledge are restricted to abstract objects. Since abstract objects are not spatially-temporally located, it is impossible for abstract objects to have the same "perceptual" qualities while being numerically distinct. The Gettier problem simply can not arise in mathematics or linguistics.

Clearly, acceptance of the causal condition as a necessary condition for all types of knowledge requires a separate argument which establishes that the sorts of difficulties which necessitated the causal condition arise in all cases of knowledge. Such an argument has never, to our knowledge, been provided. Our small discussion (as to why the account of knowledge of non-empirical propositions is not vulnerable to Gettier's problem) suggests that such an argument would be difficult to construct. Yet until it is, there is no reason to believe that a theory of knowledge, as a whole, requires a causal condition. Hence, it has never been conclusively established that an adequate theory of kowledge must be essentially causal.

Nonetheless, Gettier's 1963 paper, "Knowledge Is Not

Justified True Belief" and Goldman's "A Causal Theory Of Knowledge" have had tremendous influence on modern epistemology. The strength of the causal theory of knowledge is seemingly evidenced in its ability to account for our knowledge about the physical world. Its weakness lies in its inability to account for our knowledge of abstract objects. Rather than conclude that this demonstrates that the causal theory of knowledge is not adequate to account for all kinds of knowledge, many philosophers have taken this to demonstrate that abstract objects do not exist. They argue that because there is no coherent causal account of our knowledge of abstract objects, such objects do not exist.

Undoubtedly the problem has been compounded by the platonist's failure to articulate an acceptable theory of knowledge. Consider, for instance, the standard Godelian reference:

> ...the objects of transfinite set theory...clearly do not belong to the physical world and even their indirect connection with physical objects is very loose... But, despite their remoteness from sense experience, we do have a perception also of the objects of set theory, as is seen in the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and

may be decided in the future.[24]

Many philosophers have considered Godel's account inadequate; some have gone so far as to call it incoherent. Godel himself contributed to the situation by not giving a full account of his notion of intuition. Clearly Godel held that intuition is "like" perception; the problem lies in his failure to specify, in this passage, what he meant by "like". Katz charges that:

> The tradition from Plato to Godel proposes a conception of intuition that extends the similarity between it and introspection and perception. The tradition conceives intuition on analogy to perception and makes intuitive knowledge depend on the knower establishing some form of direct contact with the objects of knowledge... The principal point at which my Platonism departs from traditional Platonism is in rejecting such perceptually inspired accounts of intuition. I think that the traditional claim that knowledge of abstract objects is knowledge by acquaintance cannot be reconciled with the nature of these objects. The difficulty is that perception demands a causal relation between perceiver and object perceived, so that, if intuition were a form of perceptual acquaintance, then abstract objects, being outside space and time and being objective, could not enter such a relation.[25]

Acknowledging the philosophical astuteness of Godel, Katz suggests that we not interpret Godel's passage literally. A literal interpretation fails because it depends on a causal connection between humans and abstract objects. He finds, however, that a non-literal interpretation fares no better:

> If we take the references to perception non-literally, we are then left with only a metaphor: we have a vivid but misleading expression of a similarity between intuition and perception. We know no more about intuitive apprehension than we do about what it is to see that one thing logically follows from another on the basis of the metaphorical extension of the visual sense of "see". The metaphorical construal of such references fails to clarify apprehension. The Platonist tradition thus presents us with a choice between two equally unattractive alternatives: an incoherent account of intuition, or none at all.[26]

Katz discusses two alternative interpretations of Godel's passage. He objects that if we interpret "like" literally, i.e., involving causal relations, then since abstract objects cannot enter into causal relations, there can be no knowledge of abstract objects. Yet to interpret "like" non-literally is to leave us with nothing more than a metaphor, providing no account of intuition. Katz's objections are representative of the general dissatisfaction with Godel's account of intuition.

Nonetheless, I think that we can avoid the dilemma posed by Katz by providing a literal interpretation of Godel's references to perception that is both supported by textual analysis and immune to Katz's objections.

Later in the same article, Godel says:

> That something beside the sensations

> actually is immediately given follows...from the fact that our ideas referring to physical objects contain constituents qualitatively different from sensations or mere combinations of sensations, e.g., the idea of object itself, whereas, on the other hand, by our thinking we cannot create any qualitatively new elements, but only reproduce and combine those that are given. Evidently the "given" underlying mathematics is closely related to the abstract elements contained in our empirical ideas.[27]

Godel argues that the question of the objective existence of the objects of mathematical intuition is "an exact replica of the question of the objective existence of the external world".

This, taken with the passage cited earlier, suggests that if we want to understand intuition we should look to perception because there is a component to perceptual knowledge which is innate. Godel claims that our perception of physical objects includes an abstract element (we can see, here, the influence of Husserl[28]). His point is that the stimulation of sensory-perceptors is not equatable with perception; perception involves the concept of an object which could not be derived from sensations.

On this interpretation, Godel is depicted as holding that intuition is "like" perception in that both have innate constituents. Godel is not depicted as maintaining that intuition is a form of perceptual acquaintance. This suggests that since Godel's account of intuition is not a form of perceptual acquaintance, it need not be dismissed;

his account of intuition, however, still requires explication. In a funny twist, it is Katz himself who provides a more complete account of intuition which enhances, rather than undermines, Godel's account.

We can also point out that there is nothing in Godel's account of intuition which suggests that we can give an account of the intuition of expressions but not the intuition of numbers - and this is what is required to restore the epistemological motivation for a formalist construal of mathematics.

Now, to help us understand intuition better, let's follow Godel's advice and focus on the fact that knowledge of physical objects has constituents that couldn't arise merely from sensory experience. Stimulation of the sensory receptors provides the raw data but the perceiver brings to the perceptual act the notion of objecthood which includes the concept of a concrete object existing independent of perception. Pointing out that the idea of object itslf cannot be dervied from mere sensation, Godel viewed it as an innate notion.

To help us see this, we recall from our discussion of causal knowledge the example of a subject, S, and his perceptual experience when he is really seeing a vase and his perceptual experience when he is really seeing a laser photograph of the vase. The two experiences are indistinguishable in terms of the stimulation of S's sensory receptors - he has identical sensations. This example also illustrates Godel's point that the idea of objecthood cannot be based soley on sensory experience. Sensory experiences, which can mislead the perceiver, can be corrected by the perceiver's innate notion that an object exists independent of sensory stimulations - which causes the sensations. It is this notion of an independent, concrete object, which initiates the sensory stimulation of the perceiver, that accomodates the idea that two perceptions can be identical in terms of sensory experience but numerically distinct. This idea of a concrete object, independent of the sensory experience, cannot be attained through sensations. It must be innate.

Bearing in mind this discussion of perceptual knowledge, we can turn to Katz's own account of intuition. Katz develops, in Language & Other Abstract Objects (Chapter VI), an account of intuition that is modelled on the Kantian conception of pure intuition. The objects of intuition are produced by constructions. Katz explains:

> In my theory of intuition, construction produces objects of intuitive apprehension but these objects are internal representations of abstract objects rather than the abstract objects themselves...an internal representation constructed in intuition corresponds to an abstract object outside us or it does not correspond. If it corresponds, the intuitive judgement is true, if not, false.[29]

Katz's account of intuition accomodates both mathematical and linguistic intuitions. For instance, Katz describes grammatical intuition as follows:

> The source of <a grammatical> intuition is a psychological process in which the concept of an object is constructed in concreto, but the import of the intuition is an objective fact about the object internally represented. The construction is based on innate universal conditions for such constructions of three principle kinds, competence-rules expressing information about the grammatical structure of a language, a knowledge of the relation 'x knows y', and knowledge of the characteristics of the category abstract object...[30]

Katz urges that we extend this notion of intuition to include mathematical intuition. He says:

> ...we can substitute mathematical competence (i.e., the ideal calculator's knowledge of the natural numbers)...for the grammatical competence in the relation 'x is knowledge of y', so that these systems of tacit knowledge, rather than tacit grammatical rules, provide the values of 'x'. The structure of the relation (i.e., the constraints that 'x is knowledge of y' imposes on its arguments) and the innate notion of an abstract object that provides the values of 'y' are common to all relevant competences. Thus, it seems reasonable to suppose a single faculty of intuition that operates on different competences to produce distinct sets of particular intuitive apprehensions about different sets of abstract objects.[31]

As an aside, we notice that since Katz's construal of intuition accomodates mathematical intuition as well as linguistic intuition, it, too, removes epistemological justification for a formalist interpretation of mathematics.

Now, we can see, from these passages, that Katz holds that our knowledge of linguistic objects has constituents that couldn't arise merely from the way that we mentally represent linguistic facts. The distinction between knowledge and the object of knowledge compensates for the fact that mental representations can misrepresent grammatical facts. The other component of intuition is the concept of an abstract object which further shapes the characteristics of the object of knowledge. Katz says:

> Using both the 'knowledge-of' relation and the idea of an abstract object, the faculty of intuition can operate on principles reflecting the form tacit grammatical rules take in humans and depsychologizes them, reconstructing representations of sentences as concrete concepts of abstract objects. These two further components seem sufficient, since they can rectify the respects in which a speaker's tacit rules misdescribe facts about the language and construct concepts of abstract objects that properly describe sentence structure.[32]

Katz claims that there are two components of intuition which are innate: the distinction between knowledge and the object of knowledge and the concept of abstract objecthood. We can also see these components in perceptual knowledge: there is a distinction between knowledge and the object of knowledge and there is a notion of concrete objecthood. Just as Godel argues that our knowledge of the physical world has constituents that couldn't arise merely from sensory experiences, Katz argues that our knowledge of linguistic objects has components that couldn't arise merely from mental representations.

These arguments should convince the reader that Katz's account of intuition is consistent with Godel's account. We have also argued that Katz's explication of the innate constituents of intuition can be viewed as an extension of Godel's explication of the innate components of intuition. In a sense, Katz continued where Godel left off. In addition, the similarity of the innate components of intuition and perceptual knowledge attests to the validity of Godel's suggestion that intuition is "like" perception.

Our discussion about perception and intuition further supports positions held by both Godel and Katz: that our account of intuition is no more problematic than our account of perception. Both intuition and perception require innate components which cannot be accounted for by either mental representations or sensory stimulations. And there is no evidence, on either Godel's or Katz's accounts, which supports a distinction between the epistemic accessibility of numbers and expressions.

Section Three

CONCLUDING REMARKS

Our objective was to determine whether a formalist interpretation of classical mathematics is tenable from an ontological viewpoint. Since formalists maintain that the terms and quantifiers in mathematics refer to expressions, we needed to determine whether formalism is consistent with the ontologies of mathematics and linguistics. We argued that mathematical theory and linguistic theory are ontologically compatible - both disciplines require abstract objects.

In Chapter Three we were concerned with whether mathematical objects are concrete objects. We made the following observations:

(i) Field defends nominalism on the grounds that abstract objects are not needed to make inferences about the physical world or to "do science". Interpreted as a general defense of nominalism, his argument fails because it is directed against a platonism au Quine. Other platonists would contend, for starters, that Field presupposes that "doing science" is coextensive with "doing physics". Nor is Field's argument any more successful when viewed as an internal criticism of Quine's acceptance of abstract entities. We showed that if Field accepts the Quinean framework, then he must also accept Quine's holistic view of the world. But then he is unable to distinguish between mathematical theory and the set of nominalistically-statable premises and conclusions - an essential move in his argument. We concluded that Field failed to establish his nominalistic position.

(ii) Any nominalist construal of mathematics, on the lines of Goodman, presupposes that the physical world is sufficient for representation of the numbers. Since the structure of the world cannot guarantee infinitely many individuals, a nominalist interpretation of mathematics, au Goodman, fails.

(iii) We called Hilbert a nominalist because he held that the objects of mathematics were expressions which were, on his account, concrete objects. We argued that Hilbert's construal of expressions as concrete objects was incompatible with the need of mathematics for infinitely many objects. Hibert's postulation of a distinction between potential and actual infinities seems, at first blush, to meet this objection. However, in point of fact, it defuses this objection only if "potential infinity" is adequately characterized. Textual analysis suggested that Hilbert conceived of mathematical objects as construed in the mind au Kant. "Potential infinity" captures the idea that regardless of the number of steps which have been performed in such a construction, it is possible to add yet one more.

We argued that this idea makes assumptions about the mind which cannot be supported by Kantian theory. We concluded that Hilbert's notion of potential infinity could not be explicated in terms of Kant's notions. It is the inability to articulate the concept of potential infinity which undermines Hilbert's thesis that mathematical objects are expressions which are, in turn, both concrete and infinite in number.

We then asked, in Chapter Four, whether a psychological interpretation of mathematics was tenable. We maintained that Frege's criticisms of conceptualism were still sound. He argued that psychologism deprives mathematical truths of objectivity: in holding that mathematical entities are mental entities (which are subjective), psychologism denies objectivity to mathematical truths.

Dummett argues that intuitionism, the most well-developed conceptualist theory of mathematics, is a special case which escapes these charges of subjectivity. We countered that while, on Dummett's account, mathematical truths aren't subjective in the sense that they are indexed to particular individuals, they are, nonetheless, subjective because they are implicitly indexed to the principles of mental operations.

Having argued that mathematical objects are neither concrete nor psychological objects, we turned, in Chapter Five, to the ontological status of expressions. We argued, here, that Chomsky's rejection of nominalism as an inadequate conceptualization of linguistics is still definitive. Taxonomic theories could not account for the creative aspect of language. Nor could the nominalist conception of grammar account for all fo the grammatical properties and relations because it is insufficiently abstract. Linguistic theories require grammatical concepts which are not reducible to acoustic signals.

Katz has argued that conceptualism, in turn, is also not abstract enough to account for all of the grammatical properties and relations because it replaces nominalist constraints with psychological constraints. For instance, conceptualism cannot account for the fact that nondenumerably many sentences are grammatical. Nor can conceptualism account for all of the grammatical properties and relations: analyticity is one property that cannot be accounted for by the conceptualist. We concluded that only platonism can provide an adequate ontology for linguistics.

Determining that the objects of both linguistics and mathematics are abstract eliminates some objections to a formalist interpretation of mathematics. However, we initially motivated the formalist conception of mathematics on epistemological grounds. This particular motivation seemed to be rendered ineffective by the finding that expressions are abstract objects, rather than concrete objects. However, the distinction between abstract objects which are epistemologically accessible and abstract objects which are not epistemologically accessible (because only the former have concrete tokens) suggested a means of restoring the epistemological motivation. If it could be shown that expressions are epistemologically accessible while numbers, say, are not, then the epistemological motivation would be restored. We argued, however, that this distinction assumes that knowledge of abstract objects is based on interaction with concrete objects and that we have epistemic access to concrete objects but not abstract objects. We maintained that there is no way to account for how a particular token can have the generality required for it to take the place of a universal. We argued that it is not sufficient to depict the token as functioning as a standard because there is no way to reconstruct the notion of similarity, or resemblance. Hence the proposal fails because it depends on the assumption that knowledge of abstract objects is based on interaction with concrete objects.

We also argued that this assumption reflected a commitment to the causal theory of knowledge. We claimed that there is no good reason to hold that a theory of knowledge must be essentially causal. Recognizing that much of the attraction of the causal theory of knowledge stems from the poor characterization of intuition, we reexamined Godel's account of intuition, undermining some of the objections to his account. We also looked at Jerry Katz's more recent account of intuition. He portrays his account of intuition to be in opposition to Godel's account. We argued that this is, in part, based on a misinterpretation of Godel's remarks about intuition. We defended the view that Katz's account of intuition is both compatible with, and represents an extension of, the views of Godel.

We also argued that perceptual knowledge is "no better off" than intuition. Hence there is no reason to assume that we have epistemic access to concrete objects but not abstract objects. Nor could we find any justification for the view that some abstract objects might be more epistemically accessible than other abstract objects. We concluded that since both numbers and expressions are abstract objects and since both are equally epistemologically accessible, there is no epistemological basis for a formalist construal of mathematics.

It follows that this dissertation, in undermining one objection to a formalist construal of mathematics by providing an ontological characterization of linguistic and mathematical objects as abstract objects, also undermined the epistemological motivation for the formalist position. Whether formalism can be defended on other grounds is still another question.

NOTES

1 Wetzel 1983.

2 Wetzel 1983: 163.

3 Wetzel 1983: 164-165.

4 Quine 1960: 11.

5 Quine 1975: 306.

6 Quine 1960: 83.

7 Quine 1960: 83.

8 See, for example, Quine 1960: 83-85.

9 Quine 1960: 84.

10 Quine 1975: 306-307.

11 Chomsky 1975a: 199-200.

12 Chomsky 1975b: 54-55.

13 See, for instance, Katz 1966: 240-282.

14 Katz 1966: 249-250.

15 Katz 1966: 266-267.

16 Skyrms 1967.

17 Gettier 1966.

18 Goldman 1967.

19 Gettier 1966: 154.

20 Goldman 1967: 358.

21 Goldman 1967: 359.

22 Goldman 1967: 359.

23 Goldman 1967: 357. Emphasis added.

24 Godel 1947: 484.

25 Katz 1981: 200.

26 Katz 1981: 202.

27 Godel 1947: 484.

28 The idea that perception involves an abstract element was central to Husserl's philosophy; see, for instance, Husserl 1962.

29 Katz 1981: 203.

30 Katz 1981: 212.

31 Katz 1981: 213.

32 Katz 1981: 205.

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